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THE

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# MEDICAL REVIEW

OR

## QUARTERLY JOURNAL

OF

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**JOHN FORBES M.D. F.R.S.**

AND

**JOHN CONOLLY M.D.**

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# CONTENTS OF No. XI.

OF THE

# British and Foreign Medical Review.

JULY, 1838.

## Part First.—Analytical and Critical Reviews.

PAGE

Art. I.—Transactions of the Provincial Medical and Surgical Association. (Statistical and Topographical Papers.) . . . . . 1

Art. II.—Mémoires de la Société Médicale d'Observation de Paris . . 28
Memoirs of the Medical Society of Observation, of Paris
1. Recherches sur l'Emphyseme des Poumons. Par M. Louis. (Researches on Emphysema of the Lungs. By M. Louis.) . . . ib.
2. Recherches sur le Cœur et le Système artériel chez l'Homme. Par J. Bizot, de Genève, M.D. &c. (Researches on the Heart and the Arterial System in Man. By J. Bizot, M.D.) . . . . . . 41
3. Essai sur quelques Points de l'Histoire de la Cataracts. Par Th. Maunoir. (Essay on certain Points in the History of Cataract. By M. Maunoir.) . 50
4. Mémoire analytique sur l'Orchite blenorrhagique. Par M. Marc d'Espine. (Analytical Essay on blennorrhagic Orchitis. By M. M. d'Espine.) . 59

Art. III.—Guy's Hospital Reports. No. V. October, 1837. Edited by G. H. Barlow, M.A. and L.M., and J. P. Babington, M.A. &c. . . 60
1. Observations on the Ganglionic Enlargement of the Pneumo-gastric Nerve; the probable Function of that Ganglion; and the Position which it occupies in the Human Subject, and in several of the lower Animals. By Mr. Cock . ib.
2. On the Distribution and probable Function of the Laryngeal Nerves. By Mr. John Hilton . . . . . . . 61
3. Observations and Experiments on the Lungs of New-born Children, in relation to Medical Jurisprudence. By Mr. Alfred Taylor . . 62
4. Cases by Mr. Bransby Cooper . . . . . . 66
5. On the Diagnosis of Organic Diseases of the Uterus. By Dr. Ashwell . 71
6. Observations on Abdominal Tumours and Intumescence. By Dr. Bright . 72
7. On the Influence of Electricity as a Remedy in certain Convulsive and Spasmodic Diseases. By Dr. Addison . . . . . ib.
8. Cases of Disease in the Fœtus. By Mr. T. W. King . . . 74
9. Description of the Sacculus or Pouch in the Human Larynx. By Mr. Hilton ib.
10. Ophthalmic Cases treated by Mr. Morgan . . . . ib.
11. Experiments and Observations on Albuminous Fluids. By Dr. Babington . ib.

Art. IV.—Ueber Virilescenz und Rejuvenescenz thierischer Körper. Ein Beitrag zur Lehre von der regelwidrigen Metamorphose organischen Körper. Von Dr. Carl Wilhelm Mehliss, praktischen Artze, &c. in Liebenwerda . 75
On the Virilescence and Rejuvenescence of Animals: a Contribution to the Knowledge of the regular Metamorphoses of Organic Bodies. By Dr. Charles William Mehliss, of Lebenswerda

Art. V.—1. The Cyclopædia of Practical Surgery.—Art. Abortion, by Dr. Ryan 81
2. The Cyclopædia of Practical Medicine and Surgery.—Article Abortion, by Dr. Dewees . . . . . . ib.
3. A Dictionary of Practical Medicine, &c.—Art. Abortion, by Dr. Copland . ib.
4. The Cyclopædia of Practical Medicine.—Art. Abortion, by Dr. Lee . ib.
5. Dictionnaire de Médecine et de Chirurgie pratiques. (Dictionary of Practical Medicine and Surgery.)—Art. Avortement, by Prof. Dugès . . ib.
6. Encyclopädisches Wörterbuch der medicinischen Wissenschaften. (Encyclopædic Dictionary of Medical Science.)—Art. Abortus, by Dr. von Siebold ib.

PAGE

Art. VI.—Outlines of the principal Diseases of Females; chiefly for the Use of Students. By Fleetwood Churchill, m.d. . . . 86

Art. VII.—Medicine and Surgery one Inductive Science; being an Attempt to improve its Study and Practice on a Plan in close Alliance with Inductive Philosophy, and offering, as first-fruits, the Law of Inflammation; addressed particularly to the Medical Student and the Profession, but easy and intelligible to the Public also: the whole being the Introduction and first Part of a System of Surgery. By George Macilwain, Fellow of the Royal Med. and Chir. Society, &c. . . . . . . . 98

Art. VIII.—De la Morve et du Farcin chez l'Homme. Par P. Rayer, m.d., Médecin de l'Hôpital de la Charité, &c. . . . . . 113

On Glanders and Farcy in Man. By P. Rayer, m.d. &c.

Art. IX.—Report of the Proceedings under a Brieve of Idiotcy, (Peter Duncan against David Yoolow,) tried at Coupar-Angus, 28-30 January, 1837; with an Appendix of relative Documents, and Introduction. By Ludovic Colquhoun, Esq., Advocate . . . . . . 122

Art. X.—1. Traité de Diagnostic et de Séméiologie. Par P. A. Piorry, m.d. . 128

Treatise on Diagnosis and Semeiology. By P. A. Piorry, m.d.

2. Grundriss der Speciellen Semiotik. Nach den Quellen bearbeitet, von Dr. H. E. Suckow, Kreisphysicus in Jauer . . . . . 147

Outlines of Special Semeiology. By Dr. H. E. Suckow.

Art. XI.—Inaugural Dissertation on the Influence of Climate on the Health and Mortality of the different Regions of the Globe. By A. S. Thomson . 148

Art. XII.—Institutes of Surgery: arranged in the Order of the Lectures delivered in the University of Edinburgh. By Sir Charles Bell, k.g.h. f.r.s. l. & e. 154

Art. XIII.—Experiments and Observations on the Gastric Juice and the Physiology of Digestion. By Wm. Beaumont, m.d., Surgeon in the United States' Army. Reprinted, with Notes, by Andrew Combe, m.d. &c. . 172

Art. XIV.—Du Lait, et en particulier de celui des Nourrices, considéré sous le rapport de ses bonnes et de ses mauvaises Qualités nutritives, et de ses Altérations. Par le Dr. Al. Donné . . . . . 181

Considerations on Milk, and particularly that of Nurses, more especially in regard to its good or bad Nutritive Qualities, and the Changes to which it is subject. By Dr. Al. Donné.

Art. XV.—Practical Observations on the Preservation of Health and the Prevention of Diseases: comprising the Author's Experience on the Disorders of Childhood and Old Age, on Scrofula, and on the Efficacy of Cathartic Medicines. By Sir Anthony Carlisle, f.r.s. &c. . . . . 186

## Part Second.—Bibliographical Notices.

Art. I.—The Hunterian Oration, delivered in the Theatre of the Royal College of Surgeons in London, February 14, 1838. By Benjamin Travers, f.r.s. 191

Art. II.—Velpeau's Anatomy of Regions. Translated from the French. By Henry Hancock, Esq. . . . . . . 193

Art. III.—Medical Portrait Gallery. Biographical Memoirs of the most celebrated Physicians, Surgeons, &c. who have contributed to the advancement of Medical Science. By Thomas Joseph Pettigrew, f.r.s . . 194

Art. IV.—First Lines of Physiology. By Daniel Oliver, m.d. . . 195

Art. V.—On the Improvement of Medicine. By Theophilus Thompson, m.d. . 196

Art. VI.—Ueber die in der Belgischen Armee herrschende Augenkrankheit: als Commentar zu Professor Dr. J. C. Juengken's Schrift über denselben Gegenstand. Von Dr. Burkard Eble, k. k. Regimenstfeldarzte, &c. . 197

On the Ophthalmia in the Belgian Army: a Commentary on Professor J. C. Juengken's Memoir on the same subject. By Dr. Burkard Eble.

Art. VII.—A new and familiar Treatise on the Structure of the Ear, and on Deafness. By A. W. Webster . . . . . 199

Art. VIII.—De Nervo Faciali. Auctore Doctore Massalien . . 200

Dissertation on the Facial Gland. By Dr. Massalien.

PAGE
ART. IX.—A Dissertation on the Causes and Effects of Disease, considered in reference to the Moral Constitution of Man. By HENRY C. BARLOW, M.D. . 201
ART. X.—Die vergleichende Osteologie des Schläfenbeins. Zur vereinfachung der herrschenden Ansichten bearbeitet. Von ED. HALLMANN . . ib.
The Comparative Anatomy of the Temporal Bone, &c. By ED. HALLMANN.
ART. XI.—1. Memoranda on difficult Subjects in Anatomy and Surgery; being a Pocket Companion for Students preparing for the College of Surgeons. By ROBERT DRUITT, M.R.C.S.L. . . . . . . 202
2. The Student's Companion to Apothecaries' Hall, &c. By E. OLIVER, M.R.C.S. ib.
3. Ophthalmic Memoranda, &c. By JOHN FOOTE, F.R.C.S. . . . ib.
4. Libamenta Praxeos Medicæ. By D. SPILLAN, M.D. . . . ib.
5. A Collection of Medical Formulæ. By D. SPILLAN, M.D. . . ib.
ART. XII.—A Series of Anatomical Sketches and Diagrams, with Descriptions and References. By THOMAS WORMALD, Assistant-Surgeon, &c. . . ib.
ART. XIII.—A Treatise on Hernia: comprising the Surgical Anatomy, Operative Surgery, and Treatment. By M. W. HILLES . . . 203
ART. XIV.—Handbuch der Menschlichen Anatomie. Von C. F. T. KRAUSE, M.D. 204
Manual of Human Anatomy, &c. By Dr. KRAUSE.
ART. XV.—Neurologische Beobachtungen. Von Dr. F. H. BIDDER . . ib.
Neurological Researches. By Dr. F. H. BIDDER.

---

## PART THIRD.—Selections from Foreign Journals.

### ANATOMY AND PHYSIOLOGY.

Microscopical Observations on the Anatomy of the Aqueous Capsule and System of the Crystalline Lens. By Dr. WERNECK, of Salzburg . . 205
On Reflex Motions. By Dr. A. W. VOLKMANN . . . . 211
Anatomical Observations and Remarks. By Dr. KRAUSE . . . 218
On the Relative Motions of the Globules of the Blood and Lymph in the Blood-Vessels of Frogs. By T. M. ASCHERSON . . . . 219
Absence of the Right Lung (Cyanosis) . . . . 220

### PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

On the Atmospheric and Epidemic Constitution of Berlin in September, 1837 . 220
Case of slow Poisoning by Oxide of Zinc. By Dr. BUSSE . . . 221
On the Juice of the Root of the Elder-tree in Dropsies . . . ib.
Researches on spontaneous Perforations of the Stomach. By M. LEFEVRE . ib.
On the Use of the Subcarbonate of Iron (Ferri Sesquioxydum,) in Hooping-cough. By Dr. STEYMANN . . . . . . 222
On the Nature and Treatment of Syphilis in Lithuania . . . 223
On large Doses of Tartar Emetic in Articular Synovial Effusions. By Dr. GIMELLE 224
On the Use of Strychnine. By M. BALLY . . . . 225
Neuralgia of the Head, produced by a Tumour in the Brain. By Prof. B. HOLST 226
On an Epidemic Phthisis Pulmonalis. By M. MENARD . . . 227
On the Occurrence of several Spontaneous Fractures of the Ribs in a Scorbutic Individual. By Dr. GOEDECHEN . . . . . . ib.

### SURGERY.

Fatally terminating Case of Phlebitis, after Venesection. By Dr. SPÖRER . 228
Luxation of the Scapular Extremity of the Clavicle, downwards. By Dr. TOURNEL 229
On the Excision of Ulcers, &c. By M. BONNET . . . . 230
New Method of reducing Dislocations of the Os Humeri. By M. MALGAIGNE . 231
On a new Method of distinguishing Cataract. By M. SANSON . . ib.
On the Development of Adhesions of Serous Membranes, &c. By M. BELMAS . 232
Reduction of an old Luxation of the Elbow. By M. MALGAIGNE . . 233
Ligature of the Carotid Artery of a Child aged Fifteen Months. By Dr. ZEIS . ib.
Abscess beneath the Pectoralis Major, communicating with the Chest. By M. BOINET 234
Successful Application of Creosote in an old Ulcer of the Vagina . . 235

### MIDWIFERY.

Report of two Cases of Pregnancy requiring Incision of the Os Uteri to allow Delivery. By Drs. BURDACH and GRUHN . . . . 235
Singular Case of Abortion . . . . . . . 236
Inversion of the Uterus resembling Polypus. By M. FLEURY, jun. . . ib.
Remarkable Case of Embryotomy on account of Exostosis of the Pelvis. By Dr. KYLL 237

### MEDICAL STATISTICS.

Statistics of the Lunatic Asylum at Aversa, (Naples.) By FILIPPO VOLPICELLA . 238
Statistics of Insanity in Westphalia and Saxony . . . . 240
Statistical Notice of the Cholera-Epidemic in Berlin, in 1837. By Dr. CASPER . ib.
Statistics of Suicide in London. By M. MOREAU DE JONNÈS . . . 241

### FORENSIC MEDICINE.

Murder by Carbonic Acid. By M. DEVERGIE . . . . 241
On the Absorption of Sulphuric Acid, when introduced into the Stomach. By A. BOUCHARDAT and the late M. CÆSAR COURIARD . . . 244

### MATERIA MEDICA.

New Preparation of Ipecacuanha . . . . . 247

### ANIMAL CHEMISTRY.

Microscopical Researches on the Composition of the Vaccine Fluid. By M. DUBOIS 247
Comparative Analysis of Healthy Bile and that containing Calculi. By P. MURATORI ib.
Analysis of Liquor Amnii at two different Periods of Pregnancy. By Dr. C. VOGT 248
New Analysis of the Blood. By M. BEUDANT . . . . ib.
On the Presence of Urea in Dropsical Effusions. By R. MARCHAND . . 249

---

## Selections from American and Colonial Journals.

### PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

Extraordinary Case of Electrical Excitement. By Dr. WILLARD HOSFORD . 249
On Purulent Discharges from the Bladder and Rectum. By Dr. J. MOUAT . 251
Case of Extra-uterine Pregnancy. By ALLAN WEBB, Esq. . . . 252
On the Cure of Stammering by Moral Means. By Dr. WARREN . . 253

### SURGERY.

A new Treatment in a Case of Anchylosis. By Dr. BARTON . . 254
Treatment of Varicose Veins in the Pennsylvania Hospital . . . 257
A Statistical Account of Fractures, treated in the Pennsylvania Hospital . 258
Elm-bark Surgery. By Dr. W. A. M'DOWALL . . . . 259
ADDENDUM . . . . . . . 260

---

## PART FOURTH.—Selections from British Journals.

### PHYSIOLOGY.

Case of Paraplegia. By WM ELLIOTT, M.D. . . . . 261

### PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

Cases of Acute Inflammation confined to the Epiglottis. By H. MARSH, M.D. . 261
On Tapping the Head in Hydrocephalus. By T. J. CONQUEST, M.D. . 265
Case of Empyema. By T. OGIER WARD, M.D. . . . . 266
Case of Dropsy of the Pineal Gland. By S. S. STANLEY, Esq. . . 268
Account of two Cases of Bronchial Polypus: with Remarks. By JOHN NORTH, F.L.S. ib.
Two Cases of Hydrophobia: effect of Morphium. By JOHN BURNE, M.D. . ib.

### SURGERY.

On Creosote, as a local Application. By GEORGE FIFE, M.D. . . 270
Case of Scalded Glottis: Laryngotomy. By F. E. HICKS, Esq. . . ib.
On some Effects resulting from Wounds of Nerves. By JOHN HAMILTON, L.R.C.S. 271
Case of Talipes Varus Verus, affecting both Limbs . . . 272
Remarks on Lenticular Glaucoma. By WM. MACKENZIE, M.D. . . 274
A Mode of relieving Enlargement of the Veins of the Testicle. By Mr. WORMALD ib.
Treatment of Intussusception by Inflation. By S. MITCHELL, Esq. . . ib.

---

## PART FIFTH.—Medical Intelligence.

On the Present State of Medicine and Medical Education in Holland. By C. J. NIEUWENHUIJS, M.D. . . . . . . . 275
Sketch of the Present State of Medicine in Portugal . . . 284
The Provincial Medical and Surgical Association . . . . 287
OBITUARY—THOMAS BLIZARD, Esq., Professor FERGUS, Dr. EDW. HARRISON, RASORI, BARON DESGENETTES, and Dr. DECKMANN . . 288–290
BOOKS RECEIVED FOR REVIEW . . . . . 291

# CONTENTS OF No. XII.

OF THE

# British and Foreign Medical Review.

SEPTEMBER, 1838.

---

## Part First.—Analytical and Critical Reviews.

PAGE

Art. I.—1. Influence de l'Anatomie Pathologique sur la Médecine, depuis Morgagni jusqu a nos Jours. Par Risueno D'Amador . . . 293

Influence of Pathological Anatomy upon Medicine, from the time of Morgagni to the present day. By Professor Risueno D'Amador.

2. Mémoire en Réponse à cette Question: "Quelle a été l'Influence de l'Anatomie Pathologique sur la Médecine, depuis Morgagni jusqu'à nos Jours?" Par Constant Saucerotte, m.d. . . . . . . ib.

Memoir in Reply to the Question: "What has been the Influence of Pathological Anatomy upon Medicine, from the time of Morgagni to the present day?" By Constant Saucerotte, m.d.

Art. II.—1. Remarks on the Treatment of Fractures of the lower Extremities without the aid of Splints. By J. F. Burke, m.r.c.s. . . 308

2. A Practical Treatise on Fractures. By Edward F. Lonsdale, Surgeon . ib.

Art. III.—1. Commentatio de Delirio Tremente. Auct. Dr. Höegh-Guldberg 320

An Essay on Delirium Tremens. By O..C. Höegh-Guldberg, m.d.

2. Prize Dissertation (for 1831) on Delirium Tremens. By J. C. Cross, m.d. . ib.

Art. IV.—An Experimental Enquiry into the Physiology of Cutaneous Absorption, and its Application to Therapeutics, &c. By W. H. Madden, m.d. . 332

Art. V.—1. Final Report of the Committee of the Philadelphia Medical Society, on the Construction of Instruments, and their Mode of Action, in the radical Cure of Hernia, &c. By Heber Chase, m.d. . . . 341

2. De la Cure Radicale des Hernies. Par M. le Docteur Belmas . . ib.

3. Uber radicalen Heilung reponibler Brüche. Von Dr. Ph. Finck . . ib.

Art. VI.—Nouvelles Recherches sur le Rheumatisme articulaire aigu en général, &c. Par Professeur J. Bouillaud . . . . 352

New Researches on acute and articular Rheumatism, &c. By J. Bouillaud.

2. Leçons de Clinique Médicale faites à l'Hôtel-Dieu de Paris. Par le Professeur A.F. Chomel. (Rheumatisme et Goutte.) . . . ib.

Lectures on Clinical Medicine, delivered at the Hôtel-Dieu, Paris. By Prof. A. F. Chomel. (Rheumatism and Gout.)

PAGE

ART. VII.—The Transactions of the Provincial Medical and Surgical Association. Vol. VI. . . . . . . . 380

Retrospective Address. By Dr. BARDSLEY . . . . ib.

Plan for the Reports of Infirmaries and Dispensaries. By Dr. COWAN . 381

On the Medical Topography of Exeter. By Dr. SHAPTER . . 382

On the Medical Topography and Statistics of Cheltenham. By Mr. NASH . 384

Cursory Examination of the Works of Galen. By Dr. KIDD . . 385

On the Treatment of Hypertrophy of the Heart, &c. By Mr. SALTER . 387

Two Cases of Gangrene of the Lungs. By Dr. ENGLAND . . 390

Case of partial Ectopia Cordis and Umbilical Hernia. By Dr. O'BRYEN . 391

Extirpation of the Eye. By Mr. MIDDLEMORE . . . ib.

Reports of Out-Patients treated in the Birmingham-Town Infirmary. By Mr. T. RYLAND and Mr. S. BERRY . . . . ib.

Report of Cases treated at the Birmingham Dispensary. By Dr. O. WARD . ib.

Report of Cases at the Birmingham Eye Infirmary. By Mr. MIDDLEMORE . 392

Account of the In-Patients in the Geneva Hospital. By Dr. LOMBARD . 393

Influenza or Epidemic Catarrh of the Winter, 1836-7. By Dr. STREETEN . ib.

ART. VIII.—1. Beobachtung einer auffallenden bisher unerkannten Structur des Seelenorgans bei Menschen und Thieren. Von C. G. EHRENBERG . ib.

Discovery of a remarkable Structure of the Brain and Nerves in Man and Animals. By C. G. EHRENBERG.

2. Anatomie der mikroskopischen Gebilde des menschlichen Körpers. Von Dr. JOSEPH BERRES . . . . . . . 394

Microscopical Anatomy of the Tissues of the Human Body. By Dr. BERRES.

3. Beiträge zur Aufklärung der Erscheinungen und Gesetze des organischen Lebens. Von GOTTFRIED-REINHOLD TREVIRANUS . . . . . ib.

Contributions to illustrate the Phenomena and Laws of Organic Life. By GOTTFRIED-REINHOLD TREVIRANUS.

4. Vorläufige Mittheilung microscopischen Beobachtungen über den innern Bau der Cerebrospinalnerven und über die Entwickelung ihrer Formelemente. Von ROBERT REMAK . . . . . . . ib.

Provisional Communication of microscopical Observations on the intimate Structure of the Cerebro-Spinal Nerves, &c. By ROBERT REMAK.

5. Ueber den Verlauf und die letzten Enden der Nerven. Von G. VALENTIN . ib.

On the Course and Terminations of the Nerves. By G. VALENTIN.

6. Ueber die Endigungsweise der Nerven in den Muskeln, nach eigenen Untersuchungen. Von Dr. FRIEDRICH-CARL EMMERT . . . ib.

On the Mode of Termination of the Nerves in the Muscles. By Dr. EMMERT.

7. Beitrag zur mikroskopischen Anatomie der Nerven. Von Dr. ERNST BURDACH ib.

Contribution to the Microscopical Anatomy of the Nerves. By Dr. BURDACH.

8. Elements of Physiology. By J. MUELLER, M.D. Translated from the German, with Notes, by WM. BALY, M.D. . . . . . ib.

ART. IX.—1. Practical Remarks on the Diseases of the Skin, on the external Signs of Disorder, and on the Constitutional Peculiarities during Infancy and Childhood. By WALTER C. DENDY, Surgeon . . . 425

2. A short Treatise on the external Characters, Nature, and Treatment of the different Forms of Porrigo, or Scalled Head, and Ringworm. By WALTER DICK, M.D. . . . . . . . ib.

PAGE

Art. X.—1. Etudes Chimiques sur le Sang Humain. Par Dr. Louis-René Le Canu 431
Chemical Researches on the Human Blood. By L. R. Le Canu, M.D.
2. Nouvelles Expériences sur le Sang. Par M. P. Denis . . . ib.
New Experiments on the Blood. By M. P. Denis.

Art. XI.—Notes on the Medical History and Statistics of the British Legion of Spain; comprising the Results of Gun-shot Wounds, in relation to important Questions in Surgery. By Rutherford Alcock, K.T.S. &c. . 448

Art. XII.—1. Inaugural Dissertation on the Presence of Air in the Organs of Circulation. By John Rose Cormack . . . . 455
2. Remarks on a Case of Suicide, published by Dr. P. D. Handyside, in No. 134 of the Edinburgh Medical and Surgical Journal. By J. R. Cormack, M.D. ib.
3. Bulletin de l'Académie Royale de Médecine. Tome II. No. 12. Rapport de M. Bouillaud sur les Expériences relatives à l'Introduction de l'Air dans les Veines, faites par M. Amussat.—Discussion sur l'Introduction de l'Air dans les Veines. Ib. . . . . . . . 456
4. Lettre sur l'Introduction de l'Air dans les Veines de l'Homme. Par M. Velpeau ib.

Art. XIII.—The Life of Edward Jenner, M.D. LL.D. F.R.S. &c. With Illustrations of his Doctrines, and Selections from his Correspondence. By John Baron, M.D. F.R.S. &c. . . . . . 477

Art. XIV.—Counter-Irritation, its Principles and Practice; illustrated by one hundred Cases of the most painful and important Diseases effectually cured by external Applications. By A. B. Granville, M.D. F.R.S. &c. . 498

Art. XV.—Practical and Experimental Chemistry, adapted to Arts and Manufactures. By E. Mitscherlich, Professor of Chemistry, &c. Translated from the first Portion of his Compendium, by Stephen Love Hammick, M.D. . 505

Art. XVI.—An Essay on the Antiquity of Hindoo Medicine, including an Introductory Lecture to the Course of Materia Medica and Therapeutics delivered at King's College. By J. F. Royle, M.D. F.R. and L.S. &c. . . 506

## PART SECOND.—Bibliographical Notices.

Art. I.—On Diseases of the Bladder. By Wm. Coulson, Surgeon . . 509

Art. II.—Manual of British Botany. By D. C. Macreight, M.D. . . 511

Art. III.—The Narrative of a Recovery from Tic Douloureux. By the Rev. C. E. Hutchinson . . . . . . . ib.

Art. IV.—Practical Observations on Hysteria, especially relating to its Organic Character. By John Pritchard, M.R.C.S. . . . . 512

Art. V.—Lehrbuch der vergleichenden Physiologie der Haus-Saügethiere. Von Dr. E. F. Gurlt . . . . . . . 513
Elements of the Comparative Physiology of the Domestic Mammalia. By Dr. E. F. Gurlt.

PAGE

**Art. VI.**—Die Nachtheile unzeitiger und uebermässiger Anwendung des Aderlasses und anderer Blutentziehung. Von Dr. L. Wetzlar . . 515

On the injurious Consequences of unnecessary and immoderate Bloodletting. By Dr. Wetzlar.

**Art. VII.**—A Practical Compendium of the Materia Medica; with numerous Formulæ adapted for the Treatment of the Diseases of Infancy and Childhood. By Alexander Ure, M.D. . . . . 516

---

## Part Third.—Selections from the British and Foreign Journals.

### I. THE FOREIGN JOURNALS.

#### ANATOMY AND PHYSIOLOGY.

Experiments on the Introduction of Air into the Veins, made by M. Amussat, before a Commission of the Royal Academy of Medicine of Paris . 517

On the Influence of Galvanism on the Process of Artificial Digestion. By Drs. Purkinje and Pappenheim . . . . . 527

#### PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

Cauterization of the Pharynx in Croup. By Dr. Felix Hatin . . 530

On the Employment of the Hydrochlorate of Tin. By Dr. Schlessinger . ib.

On Epidemic Epilepsy in Schools. By Dr. Meyer . . . 531

Treatment of Spermatorrhœa . . . . . . 532

On Artificial Auscultation. By M. Petrequin . . . . ib.

On the Use of the Essential Oil of Turpentine in Diseases of the Eye. By Dr. A. Trinchinetti . . . . . . 533

Employment of Chlorine in acute and chronic Bronchitis. By Dr. Toulmouche ib.

Of the Symptoms of the earliest Stage of Phthisis Pulmonalis. By M. Fournet 534

#### SURGERY.

On Chopart's Method of Amputating the Foot. By M. Blandin . . 535

On Rupture of the Perineum in Females. By Professor Dieffenbach . 536

Beneficial Action of Acetate of Lead in incarcerated Hernia. By Dr. Huxthausen 537

On Hemlock Baths in Cutaneous Diseases. By Dr. Fantonetti . . 538

Dislocation of all the Metatarsal Bones upon the Tarsus. By M. Mazet . ib.

#### MIDWIFERY.

On Puerperal Fever. By Dr. A. Gruber . . . . 539

Case of Spontaneous Rupture of the Uterus. By M. Gendrin . . ib.

#### MEDICAL JURISPRUDENCE.

Important Cases of Death after Wounds and Injuries . . . 540

### ANIMAL CHEMISTRY.

PAGE
Chemical Composition of Human Lymph. By R. T. Marchand and C. Colberg 544
Microscopical Researches on the Composition of Vaccine Fluid. By M. Dubois 546
Microscopical Examination of Pus, Mucus, and Effused Fluids. By Dr. Mandt ib.

## II. THE AMERICAN AND COLONIAL JOURNALS.

### MEDICINE.

Case of Pneumonia, with Polypiform Concretion in the Heart. By Professor Dunglison . . . . . . . 547

### SURGERY.

Operations for the Excision of the Ribs. By Dr. Warren . . . 548
Two Cases of double Amputation of the Legs from Frost-bite. By Dr. Sewell . 550

## III. THE BRITISH JOURNALS.

### PHYSIOLOGY.

Observations on the Fluid in the Vesiculæ Seminales of Man. By John Davy, M.D. 551
An Experimental Enquiry into the Influence of Nitrogen on the Growth of Plants. By Robert Rigg, Esq. . . . . . . 552
Researches on Suppuration. By George Gulliver, Esq. . . . 553
On the Differences of the Laws regulating Vital and Physical Phenomena. By Wm. B. Carpenter, M.R.C.S. . . . . . . ib.

### PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

On the State of the Pupil in Typhus, and the use of Belladonna in certain Cases of Fever. By Robert J. Graves, M.D. . . . . 554
A Statistical Enquiry on Fever. By Arthur Saunders Thomson, M.D. &c. . 556
On the Treatment of Rheumatic Gout. By A. L. Wigan, Esq. . . ib.
New Mode of exhibiting Copaiva. By Charles Evans, Esq. . . 558

### SURGERY.

On the Primary Causes of Strangulation, and an improved Mode of performing the Taxis in Cases of intestinal Herniæ. By James O'Beirne, M.D. . ib.
Observations arising out of the Results of Amputation in different Countries. By Benjamin Phillips, Esq. F.R.S. . . . . 561
On the Treatment of Small-pox by puncturing the Pustules. By J. Langley, Esq. 563
On the Efficacy of Pressure in certain Cases of Venereal Phagedænic Ulceration. By Hugh Carmichael, A.M. . . . . . 564
On Chronic Cystitis, &c. By John O'Bryen, M.D. M.R.C.S. . . . 565

### MIDWIFERY.

PAGE
On the best Means of applying Pressure to the Uterus after Delivery. By J. L. FENNER, Esq. . . . . . . 567

## PART FOURTH.—Medical Intelligence.

Provincial Medical and Surgical Association . . . . 569
British Association for the Advancement of Science . . . 573
Medical Relief under the New Poor-law . . . . . 586
New Work on Physiology. By W. B. CARPENTER . . . 590
College of Surgeons: Adjudication of the Jacksonian Prize . . . ib.
Account of a fresh Supply of Vaccine Virus from the Cow. By M. ESTLIN . 591
On the Medical Institutions of Cairo. By CLOT-BEY . . . 592
Prussian Regulations regarding Leeches . . . . 593
OBITUARY.—JOHN SIMS, M.D. . . . . . 594
BOOKS RECEIVED FOR REVIEW . . . . . ib.

INDEX, TITLE, &c.

THE
BRITISH AND FOREIGN
MEDICAL REVIEW,
FOR JULY, 1838.

## PART FIRST.

## Analytical and Critical Reviews.

### ART. I.

*Transactions of the Provincial Medical and Surgical Association.—London and Worcester.* Vols. I.—V. 1833-37. 8vo. (STATISTICAL AND TOPOGRAPHICAL PAPERS.)

IN the general notices of these volumes which we gave in former Numbers, we expressed our intention of referring to the statistical and topographical papers contained in them on some future occasion. This we now propose to do, with the view of bringing the important subjects to which they relate more prominently before our readers. It is not, however, intended to discuss the respective merits of these papers, on some of which, for obvious reasons, it is not for ourselves to pass any opinion. We are merely desirous of laying before our readers the more important facts which they contain, in the hope of impressing upon the profession the benefits likely to result from the cultivation of these branches of medical science, when conducted upon correct principles.

It has been reserved for the present age to estimate aright the value of statistical investigations; and it is now for the first time that we are fully alive to the necessity of reducing every branch of knowledge, susceptible of them, to the precision and rigorous exactness which numerical methods alone can give. No science, however rich in facts, can be said to have made real progress, until it is more or less brought within the sphere of mathematical calculation; and it is only in proportion as the rules and precision of what are termed the exact sciences are brought to bear upon it that any branch of knowledge can be said to approach perfection. This is a fact so evident in mechanical philosophy, that it need not here be insisted on; but that the natural sciences—those which are thought to depend more especially upon the accumulation of observations alone,—should be subject to the same rules, is perhaps not so clear to those who, from whatever cause, have not had the opportunity of tracing the recent progress of these sciences. The important aid, however, derived from the application of the doctrines of numbers and proportion to some of these must be familiar to all. It cannot have escaped the observation of any well-informed member of our own profession, that the precision introduced into chemistry, for instance, since the development of the atomic theory, has been attended with the most important and unlooked-for results; results quite independent of the hypothetical part of the theory, and arising solely from the application of the most simple elements of number and proportion to the facts of the science primarily deduced from experiment and observation. Errors of analysis have hence been detected, the existence of new principles has been rendered probable, and, by researches consequent thereon, demonstrated; while the whole science has assumed a precision and exactitude, an order and regularity, in strict accordance with that principle of harmony which pervades all creation. The same remarks apply to the natural sciences, more strictly so called. Astronomy, in particular, (which by some has been termed the Natural History of the Celestial Bodies,) is brought almost entirely within the sphere of physical science, by the application of the higher branches of mathematics to the investigation of the habitudes and relations of these bodies with respect to each other. Geology also seems now about to reap the advantages likely to result from the application of the same principles to the bewildering mass of facts and speculations, too often crude and sometimes wild in the extreme, with which the science is encumbered. The application of the principles of mathematical science in the development of the forms and structure of crystalline bodies by Hauy, and the doctrines of isomorphism lately advanced by Professor Mitscherlich, are of the first importance in the study of the mineral productions of our globe; while the researches into physical geography, by Humboldt and others, and recently in our country by Mr. Watson, show the importance of numerical methods of investigation, as applied to the vegetable and animal kingdoms. The statistics of Botany and Zoology, if they do not promise to make us better acquainted with the principle of life itself, will at least tend to develop some of those laws by which this principle acts, and through which it influences the organic world.

In a recent Number, we entered at some length into the consideration of the Numerical Method, as applicable to medical science; and, if we found reason to doubt the extent of this, as contended for by M. Louis and others, in regard to practical medicine, we entirely accord with that distinguished physician as to its immense value in every department of medical statistics. It is upon this method, indeed, that all medical enquiries of a statistical kind are mainly founded; and it is by the classification of the results obtained under its use, and the due comparison of these as they occur in different localities, that we expect to derive much knowledge as to the causes, the prevention, and the treatment of disease. When, therefore, we claim for statistical and topographical enquiries the earnest consideration of the medical profession, it is because we are convinced that these enquiries, if properly conducted, will throw light upon much that is now obscure, and reduce to precision much that is now doubtful in medical science. They will simplify our views as to the etiology of disease, by removing those elements from the enquiry into any particular case or class of cases which are foreign to it; and, in proportion as the causes are thus eliminated, the treatment adopted must, in like manner, become more simple, better adapted to its object, and consequently more efficacious.

From the nature of the materials in our possession, we must, on the present occasion, confine our attention chiefly to the southern and western parts of England, and especially to the western districts of Cornwall and the midland district of the Vale of Severn. In comparing these two localities together, we shall, for the most part, make use of the information contained in the volumes before us,—viz. of Dr. Forbes's Essay on the Land's End, for Cornwall; and of the valuable papers by Mr. Addison, of Malvern, Drs. Carrick and Symonds, of Bristol, Mr. Watson, of Stourport, Dr. Streeten, of Worcester, and Dr. Black, of Bolton. In addition to these, we shall avail ourselves of the facts contained in the Illustrations of the Natural History of Worcestershire, by Dr. Hastings; the Reports of the Worcester Infirmary and Dispensary, in the Midland Medical Reporter; and some other sources to which we shall allude as occasion requires.

The hundred of Penwith, or the district of the Land's End, is the western extremity of the county of Cornwall, and forms a projecting tract of a rude triangular shape, of which the sides washed by the sea are respectively about fifteen and twenty-five miles. The mean length of the district is twenty miles; the mean breadth about seven. From this it is evident that, in respect of climate, it must assimilate to that of a small island; as, with the exception of its eastern portion, or what may be termed its base, it is entirely surrounded by the waters of the Atlantic. The surface of this district is hilly, its shores precipitous; the mean height above the level of the sea probably between 400 and 500 feet; its highest hills not exceeding 1000 feet, although the hills beyond the boundary to the east are considerably more elevated. The geological formation is granite and slate of the primitive class; the alluvial deposits abundant in the valleys and low grounds, and partaking of the characters of the neighbouring rocks. There is a profusion of streamlets originating in the hilly tracts, but no river of any importance; the springs are copious and numerous, the water very pure. Both the granite and slaty rocks abound in metalliferous veins; and the extensive copper and tin mines, from the occupation which they afford to a large class of the inhabitants, form an important consideration in relation to medical statistics, as well as to other branches of statistical research. The climate of the Land's End is characterized by its equable temperature and its extreme humidity. The annual mean temperature at Penzance is 51°8; the greatest range, 65°; the mean annual range, 49°; the mean daily range, 6°7. The mean barometrical pressure is 29.61; the mean annual range, 1.68. Snow is seldom observed to fall, and in the vicinity of Penzance never remains on the soil for any length of time. The most prevalent winds are between north and west; those next in frequency between south and west. Thunder-storms are of rare occurrence. The productions of the soil also show the mildness of the climate; as many shrubs and plants grow in the open air, which, in most other parts of the kingdom, are killed by the cold and frosts of winter or early spring. The coolness of the summers, with the want of sunshine, however, prevents the due ripening of many of the late fruits,—as apricots, plums, &c.; and the harvests are by no means so early as in the midland counties.

The inhabitants are variously employed in agriculture, mining, handicraft and trade, and fishing. Of these the mining class is by far the most numerous, amounting to nearly one-half of the whole. They are exposed, in the mines, not only to severe work, frequently continued for a length of time, with a very scanty supply of light, and breathing a confined atmosphere, loaded with dust and impurities of different descriptions, but also to great humidity, conjoined with a considerable elevation of temperature, which increases with the depth of the mine and consequent atmospheric pressure. With respect to their domestic economy, many of the habitations of the poorer classes are built of clay and straw intermixed; but the cottages so constructed are generally warm and dry, however wretched in their external appearance. Fuel is scarce; the dress not peculiar; the diet composed chiefly of fish, pork, potatoes, and barley-bread. They were formerly much addicted to the use of ardent spirits, but there is now (since the introduction and spread of Methodism) a considerable improvement in this respect. Other features of their physical and moral condition we shall probably have occasion to refer to, in considering the diseases to which they are liable.

The general features of the Vale of Severn, although in some respects approaching to those of the Land's End, in others differ very considerably. That part of which the city of Worcester forms the centre is a midland district, extending the whole length of the county, from Bewdley on the north to near Tewkesbury on the south; a distance of about thirty miles. Its breadth, from the base of the Malvern chain to the somewhat elevated ground where the lias limestone appears, is about eight miles. The geological formation of the whole of this extensive valley, constituting the greater part of the plains of Worcestershire, belongs to the red marl, which is covered with a rich alluvial deposit, interspersed with numerous gravel-beds. The Malvern chain and outlines, which range from north to south, form the western boundary of this district, and are composed of rocks belonging to the granitic and transition series, with occasional appearances of basalt and other varieties of the trap rocks. The elevation of this chain varies from 1000 to 1500 feet above the level of the sea; that of the plains, bordering more immediately upon the Severn, is about eighty feet. The river Severn, which is usually confined low within its banks, runs through the centre of the district, in a direction nearly parallel with the Malvern Hills, and receives in its course the Stour, the Salwarp, the Terne, and other tributaries of less note; the Avon also joining it at Tewkesbury. The country, for miles round, is often flooded by the rise of the Severn and Terne, which are liable to overflow their banks whenever there are heavy and continued falls of rain in the upper country, whence they take their rise. Worcester, Stourport, Upton, and Bewdley, are the towns situated upon the banks of the Severn. Droitwich, which is also within the district, is on the Salwarp at no great distance. The water which rises through the red marl is, in many places, charged with sulphate of lime; the springs about Droitwich are more or less impregnated with salt, and those which have been sunk for the purposes of the salt-manufactory, long established in that place, are charged with it to the point of saturation. The Malvern waters are chiefly celebrated for their extreme purity. The climate of the Vale of Severn generally is mild and equable; a circumstance to which its proximity to a large arm of the sea, and its generally sheltered situation, materially contribute.* The number of rivers and the frequent inundations, with the low elevation of the greater part of the plains, necessarily render it humid; but the summers are warm, the vegetation luxuriant, at least in the marly districts, and the harvest some days earlier than in the neighbouring counties or in the Land's End. Of the natural productions of the Vale of Severn, the cider and perry afforded by the numerous apple and pear orchards, and the supply of hops from the extensive hop-yards, are the most important.

The chief occupations of the inhabitants are, in the country districts, agriculture; in the neighbourhood of Worcester, different branches of the glove manufacture and the china-works; at Droitwich and Stoke Prior, various operations carried on in connexion with the preparation of salt from the brine-springs of that neighbourhood. The general habits and condition of the poor present little that requires remark; the chief peculiarities being the frequent use of cider and perry, instead of malt liquor, in the agricultural districts; and the confinement and constrained position to which the females especially are subjected, from the nature of their occupation in the manufacturing of gloves. To some of the details connected with this subject we shall hereafter call attention, when considering their effects in the production of disease.

These observations, it will be remarked, chiefly apply to the more inland part of the district. It is, however, upon this that we have the most ample details, both in a topographical and a statistical point of view. The peculiar circumstances of Bristol, which is situated near the western extremity of the Vale of Severn, we shall draw attention to as occasion points out.

The difference in the longevity of the inhabitants of the two localities which we have thus rapidly surveyed, presents some features of considerable interest. Upon this point we have sufficiently full information in the tables published in the second volume of the Transactions. Table xiii. (*Trans. of the Prov. Med. and Surg. Assoc.*, vol. ii. p. 115,) shows the proportion of deaths to the living, in the whole county of Cornwall, taking the mean of the years 1811 and 1821, to be 1 in 66½; and that of the hundred of Penwith, or Land's End district, 1 in 64; that of the whole of England and Wales being 1 in 54. In Bristol, the ratio of mortality during ten years, from 1813 to 1822 inclusive, as ascertained by Drs. Carrick and Symonds, was 1 in 45. In the district lying between the river Severn and the top of the Malvern hills, an agricultural district forming part of the Vale of Severn, it is 1 in 65, as shown by Mr. Addison, from the returns of twelve years. From Table xv., however, (*Op. cit.* p. 119,) we learn that the ratio of mortality in the mining parishes of the hundred of Penwith (that is, in nine parishes in which the mining population, as compared to the other classes, is as 100 to 47,) is 1 in 58; whereas, in fifteen agricultural parishes, including towns and fishing villages, in which the mining population is to the other classes as 100 to 426, the ratio is only 1 in 64, or nearly the same as that of the Malvern district of the Vale of Severn. Dr. Hastings, however, states the rate of mortality in the whole county of Worcester at 1 in 52, and 1 in 53, as calculated upon the averages of five years preceding the several periods of 1811, 1821, and 1831, (*Illustrations of the Natural History of Worcestershire*, p. 17;) while from other documents, from an average of ten years, it appears to have been 1 in 56. The mean may be taken at 1 in 53. This ratio of mortality, however, includes the population of the towns; of which Worcester, containing about one-eighth of the whole population of the county, though by no means the most unhealthy, has a ratio of mortality of 1 in 48. These results will be more clear if exhibited in a tabular form.

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* The mean temperature of Worcester, in the year 1832, was 52.7; the mean barometric pressure, 29.93. The mean temperature of Malvern, according to Mr. Addison, is 49.6.; the mean barometric pressure, 29.362; the mean range of the barometer, 1.155.

| | Ratio of Mortality. | |
|---|---|---|
| In the whole of England . . . | 1 in 54 | Mean of 1811 and 1821. |
| — whole county of Cornwall . . | 1 ... 66½ | Ditto. |
| — hundred of Penwith . . . | 1 .. 64 | Ditto. |
| — hundred of Penwith . . . | 1 ... 61 | 1811. |
| — mining parishes of ditto . . | 1 ... 58 | 1811. |
| — agricultural parishes of ditto . | 1 ... 64 | 1811. |
| — whole county of Worcester . | 1 ... 53 | Mean of 5 years, 1816 to 1820. |
| — city of Worcester . . . | 1 ... 48 | 1821. |
| — agricultural district of Malvern . | 1 ... 65 | Mean of 10 years, 1821 to 1830. |
| — city of Bristol . . . . | 1 ... 45[*] | Ditto of 10 years, 1813 to 1822. |

Now, by comparing the agricultural parts of each district, in which the occupation of the inhabitants is the same, and finding that the ratio of mortality of the one so nearly approaches to that of the other, we are entitled to infer, assuming these details to be correct, that the occupation of the population is that one condition which, of all others, would seem to influence most the average period to which life extends. Any differences which may exist between these two localities in climate, soil, natural productions, &c. appear either to be trifling in themselves, to counterbalance each other, or to exert but little effect on the general longevity of the population. The sketch which we have given of the respective topographies of the Land's End and Vale of Severn, however, shows that several important points of diversity are found in these situations, although those of climate have been perhaps over-rated. It remains, therefore, to be decided which of the two latter suppositions are correct. A very considerable difference, it will be observed, exists in the ratio of mortality of the cities of Worcester and Bristol, on the one hand, and that of the mining part of the Land's End, on the other. In the latter, which differs from the agricultural part of the same district chiefly in the occupation and mode of life consequent thereon of the greater portion of its inhabitants, we have a considerable increase in the rate of mortality; which is, however, still by no means equal to that of Bristol, or even of Worcester. In these situations, therefore, other causes, besides the occupations of their inhabitants, must come into operation; or these occupations must be presumed, taken conjointly, to be more injurious than mining. As we have before remarked, the diversities of climate have probably been overestimated; and those of soil, or other conditions which are common to the agricultural and manufacturing or trading population, may, for various considerations, be thrown aside. Are the occupations, then, of these large towns, especially of Bristol, so extremely injurious in their effects upon the health and longevity of the working classes? or is the higher rate of mortality to be attributed, in great part, to the crowding together of the inhabitants, and to the more precarious or insufficient means of support in manufacturing and commercial towns?

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[*] These ratios are not quite in accordance with those deducible from the population returns of 1831; but we have thought it better to make use of the details afforded us in the several papers, as they seem to have been drawn up with great care by their respective authors, and entitled to the fullest credit.

The ratio of mortality of a district does not, however, indicate precisely the average longevity of its inhabitants, but is a compound result, arising from a consideration of the total population, increased by the births and influx of strangers, on the one hand, and diminished by the deaths and emigration, on the other; so that in a district in which there is a variable immigration of strangers, or emigration of its native population, the apparent ratio of mortality will be influenced by many circumstances which are foreign to the true ratio, or that which indicates the longevity of its native population. Further, the average longevity being deduced from the combined longevities of each individual residing in the district, it is evident that, from peculiar circumstances, acting at different periods of life, other elements than those simply expressive of the total numerical ratio should, in all cases, enter into the calculation, before we can deduce correct results as to the influence of any particular cause or causes operating in limiting the extent of the average longevity of the whole. The tables in the first part of the Essay on the Land's End District (Tables xv. to xxii., *Prov. Trans.*, vol. ii. p. 121 *et seq.*) are peculiarly instructive in regard to this point, as they not only show that, in the mining parishes of the hundred of Penwith, the longevity of the female part of the population greatly exceeds that of the male, and that, as compared with the agricultural parishes, the longevity of the agriculturist is greater than that of the miner; but that this difference obtains also in early life, and before the occupation could be supposed to exert any direct influence. The explanation of this fact is probably to be found in the more or less debilitated or diseased state of the parents, induced by their occupations or habits of life, operating in rendering their children of a more weakly constitution, even from birth.

To enter fully into such questions as these would require a much more extended notice than we can here devote to the subject, and we therefore turn to the consideration of the differences which may be found to exist in these several localities as respects the conditions of disease. The following table exhibits a numerical statement of the diseases occurring in the Land's End and at Worcester, classified and arranged for the purpose of convenient reference in the subsequent part of these observations.

| | Worc. city, 1832. | Worc. Disp. 1828–31. | Worc. Inf. 1828-30-32. | Worcester, Total. | Land's End. | Hobart Town. |
|---|---|---|---|---|---|---|
| 1. General fevers | 301 | 318 | 73 | 692 | 421 | 1097 |
| Eruptive fevers | 101 | 34 | 0 | 135 | 260 | 99 |
| 2. Rheumatism and neuralgia | 62 | 110 | 290 | 462 | 490 | 2428 |
| 3. Dropsies | 22 | 43 | 48 | 113 | 152 | 198 |
| 4. Scrofula | 26 | 21 | 55 | 102 | 253 | 93 |
| 5. Scirrhus and cancer | 2 | 5 | 20 | 27 | 42 | 4 |
| 6. Diseases of the brain and nerves | 94 | 133 | 180 | 407 | 638 | 1252 |
| 7. Diseases of the heart, &c | 11 | 32 | 64 | 107 | 78 | 32 |
| 8. Diseases of the lungs: | | | | | | |
| Bronchitis, asthma, catarrh, &c. | 191 | 329 | 216 | 736 | 601 | 1784 |
| Phthisis and hæmoptysis | 112 | 235 | 69 | 416 | 297 | 202 |
| Pneumonia, pleuritis, &c. | 99 | 145 | 220 | 464 | 572 | 516 |
| 9. Diseases of abdomen: | | | | | | |
| Diarrhœa, cholera, and dysentery | 636 | 78 | 27 | 741 | 354 | 2711 |
| Dyspepsia, &c | 170 | 318 | 233 | 721 | 952 | 685 |
| Hepatitis and jaundice | 24 | 74 | 187 | 285 | 89 | 310 |
| Gastritis, peritonitis, and others | 69 | 101 | 88 | 258 | 630 | 2557 |
| 10. Diseases of the urinary organs | 10 | 24 | 16 | 50 | 106 | 118 |
| Diabetes | 0 | 2 | 8 | 10 | 8 | 0 |
| 11. Uterine diseases | 33 | 77 | 143 | 253 | 625 | 108 |
| 12. Chronic eruptions | 57 | 39 | 135 | 231 | 528 | 1457 |
| 13. Diseases of the organs of the senses | 46 | 42 | 176 | 264 | 293 | 1065 |
| 14. Syphilitic diseases | 38 | 34 | 115 | 187 | 51 | 1135 |
| 15. Bronchocele | 7 | 11 | 29 | 47 | 7 | 0 |
| 16. Miscellaneous diseases, chiefly surgical | 159 | 203 | 1132 | 1494 | 1344 | 6779 |
| Hernia | 5 | 9 | 256 | 270 | 36 | 363 |
| Accidents | 63 | 35 | 685 | 783 | 0 | 5015 |
| Totals | 2338 | 2452 | 4465 | 9255 | 8827 | 30008 |

We have been at some pains to draw up and arrange the preceding table from tables of diseases given in the Provincial Transactions and in the Midland Reporter. The results of the first column are obtained from the tables of disease as it appeared in the city of Worcester, during the year 1832, (*Transactions of the Prov. Association*, vol. i.); the second column is drawn up from the Reports of the Worcester Dispensary, for the years 1828, 29, 30, and 31, published in the Midland Reporter; the third from the reports of the Worcester Infirmary, for the years 1828, 29, 30, and 32,* (*Midland Medical Reporter*, vols. i. ii. and iii.; *Prov. Trans.*, vol. i. p. 373); the fourth presents the totals of the three preceding columns; the fifth column contains the results of Dr. Forbes's tables of disease in the Land's End district, and embraces a period of seventeen years,—namely, three years, from 1810 to 1812, three years, from 1819 to 1821, and eleven years, from 1823 to 1833 inclusive, (*Provincial Transactions*, vol. iv. p. 167); and the sixth presents a general abstract of the returns, furnished by Mr. Scott, of the Medical and Surgical Diseases treated at the Colonial Hospital, Hobart Town, Van Diemen's Land, during the years 1821 to 1831 inclusive, (*Prov. Trans.*, vol. iii.,) and is here added for the sake of comparison with the results afforded by the preceding columns. We might have embraced with advantage the diseases of many other localities in this tabular view, especially those of London, of Carlisle, and of Birmingham, and might thus have been enabled to compare the conditions of disease among the vast and crowded population of the metropolis, and in large towns situated in the northern and central portions of the kingdom; but we are desirous, on the present occasion, of confining our attention as much as possible to a comparison between the two districts to which the preceding tables chiefly refer. We shall, however, have occasion, in the course of our observations, to refer to the state of disease as it is found to prevail in these and other places, with the view of illustrating the peculiarities of the districts more especially under consideration.

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* We should gladly have availed ourselves of the tables of disease given in the Reports of the Worcester Infirmary, for the years 1835 and 1836, and published in the fifth volume of the Transactions; but as fever, and other diseases presumed to be contagious, would seem to be excluded from that institution, and as we have no reports from the Dispensary, or from any other institution existing in that city at which such diseases come under treatment, we have thought it better not to include the results of these tables in the general aggregate here given.

*Fevers.* General fever appears to be much more prevalent in the Vale of Severn than in the Land's End. The proportion in the city of Worcester, as obtained from the first and second columns of the preceding table,—by far the greater proportion of cases admitted at the dispensary, to which the second column refers, being from the city itself, (*Midland Reporter*, vols. i. ii. and iii.,)—is between one-seventh and one-eighth of the total cases of disease. Of the prevalence of fever in the surrounding country we are unable to form an estimate, as we have no returns of any fever-hospital, and the small number admitted at the infirmary, amounting only to seventy-three cases during a period of four years, out of 4465 cases of disease in general, shows that there is probably some restriction placed upon the admission of this description of cases within the walls of that institution; a restriction which would seem also to extend to other disorders presumed to be contagious, as we find no notice of any eruptive fevers having been admitted during the period referred to.* Out of 578 cases reported by Mr. Addison, (*Prov. Trans.*, vol. iv. p. 87,) it appears that 64, or 1 in 9, were fever; so that the country lying between the banks of the Severn and the base of the Malvern Hills, including also the elevated village of Malvern itself, would not seem to enjoy much greater immunity from the causes generating fever than the more crowded and confined districts of the city of Worcester. This proportion, namely 1 in 9, is, however, somewhat too high, as Mr. Addison has not included surgical cases in his return. Still we may fairly draw the inference that the fever of the Vale of Severn, prevalent during the period to which these observations refer, is to be attributed chiefly, if not altogether, to the operation of causes in a great measure independent of the artificial habits or condition of the population, and partaking either of the nature of emanations from the soil, or of some atmospherical condition, or (to use the language of Sydenham) reigning epidemic constitution, operating more or less throughout the whole district. We are strengthened in the former of these opinions by observing that, in those parts of the district referred to in which miasmatic exhalations more especially prevail, the cases of fever were of more frequent occurrence; as in the low lands bordering the river Severn, and particularly among the population living within the sphere of influence of a canal which skirts one of the suburbs of the city of Worcester, (*Midland Reporter*, vol. i. pp. 207 and 349.) In Bristol, the number of fever-cases, in 700 of disease generally, admitted at the infirmary during the year 1832, as stated by Drs. Carrick and Symonds, (*Prov. Trans.*, vol. ii. p. 177,) was 171, or nearly one-fourth. How far this very high ratio of prevalence is to be attributed to a more crowded and more necessitous population, or to the peculiar circumstances under which this city is placed with respect to the accumulation of refuse of every description in the muddy bed of the Avon, the exhalations from which are, from the rise and fall of the tide, periodically left to spread their deleterious influence over the crowded and ill-ventilated streets of the lower town, is a question for the decision of those who are better acquainted with the locality; but the following extract from the essay of Drs. Carrick and Symonds on the Medical Topography of Bristol, published in the second volume of the Transactions, is to us a sufficient ground for attributing the prevalence of fever to the latter of these causes, although we doubt not that its severity may be materially increased by other circumstances.

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* In the returns of the Worcester Infirmary for 1835 and 1836, out of 2627 cases of disease, 36, or 1 in 73, were cases of continued fever, which approximates very closely to the results of the preceding years.

> "Before the floating harbour was constructed, every facility was afforded for the removal of filth, by the unobstructed course of the rivers; [the Frome and Avon, upon the junction of which Bristol is situated;] but, after the accomplishment of that work, the accumulation of the matters discharged by the drains in a stagnant body of water occasioned no slight inconvenience to the citizens for many years." . . . .
> "Within the last few years, it has been customary to empty the float twice annually, and a large sewer has been formed, which runs from the embouchure of the Frome in a direction parallel with the float, and, after receiving the drains from the central parts of the city, discharges itself into the new channel of the Avon. Into this sewer the Frome sends a division of its stream, loaded with the contributions which it has received at every step of its progress through some of the most closely built and densely crowded districts. Unhappily the current of this river is narrow, torpid, and scanty; in consequence of which it often struggles ineffectually with the burthens accumulated upon it, and deposits them upon its bed, the sides of which become elevated into pillows for the exhausted, almost stagnant, waters, and exhale miasms sufficient, it might be imagined, to infect the whole neighbourhood. In the summer time we have seen the stream shrunk to a width scarcely exceeding that of a large gutter, and trickling between two mounds of mud. At all times it is almost impossible to cross the bridges by which it is concealed from sight, in the midst of streets and lanes, without being reminded, by particular odours, of its propinquity." (*Prov. Trans.* vol. ii. p. 162.)

The superiority of the Land's End in this respect is very great; the proportion of fever cases to those of other diseases being scarcely more than 1 in 21; evidencing, in a remarkable manner, either the absence of miasms, at least such as are capable of generating fever, or their speedy dispersion, or neutralization by other agents in operation throughout the district. It is a curious fact, and one which tends much to confirm the accounts which have been given of the salubrious nature of its climate, that, in the newly broken-up country of Van Diemen's Land, the proportion of fever should be so low as 1 in 27; or, excluding the accidents, the number of which is very great, 1 in 23. Out of 18,663 cases received as out-patients at the Birmingham Infirmary, during the years 1832 to 1835 inclusive, (*Reports of the Out Cases attended at the Birmingham Infirmary*, by Messrs. Parsons and Ryland, *Provincial Transactions*, vols. i. to iv.), there were 1468 cases of fever, or 1 in 12.7. This is a ratio of prevalence considerably lower than those of the Vale of Severn, which we have previously given. At Plymouth, according to Dr. Blackmore's Tables (*Reports on the Diseases of Plymouth*, by E. Blackmore, M.D., *Edinburgh Med. and Surg. Journal*, vol. xxxi. p. 269), of 5648 cases there were 400 of fever, or 1 in 14.2; a ratio lower than that of the midland town of Birmingham, but still considerably higher than that of the Land's End. It would hence appear that the opinion expressed in the Essay on the Medical Topography of the Land's End, as to "the great prevalence of the common continued epidemic fever of this country, variously denominated contagious fever, typhus, &c. in the town and immediate vicinity of Penzance," (*Prov. Trans.*, vol. iv. p. 162,) however just in itself, is by no means applicable to the district in question, when compared with other localities. The total number of cases of general fever included in the Penzance Tables, from which the ratio before given, namely 1 in 21, has been deduced, is 421; but in this number are included 26 cases of infantile remittent, and 10 of hectic. Excluding these, and taking the cases of continued fever only, the common epidemic of the country, with a very few, not more than three cases of intermittent, we have 385 cases extending over a period of seventeen years; that is between twenty-one and twenty-two cases of fever each year, which, as compared with other places, seems to be a small number to apply for relief from this cause at a public institution in the midst of a crowded population. At Bolton le Moors, in which continued fevers ("typhus and low fevers,") are thought, of late years at least, not to have prevailed to any serious extent (*Medical Topography of Bolton*, by Dr. James Black; *Prov. Trans.*, vol. v. p. 201,) of 1901 cases of disease occurring between the 1st of November, 1824, and the 31st of October, 1825, 68, or 1 in 28, were synochus and typhus; which does not differ greatly from the Land's End ratio of the same forms of febrile disease. The far greater prevalence of common continued fever in the cities of Worcester and Bristol, situated as they are in the midst of malarious districts, or at least of low, moist meadows on the banks of rivers, and exposed to the exhalations arising from stagnant waters and canals, is a strong evidence in favour of the views advocated by the late Dr. M'Culloch; and the testimony of the older practitioners in Cornwall, who are of opinion that "sporadic fevers of a continued or imperfectly remittent character, and of a contagious nature, were more frequently met with in former years, more particularly in the lowest and dampest localities, and on the more uncultivated moors," (*Prov. Trans.*, vol. iv. p. 165,) is to a certain extent confirmatory of the same opinions. It will be observed that these remarks are somewhat at variance with the opinion expressed in the Essay on the Medical Topography of the Land's End.

"In explanation of the cause of the continued prevalence of epidemic fever in this district, it will, I apprehend, be considered sufficient, by most persons, to refer to the economical details in the former part of this paper. In the close, ill-ventilated, and dirty tenements, in the poverty and deficiency of food and clothing, and in the mental depression consequent thereon, we may find ample causes of fever, although scarcely any of the usual sources of malarious influence are to be traced in the district—a fact still more strikingly demonstrated by the almost total absence of intermitting and remitting fevers. This is a circumstance deserving the attention of those who are disposed, with the late Dr. Armstrong, to look to terrestrial miasmata for the cause of all our fevers," (p. 165.)

There can be little doubt that the views of Drs. M'Culloch and Armstrong were too confined and too exclusive upon this point; but the absence of intermitting or remitting fevers by no means proves the absence of malaria. Malarious influence may be present, although not in such intensity as to develop the purely intermittent or remittent types of fever; and, in the present state of our knowledge, it is impossible to say how far the slighter degrees of this influence, or the modified sources from which it is derived, may operate in the production of the *more* continued form which fever has assumed, not only in this locality, but throughout the country generally, since draining has been more extensively and more regularly practised. We have said the *more* continued form, because we believe that few who have been accustomed to observe the common epidemic fever of this country will deny that, in the earlier stages, at least, and even after the disease is fully formed, an evident tendency towards remission occurs in almost every case, the exceptions being commonly found only in close ill-ventilated apartments in confined situations.

There appears to be little difference in the general characters of the fever in the two districts more especially under notice. Dr. Forbes states, that "the continued fever of the Land's End exhibited nothing peculiar in its character. It was often very severe, long-protracted, and frequently fatal. Death occurred rarely from sudden collapse; sometimes from intestinal hemorrhage; commonly by the typhoid or comatose state, indicating an affection of the brain of an unknown quality; never by the supervention of anything like putridity," (p. 164.) The following extract from the Report of the Diseases of Worcester during the year 1832, by Dr. Streeten, shows the character of the disease in that situation:

"The fevers which occur in Worcester, necessarily vary according to the dryness or moisture, and other changes of the atmospheric condition, and according to the greater or less exposure of the particular localities in which these affections are prevalent to the operation of such changes; but as those parts of the town which are chiefly inhabited by the poor, amongst whom fever is generally found to prevail more than in the more elevated classes of society, are low in situation, and exposed to exhalations arising from the presence of moisture and a mixture of vegetable and animal refuse, the fever which occurs here, is chiefly of such a nature as that which arises in corresponding situations elsewhere. Intermittents in their most distinctive form are scarcely known, for there are no purely marshy or fenny spots; but from the effects of imperfect drainage, and the mixture of animal and vegetable exhalations, fever of a mild typhoid type is very common,—many parts of the town, and particularly those situated in the ill-drained districts bordering upon the canal, being scarcely ever entirely free from it. In a general sense Worcester may be said to be tolerably well ventilated; and, accordingly, true typhus fever is of comparatively rare occurrence, though instances of apparent communication of the usual low typhoid fever from one individual to another, are occasionally met with. The local affections with which these fevers are commonly accompanied, are most frequently a sub-acute bronchial inflammation, which is especially found on the banks of the river, or in those who are, by their occupations, predisposed to pulmonary affections of this description. In other parts of the town, and especially in such as are damp and ill drained, as is the case with the localities before referred to as bordering on the canal, and in some low grounds near the river, a gastro-enteric affection is a frequent accompaniment, marked by more or less pain at the pit of the stomach, tenderness of the abdomen, diarrhœa, and, sometimes, even hemorrhage from the bowels. Severe headach, with delirium and general marks of inflammatory action within the cranium, are chiefly found in the suburbs and more open districts, and conjoined with greater depression of the powers of the nervous system, in the close and badly ventilated spots in the heart of the city." (*Prov. Trans.*, vol. i. p. 380.)

The following observations by Dr. Hastings in his Illustration of the Natural History of Worcestershire are also well worthy of notice. "The whole of this county," he remarks, "in which the sandstone prevails, enjoys a great freedom from severe febrile diseases, the worst forms of typhus fever being rarely witnessed; and, from my own experience, I may assert that the hilly parts of Worcestershire, where there is a clay soil, as about Broadway, and the line of hills running in that direction, are much more prone to be affected with fever than the sandstone district which comprises the lower parts of the county. This immunity from severe forms of fever is not confined to the rural population; but is in a great measure also to be observed in the large towns. In Worcester it is a very rare thing to meet with very severe cases of fever, and the worst forms of the disease in this town are seen after flood-time in the vicinity of the river Severn," (p. 31.)

But although the general characters of fever, as it occurs in Worcester and the Land's End, present but few distinctive marks, it would appear that, in the latter situation, the cases were on the whole more severe, and consequently more fatal. The ratio of mortality from this disease is not given in the Penzance Tables, but in Worcester it is very low; as of 306 cases occurring at the Worcester Dispensary, from 1828 to 1831, it is stated that only 13, or 1 in 23½, proved fatal; and of 96 occurring at the same institution during the year 1832, there were four deaths, or 1 in 24, almost exactly the same proportion as the average of the preceding years. (*Prov. Trans.*, vol. ii. p. 384.) Dr. Blackmore, gives as the ratio of mortality of fever cases in Plymouth, 1 in 21, which does not greatly differ from that of Worcester; while in Edinburgh, according to Dr. Craigie's Clinical Report, of 825 cases of fever admitted at the Infirmary, from the 25th of October, 1834, to the 24th of October, 1835, 83, or 1 in 9¾, proved fatal. (*Edin. Med. and Surg. Journ.*, vol. xlvi. p. 37.) 1 in 15.6 is given by Mr. Parsons as the ratio of the years 1833, 34, and 35, at Birmingham. (*Prov. Trans.*, vol. iv. 490.) In the London Fever Hospital, according to Dr. Southwood Smith, the proportion of deaths is very much higher, amounting to about one-fourth of the cases.

The tables afford but little information respecting eruptive fevers, under which are included Small-Pox, Measles, Scarlet-Fever, Urticaria, and Erysipelas; as the number of cases is not great, and from the greater proportion of such cases being usually the subject of domestic treatment, those which are included in Infirmary or Dispensary Returns afford but little indication of the extent to which they frequently prevail. It should however be remarked, that the ninety-nine cases mentioned in the Van Diemen's Land returns were all cases of erysipelas; none of the epidemic eruptions having as yet made their appearance in the colony.

*Rheumatic and Neuralgic Diseases.* In the Penzance Tables, the various forms of rheumatism and neuralgia are classed together under one general head, and amount to 490 cases in all. Of these, 439 were acute and chronic rheumatism, and 51 sciatica, lumbago, neuralgia, and nervous pains. The proportion to the whole number of cases is 1 in 17.6, but there appears to have been a considerable variation in the degree of prevalence of this class of diseases in different years embraced within the period over which the reports extend, and a remarkable increase during the later years. The following is the proportion of rheumatism, as observed in various localities:

| | | |
|---|---|---|
| Worcester city . . | 1 in 30.6 | Dispensary Reports, Midland Reporter. |
| Birmingham . . | 1 ... 29.0 | Messrs. Parsons and Ryland, Prov. Transactions. |
| North of England . | 1 ... 22.0 | Dr. Haygarth, quoted by Dr. Forbes. |
| Malvern[*] . . . | 1 ... 20.6 | Mr. Addison, Prov. Transactions, vol. iv. p. 136. |
| Penzance . . . | 1 ... 20.3 | Dr. Forbes, Prov. Trans. vol. iv. p. 177. |
| Worcester city & county[†] | 1 ... 20.0 | Midland Reporter, Prov. Trans. (see Table.) |
| Plymouth . . . | 1 ... 18.4 | Dr. Blackmore, Ed. Med. Journ. vol. xxxi. p. 271. |
| Penzance[‡] . . . | 1 .. 17.6 | Penzance Tables, Prov. Trans. vol. iv. |
| Bolton le Moors . | 1 ... 17.6 | Dr. Black, Ed. Med. Journal, vol. xxv. p. 444. |
| Plymouth . . . | 1 .. 17.0 | Dr. Woolcombe, quoted by Dr. Forbes. |
| London . . . | 1 .. 14.7 | Drs. Willan and Bateman, quoted by Dr. Forbes. |
| Van Diemen's Land . | 1 ... 12.3 | Mr. Scott, Prov. Trans. vol. iii. |
| Bristol[§] . . . | 1 ... 6.8 | Drs. Carrick and Symonds, Prov. Tra. vol. ii. p. 177. |

It is obvious, from a consideration of the preceding results, that one of the causes upon which rheumatism has been supposed to depend is not of that importance in the production of the disease which has been assigned to it. Of the ten localities given in the preceding table, the highest ratios of prevalence are by no means to be found in the coldest situations. Thus, in the north of England, the proportion of rheumatism is only 1 in 22; whereas, in the milder climates of the Land's End, Plymouth, and London, it is respectively 1 in 20.3, 1 in 17, and 1 in 14.7. The influence of moisture cannot, however, fail to be noticed in the cases of Plymouth and London, and more especially Bristol. In this last-mentioned site, the ratio of prevalence is as high as 1 in 6.8; and though, as we have remarked, this is by no means to be taken as the actual proportion in which the disease occurs, still its frequency is such as considerably to exceed most other places in the kingdom. This, however, is not difficult to account for, situated as Bristol is, on the Western side of the island, near the mouth of a large river, or rather arm of the sea, and upon the junction of two smaller rivers, with floats and basins and stagnant waters intersecting the town; the damp exhalations arising from which must necessarily be much confined in the lower portions of the town by the surrounding elevated land.

The greater prevalence of those causes which are instrumental in the production of neuralgic disorders in the Vale of Severn than in the Land's End, is shown by the relative increase in the ratio of prevalence in the former of these localities, when this class of diseases are taken into the estimate. Thus, in Worcester, the ratio of prevalence of rheumatism alone is 1 in 30.6, which, by including the neuralgic affections, is increased as much as to 1 in 20; whereas, in Penzance, where the proportion of rheumatic cases is 1 in 20.3, the increase from the addition of neuralgia is not more than to 1 in 17.6.

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[*] This probably includes lumbago, sciatica, and perhaps neuralgia.

[†] Lumbago, sciatica, and neuralgia are included in this ratio. [‡] Ditto.

[§] This is a very high ratio, and is obtained from 700 medical cases treated in the Bristol Infirmary, of which 117 were rheumatic. Making every allowance for the addition of cases of lumbago and sciatica, which are probably included in the 117, and the exclusion from the total of the surgical cases, it will still much exceed the ratio of other places.

It may be observed, that this mode of computing relative proportional results, by comparing the increase of the ratios of prevalence which is obtained by the addition of other elements, although not affording conclusions absolutely correct, is yet of great importance in some cases, as giving a closer approximation to the truth than can be attained by the ordinary method of comparing the apparent ratios of prevalence of any one disease, taken separately. Many causes tend to affect the correctness of the proportions thus ascertained; as, the different views of the authors by whom the diseases have been respectively classified; the admission or omission of surgical diseases, and of the slighter medical cases in the tabular arrangements, &c. Thus, in the Hobart Town Report, the proportion of surgical cases is very large; the accidents alone amounting to rather more than one-sixth of the whole number of cases registered; and, though we might easily get rid of this source of error by omitting this class of affections from the estimate, which indeed, strictly speaking, should be done, (as accidents have nothing to do with the general conditions of disease,) we are precluded from doing so on the present occasion by some of the tables enumerating these among other surgical diseases. But the relative increase in the ratio of prevalence of a disease, in one locality as compared with another, by the addition of another disease, incontestibly shows that, in the two localities, one class of cases is proportionally of more frequent occurrence than the others; as in the instance above given of rheumatism and neuralgia. Setting aside other sources of error, we are thus fully entitled to conclude that neuralgic affections, as compared with rheumatism, are relatively more prevalent in Worcester than in Penzance; or, in other words, that the causes generating neuralgic affections, whether really more prevalent in Worcester or not than the causes generating rheumatism, are relatively in greater activity than at Penzance, although in this latter situation both the one and the other may be actually of greater or less prevalence.

The proportion of cases of acute rheumatism to those of the chronic form of the disease, appears to vary greatly in different parts of the kingdom. Dr. Forbes regards the acute disease as of comparatively rare occurrence in Cornwall.

> "The whole cases seen by me, in four years and a half, were four in number, viz. two in 1817, and two in 1821; not one case having been met with in the three intermediate years. In the Dispensary Report for 1821, I stated, 'During the whole of the last two years, not one case of acute rheumatism has been entered in the books; and I cannot help thinking that the total absence of the disease, among so large a body of individuals, for so long a period, is a circumstance that would be reckoned very singular in the northern or even central parts of our island. It is, at all events, I think, sufficient to render doubtful the truth of a common notion, that a moist climate is favorable to the production of rheumatism." (*Prov. Trans.*, vol. iv. p. 174.)

It appears, however, from the testimony of Dr. Montgomery, to have been of more frequent occurrence at Penzance in subsequent years. The numerical proportion of the acute to the chronic disease is stated by Dr. Forbes to have been, during his residence, and the two years subsequent to his leaving the Penzance Dispensary, 5 of the former to 69 of the latter, or nearly 1 in 14 (1 *to* 14, that is, 1 *in* 15.) The proportion in London appears to be 2 in 7, and the mean of the London and Plymouth tables, and Dr. Haygarth's Reports from the north of England, referred to by Dr. Forbes, is 1 in 3. The proportion, however, in the midland counties varies greatly from those here given. In the Reports of the Worcester Dispensary for the year 1828, (*Midland Medical Reporter*, vol. i.), the distinction between the acute and chronic forms is observed, and out of 20 cases there were 12 of the former to 8 of the latter, or 3 in 5. In the year 1835, of 75 cases admitted at the Worcester Infirmary 54 were acute and 21 chronic, or nearly 3 to 1, (*Prov. Trans.*, vol. v.) In the Birmingham tables (*Prov. Trans.*,) of 642 cases, 416 were acute and 226 chronic, being nearly 2 in 3, or in the proportion of 2 to 1; exactly reversing the mean of London, Plymouth, and Carlisle. From Mr. Watson's observations on the Medical Topography of Stourport (*Prov. Trans.*, vol. ii. p. 181), it may also be inferred that acute rheumatism is of frequent occurrence in that neighbourhood, while the chronic form of the disease is not so: he says, "I have never considered chronic rheumatism as a frequent disease here; but I must observe, that I have generally considered those pains which are commonly called rheumatic as depending upon a disordered state of the digestive organs, and treated them as such," (p. 197.)

The causes of this great variation in the relative prevalence of the acute and chronic forms of rheumatism in different localities do not appear; and it will probably require an extended series of observations to remove the obscurity in which they are at present involved.

> "I am not aware," (says Mr. Watson,) "that it is more frequent among watermen than among those persons who are less exposed to wet and cold; and at least one half of my patients have been young women. Nor are those persons who are exposed to great change of temperature in foundries and forges more liable to rheumatism: I do not, at present, recollect a single case of acute rheumatism among them." (*Prov. Trans.*, vol. ii. p. 196.)

> "The evidence supplied by the practitioners of the district," says Dr. Forbes, "as to the relative prevalence of rheumatism among the different classes of the community is somewhat contradictory, but the weight of testimony is decidedly in favour of the opinion that the disease is not more prevalent among miners than among other labourers; and this is the conclusion to which my own observation leads. It must be owned, that this is rather a singular circumstance, according to the commonly received etiology of the disease, when we refer to what was stated in a former part of this paper respecting the extreme dampness, or rather wetness, of some of the mines. Would it, therefore, seem probable that the effect of the moisture was counteracted by the warmth of the locality, and the immunity from currents of air in the bottom of the mines?" (*Prov. Trans.*, vol. iv. p. 177.)

These remarks, however, refer to the chronic as well as to the acute form of the disease, and apply rather to the occupation of a portion of the population than to the circumstances of the whole district. The suggestion thrown out as to the probable influence of the warmth of the mines, in counteracting the effects of the damp, is equally applicable to those who work in foundries and forges, alluded to by Mr. Watson: but in these latter situations there is certainly no immunity from currents of air:

possibly in both instances the continued and violent exercise, and the consequent open state of the skin, may act as a counteracting influence. However this may be, we must observe that our own experience does not bear out Mr. Watson's statement, that acute rheumatism is not more frequent among watermen than among persons less exposed to wet and cold; and that, with respect to foundries and forges, humidity, which we are inclined to consider as the more efficient cause in the production of the disease, does not particularly prevail.

We have dwelt thus long upon the subject of rheumatism, because we believe it to be one upon which erroneous opinions are very prevalent; at least the commonly received views as to the causes of the disease would seem to have been taken up without a sufficient examination of the grounds upon which they have been adopted. Nothing is wanting to develop the true bearing of the alleged causes, cold and humidity, in the production of the disease, but a series of careful observations in different parts of the country. There are few diseases which exert a more baneful influence upon the welfare and comfort of a considerable portion of the community. Not only is the attack itself frequently of extreme severity, and, from the protracted duration of the symptoms, attended with considerable loss of time to those who can ill spare it, but the foundation of permanent derangement of health, especially amongst the labouring part of the community, is too frequently laid in permanent and irretrievable organic disease of the heart, or in thickening and contraction, or other changes, of structure in the joints. The real remedy for these serious evils is to be found in the adoption of such measures as are calculated to prevent, or at least to diminish, the frequency of its occurrence; and efficient measures can only be devised for the attainment of this purpose from an accurate acquaintance with the true cause or causes of the disease. It is not, therefore, too much to require from those who are placed in circumstances which enable them to enter upon the enquiry with advantage, such a series of observations; and, if these are made with the requisite care and attention to particulars which all such enquiries demand, there can be no doubt but that valuable knowledge will be obtained.

*Dropsies.* Medical statistics are as closely connected with, and as capable of throwing light upon moral statistics, as other branches of statistical enquiry. They may, indeed, be frequently regarded as another mode in which the effects resulting from the operation of the various circumstances acting on population are expressed. Assuming our knowledge to be in a sufficiently forward state for the purpose, the prevalence of certain diseases tends to show the circumstances of the locality in which they occur; and, again, the knowledge of the circumstances of the locality affords an equal indication as to the conditions of disease which prevail therein. As a familiar illustration of this principle, we may refer to the existence of intermittents in marshy countries. The return of numerous cases of ague in a series of medical reports necessarily leads to the inference that the locality to which these reports refer contains a considerable portion of marshy land; while, on the other hand, if, in a topographical account, we are informed that the district is low, marshy, and abounding in stagnant waters, we equally infer that some forms of intermittent or remittent disease are necessarily prevalent there. The application, however, of this principle must, in the present state of our knowledge, be made with great caution. Perhaps there is no class of affections with which we are acquainted which has been considered to develop the moral state of a locality in which they are found to occur more than dropsies. The prevalence, for a series of years, of the various forms of dropsy has been thought to be a pretty sure indication that the population of the district are more or less given to intemperance, especially in the use of ardent spirits. Guided by this principle, we should perhaps infer, from the proportion of dropsical cases, that such was the case in the Land's End, the ratio of prevalence of dropsy in this locality being as high as 1 in 58; but Dr. Forbes observes, that both the mining and agricultural population, notwithstanding the statements of Polwhele and Dr. Borlase as to their former intemperate habits, "must now be considered as habitually sober, although," he adds, "they, unquestionably, occasionally commit excesses in this particular." (*Prov. Trans.*, vol. ii. p. 101.) This opinion, however, would seem to admit of some limitation, from what is stated in the continuation of this essay. "Dropsy," it is remarked, "appeared not to affect miners, or any one class of persons more than another; unless indeed we reckon *drunkards* as a class, and as the chief of them the landlords of public-houses; the greater number of whom here, as elsewhere, fall victims to their intemperance, of which some form of dropsy is the most obvious result; although itself generally a consequence of some other disease. The frequency of dropsy in this district seems also established by the mortuary registers both of St. Paul and St. Hilary, particularly of the former, in which it bears a proportion of no less than one in twenty of the total deaths." (*Prov. Trans.*, vol. iv. p. 201.)

The ratio of prevalence of dropsy in the Land's End is, as we have already stated, 1 in 58; in London, it is probably nearly the same; in Worcester, it is 1 in 82; in Bolton le Moors, 1 in 79; in Plymouth, again, where probably, from the number of sailors, there is a considerable consumption of spirits, it is 1 in 55; and in Bristol, also a sea-port town, it is 1 in 32. In Birmingham, the ratio is as low as 1 in 155; but, as the reports of Messrs. Parsons and Ryland refer only to the out-patients of the Infirmary, and dropsy is generally a very severe disease, and therefore more likely to be received within the walls of the institution, we can scarcely take this as affording even an approximation to a correct result. The low proportion in Worcester is more likely to be owing, at least in part, to the substitution of cyder, perry, or ale, for spirits, than to any superior morality of the population, though it is also probable that the manufactory of that city and its immediate vicinity, not requiring much expenditure of physical force, may also have a negative effect upon the habits of the people, in rendering them less inclined to excess in the use of intoxicating liquors than occupations which are more laborious. Anasarca appears to be decidedly the most frequent form of dropsical disease. Of 87 cases occurring at the Penzance Dispensary, during eight years of the seventeen over which the returns extend, 62 were cases of anasarca, 22 of ascites, and 3 of hydrothorax. Dr. Forbes is disposed to consider the proportion of anasarca as unusually large; and the results of the Plymouth Tables, furnished by Dr. Blackmore, tend to confirm the correctness of this opinion. Of 102 cases occurring at Plymouth, 56 were anasarca, 31 ascites, and 15 hydrothorax. In the Worcester

Reports, of the 65 cases entered in columns 1 and 2, 29 are recorded as anasarca, 12 as ascites, and 13 hydrothorax; the remainder being simply designated hydrops, or general dropsy. Of 57 cases included in the reports of the Worcester Infirmary for the years 1835, 1836, (*Prov. Trans.*, vol. v.), 26 are anasarca, 27 ascites, 3 hydrothorax, and 1 general dropsy; giving, of 110 cases occurring at Worcester classed under the heads of anasarca, ascites, and hydrothorax, 55 of the first or exactly one half, 39 of the second, and 16 of the third form of the disease. The following arrangement of these results exhibits the proportionate numbers of the different forms of dropsy in one hundred cases of the disease at the several localities referred to:

| | Land's End. | Plymouth. | Worcester. | Mean. |
|---|---|---|---|---|
| Anasarca . . . | 71.3 | 53.7 | 50.0 | 58.3 |
| Ascites . . . | 25.3 | 22.2 | 35.5 | 27.7 |
| Hydrothorax . . | 3.4 | 24.1 | 14.5 | 14.0 |

We cannot help adverting to the remark made by Dr. Forbes, in a preceding quotation, in regard to the liability of *Innkeepers* to dropsy, as we have always regarded the fate of this class of men as one of the most striking illustrations of the destructive influence of intemperance. During a long course of practice, we have been struck with the speedy and almost regular disappearance of one landlord after another from nearly all the smaller inns of an extensive district, including a large town and country population. All of these men, and most of them in the prime of life, have fallen victims to diseases originating in drinking; the temptation to this vice being almost always found too strong to be resisted by persons of this rank of life, when perpetually presented to them. We doubt not but this statement will be confirmed by all our provincial readers of much experience.

*Scrofula.* The proportion of glandular diseases appears at first sight to be much greater in the Land's End, where the ratio of prevalence is as high as 1 in 35, than in the midland counties, where it is no more than 1 in 90; but there is a source of fallacy here, which, although it prevents the attainment of any accurate conclusion upon the subject, it is necessary to take note of, lest the inspection of the tabular results before given should lead into error. In the Land's End Tables the section, *External Scrofula*, comprises 253 cases, including rickets, enlarged glands, diseased joints, and cutaneous ulcers; whereas, in the Worcester Tables, by the term *Scrofula* it is simply indurated, enlarged, and ulcerated glands which seem to be intended; amounting to 102 in the whole. In the third column, among the miscellaneous surgical cases, are included fifty-six cases of diseased joints, many of which were probably scrofulous, although no indication of their being of this nature is given in the Reports of the Worcester Infirmary, as published in the Midland Reporter, from which this column has been drawn up. Still the proportion of external scrofula is evidently greater in the Land's End district, a circumstance which may perhaps be in part accounted for by the greater prevalence of internal tubercular disease in the Vale of Severn.

> "It appeared to me," observes Dr. Forbes, "that the common external indication of the scrofulous constitution, visible in the complexion, texture of the hair, configuration of the body generally, and of the features of the face more particularly, were more marked among the inhabitants generally, and especially among the upper classes,

than in other parts of England, where I have had opportunities of observing the condition of the people; and I thought I could discover a larger proportional number of white swellings, incurvations of the spine, hip cases, and glandular tumours, there than elsewhere. Of the very general and great prevalence of these diseases throughout the district, the answers of the resident practitioners, under the heads *scrofula*, *rickets*, *white swelling*, *hip-disease*, *spine-disease*, are sufficient evidence; but then absolute proportional superiority in point of frequency, relatively to other districts, cannot be considered as established on the present evidence." (*Prov. Trans.*, vol. iv. p. 190.)

Dr. Forbes is inclined to attribute this prevalence of scrofulous disease, first, to the relaxing nature of the climate arising from its extreme humidity and equable temperature, and, secondly, to the frequent intermarriage of families, owing to the isolated character of this district.

"This latter circumstance," he continues, "is likely to operate in two ways; first, by generating the strumous diathesis in healthy subjects, on the well-known principle of breeding *in and in*; and, secondly, by tending to develop and strengthen the predisposition, when already existing. There is no fact better established in physiological pathology, than that the offspring of parents, both of whom are touched with any hereditary malady or morbid predisposition, are likely to exhibit the same malady or morbid predisposition in a greater degree than either of the parents." (p. 191.)

The low ratio both of external glandular disease and of internal tubercular disease (phthisis) in Van Diemen's Land, (that of the former being only 1 in 322, and of both together not exceeding 1 in 101,) is a curious and very interesting fact.

*Scirrhus and Cancer.* There are two circumstances attending the affections arranged under this head to which it will be right to direct attention, although the small number of cases recorded prevents our drawing any conclusions respecting them. The first of these is, their much greater prevalence in the Land's End than in Worcester; the proportions being, in the former case, 1 in 210; in the latter, 1 in 342: the other is, the almost entire immunity of Van Diemen's Land from affections of this nature; the number, in upwards of 30,000 cases, being only 4, or 1 in 7502.

*Diseases of the Brain and Nervous System.* These are much more prevalent in the Land's End than in the Vale of Severn; the total number in the former of these localities being 638, or 1 in 14.8; in the latter, 407, or 1 in 22.7. This section, however, embraces so many diseases differing in their nature, in the circumstances under which they are found to occur, in the classes of inhabitants which they severally affect, and in the causes to which they may be respectively attributed, that, without a more extended analysis of them than our limits will permit, it would be impossible to derive any advantage from their consideration.

*Diseases of the Heart and Blood-Vessels.* Considering the great exertions made by the miners when at work, and on their return from it in ascending the ladders to the surface with a considerable load upon their backs, and the prevalence of bronchitis, (especially the chronic forms,) of asthma, and of rheumatism, among them, we should have expected that organic disease of the heart would have proved of very frequent occurrence. The whole number of cases, however, reported in the Penzance Tables, amounts only to 78, affording a ratio of prevalence of 1 in 113; whereas, in Worcester, where the causes presumed to be influential in inducing changes of structure in the heart and larger blood-vessels do not seem to exist in such intensity, the number of cases returned in the various reports is 107, the ratio of prevalence 1 in 82. This difference, however, we are inclined to attribute, at least in part, to diseases of the heart being better understood and more readily recognized in the present day than formerly. The Worcester reports extend over a period of five years, from 1828 to 1832 inclusive: the Penzance Tables consist of the returns of three successive periods, the first two of which are three years each,—namely, from 1810 to 1812, and from 1819 to 1821, and the last eleven years from 1823 to 1833. In the first period only one case of diseased heart is entered, in the second period not more than four; so that in six years, which it should be observed were previous to the introduction of mediate auscultation, five cases only are recorded to have occurred; whereas, in the last period of eleven years, we have no less than seventy-three, or nearly the whole number. This statement accords with the opinions expressed by Dr. Forbes in the following passage:

> "I have no remark to make on this class of diseases, except to express my belief, founded partly on reasoning and partly on my own observation, that miners are more subject to organic changes of this organ, particularly dilatation, than most other classes of workmen. The causes of such a morbid state, in this class of persons, stand out prominently, both in their habits and diseases. The extreme strain on the respiratory and circulatory organs produced in the ascent from mines, described in the first part of this paper, and the chronic obstructions to the transmission of blood through the lungs, so frequent a form of disease in this class of persons, are strikingly calculated to produce these affections." (*Prov. Trans.*, vol. iv. p. 203.)

The practice alluded to in the foregoing extract, as to the mode of ascending from the mines, will be found described at length in the preceding part of the essay, (*Prov. Trans.*, vol. ii. p. 95,) and its injurious effects upon the organs of respiration and circulation forcibly pointed out in the letters from Dr. Wise and other practitioners inserted towards the conclusion of the second part. (*Prov. Trans.*, vol. iv. p. 221, *et seq.*) Dr. Wise thus describes this injurious custom:

> "Each miner having taken his portion of the blunted tools on his back, amounting in weight perhaps to from fifteen to twenty pounds, the most active generally takes the lead, and at whatever pace, is closely followed, along the horizontal galleries and up the perpendicular ascent of the ladders, by the rest of the party, not only from their desire to keep together, but to avoid a certain slur which would attach to any one falling in the rear. The rapid pace at which they usually set off soon hurries the circulation and respiration, and frequently to such a degree as to give rise to the most distressing feelings. At this time each would slacken his course, although none dare propose to do so; and thus they go on till they reach "grass," when they find themselves completely overpowered by the excessive actions of the heart and lungs, so as to be wholly unable for some time to speak." (p. 223.)

*Diseases of the Lungs.* The diseases of the organs of respiration require a more extended consideration, embracing, as they do, some of the most important deviations from health which can engage the attention of the profession. The severity and danger of the acute diseases of these organs, the intractable nature of many of the more chronic affections, the entire failure of the best directed efforts at cure in the vast majority of the most important of the whole, tubercular phthisis, are serious calls upon the humanity of the physician to devote his judgment, his skill, his most unwearied attention, his most unremitting industry, to the *investigation* of every point which is in any way calculated to throw light *upon this* class of diseases. If the position taken by Sir James Clark, in his admirable treatise on Phthisis Pulmonalis, be granted,—and we believe that few if any will be found to contest it,—that it is the prevention of the disease to which our efforts must chiefly tend, if we would obtain any measure of success against this insidious destroyer, it is equally true that the developing of the external causes which exert an influence in its production must be an object of the highest practical importance. We feel convinced that much light will be thrown upon these causes, and their mode of operation in inducing the deposition of tubercles, not only in the pulmonary tissues, but in other textures and organs of the body, by the study of the varying conditions of the different localities in which they are found to prevail, as compared with those, if such there be, which enjoy a comparative immunity from them. In this point of view, statistical and topographical details are of the highest utility: on the present occasion, we regret that the documents before us, although highly valuable, are much less comprehensive than we could desire.

The total number of diseases of the lungs in the two localities of Worcester and the Land's End is 3086; the total number of cases of disease in the two districts, 18,082, giving 1 in 5.8 as the general ratio of prevalence. In the former district there are 1616 cases of diseased lungs returned, affording a ratio of 1 in 5.7; in the latter district there are 1470, affording a ratio of 1 in 6. The ratio of prevalence being so nearly the same in each district as the general ratio of the two together, any disproportion in the ratios of prevalence of particular forms of pulmonic disease becomes the more striking, as showing a positive tendency existing in one or other of the localities, independent of many sources of error, which under other circumstances tend to complicate and obscure conclusions of this kind. The following table shows the ratio of prevalence of phthisis, as compared with diseases of the lungs, and with diseases in general in these districts, and in some others:

| | Cases of Phthisis. | Cases of Diseases of Lungs. | Ratio of Phthisis. | Total Cases of Disease. | Ratio of Phthisis. |
|---|---|---|---|---|---|
| Land's End and Worcester, | 713 | 3086 | 1 in 4.3 | 18082 | 1 in 25.3 |
| Worcester . . . | 416 | 1616 | 1... 3.9 | 9255 | 1... 22.2 |
| Land's End . . . | 297 | 1470 | 1... 4.9 | 8827 | 1 .. 28.7 |
| Plymouth . . . | 176 | ... | ... | 5648 | 1... 32.0 |
| Birmingham . . | 295 | ... | ... | 18663 | 1... 63.9 |
| Hobart Town . . | 202 | 2502 | 1... 12.3 | 30008 | 1... 148.5 |

From the foregoing tabular view, it appears that tubercular consumption including hæmoptysis, (which is in nearly every instance either a symptom merely of phthisis, or at least accompanied with and resulting from the presence of tubercles in the pulmonary organs,) is more prevalent in that part of the Vale of Severn of which Worcester is the centre than in the Land's End, or indeed any other of the localities included in the table. This is a remarkable fact; as, from what has been before observed with respect to scrofula, to which tubercular consumption must be considered as nearly allied, it appears that the causes of tubercular depositions, and of that variety of constitution in which they are found to occur, are perhaps to the full as prevalent in the one locality as in the other. The greater prevalence of the external forms of scrofula in the Land's End district would, indeed, probably more than counterbalance the difference existing between the two localities in regard to the internal forms of the disease, and more especially phthisis. This, however, is not the case with the town of Birmingham, in which the ratio of prevalence of phthisis, as compared with other diseases, is as low as 1 in 63.9; for the proportion of external scrofula is not more than 1 in 162; a much lower ratio than either at Worcester, where it is 1 in 90, or in the Land's End, where it is 1 in 35; so that, in this large manufacturing town, notwithstanding the close crowding of the population and the nature of their avocations, there would appear to be a much less degree of prevalence of this fatal disease than in almost any other part of the kingdom. This is a statistical fact of vast import, if a more extended survey of the conditions of disease should confirm its accuracy; and we would here call the attention of the profession in Birmingham to the subject, with the view of inducing some of its members to undertake the labour of drawing up a full topographical account of that town and its immediate neighbourhood, with an especial reference to the degree of prevalence of the different forms of tubercular disease. It is very possible that some peculiar causes may have unduly modified the results of Mr. Parsons: at least, we find a very different result in a report by Dr. Ward in a subsequent volume of the Transactions, (*Prov. Trans.*, vol. v. p. 407,) viz. only 1 in 10. It is proper to observe, however, that the numbers included in Dr. W.'s report were very few in number (585) compared with those in the Tables of Mr. Parsons.* The proportion of phthisis in London is even greater than at Worcester, amounting to 1 in 19.7. In Plymouth also, according to Dr. Woolcombe, it is as high as 1 in 19.6: this ratio, however, it will be observed, is at variance with that obtained from Dr. Blackmore's Tables, which give the proportion of 1 in 32. The ratio of prevalence of scrofula in the same town, according to Dr. Blackmore, is 1 in 73. In Bolton le Moors, according to Dr. Black's Tables, (*Edin. Med. and Surg. Journ.*, vol. xxv. p. 444,) it is 1 in 21.9; being somewhat higher than at Worcester, though not equalling the rate of prevalence in London.

A due consideration of the preceding details will, we think, induce the physician to pause before he recommends the town of Penzance, and possibly other parts of the southern coast, as a residence for phthisical patients; for, however the equable nature of the climate may in some respects be suited to the consumptive invalid, the propriety of placing those who are either already affected with, or in any way predisposed to, tubercular phthisis in a locality where the disease is itself so prevalent, and the tendency to it so strongly marked, must be very questionable. Van Diemen's Land would seem to be a much more eligible situation to which those who are of a consumptive tendency might be sent; for the comparatively small number of cases of the disease, (only 202 out of upwards of 30,000,) and the low ratio of prevalence, not only with respect to diseases in general but, what is perhaps in this case a much more severe criterion, with regard to diseases of the pulmonary organs, is a fact worthy of attention, and one which speaks very favorably for the climate of this rising colony.

Sir James Clark estimates the relative mortality from this disease, both in

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* Mr. Parsons' early death is a loss, not only to Birmingham but to the profession of which he was so valuable and esteemed a member. He was equally distinguished for his talents and moral worth.

France and England, at 1 in 3; Dr. Forbes estimates it, for the Land's End alone, at 1 in 4.3. At Carlisle, it is 1 in 7.5; at Plymouth, according to Dr. Woolcombe, 1 in 4.1, according to Dr. Blackmore, 1 in 6; giving as a mean for this locality 1 in 5. At London, it is 1 in 4.2; at Birmingham, according to Mr. Parsons, as deduced from observations for the years 1833, 34, and 35, 1 in 5.3. At Malvern, according to Mr. Addison, of 33 deaths, 7, or 1 in 4.7, were from phthisis. This gives a general average of 1 in 5.16, instead of 1 in 3, so that Sir J. Clark's estimate, at least as respects England, is probably too high. It is perhaps worthy of notice that, of these six localities, the ratio of mortality from phthisis is less than the mean at Carlisle and Birmingham, that is, in the two which are situated more to the north, and which may therefore be presumed to have a colder and more rigorous climate; while in the warmer climates of London, Plymouth, and the Land's End, it is greater than the mean. How far extraneous circumstances may have operated in the introduction of error into these conclusions we are not in a condition to form an opinion; but, of 155 deaths which occurred at the Worcester Dispensary, during the years 1828 to 1831 inclusive, 62, or 1 in 2.5, were from phthisis, (*Midland Medical Rpeorter*,) which proportion, if taken into the general account, would give a mean ratio of 1 in 4.75, instead of 1 in 5.16. This, however, is obviously an erroneous estimate of the relative mortality from phthisis, as compared with other diseases for Worcester, since it is evident, from a comparison of the columns 2 and 3 of the table given at page 8, that some regulation as to the admission of these cases exists at the Infirmary of that city, and that, therefore, a greater number than the just proportion are admitted at the Dispensary: the relative mortality from this cause at this last institution is therefore probably higher than the true ratio for the city and its immediate neighbourhood. The number of cases denoted *phthisis* admitted at the Worcester Dispensary during this period, (from 1828 to 1831 inclusive,) was 235; the number of deaths from this cause 62, or 1 in 3.8: the proportion of deaths to the cases at Birmingham during the years 1833, 34, and 35, was, according to Mr. Parsons, 1 in 1.38; a difference which leads to the supposition that either many cases of tubercular phthisis in its earlier stages are classed under other affections at the Birmingham Infirmary, (a supposition which is supported by the number of cases of hæmoptoe which occur in the tabular returns of that Institution), or that some of those which have been so classed in the tables of the Worcester Dispensary were not the true tubercular consumption, but some other form of chronic pulmonary disease.

Bronchitis, catarrh, and other affections of the pulmonary mucous membrane form a large proportion of the diseases to which the inhabitants, both of the Land's End and of the city of Worcester, are liable. This is probably to be attributed, in part at least, to the humidity and relaxing nature of the climate; but, however a predisposition to these diseases may be thus acquired, the inhalation of particles of dust, forming a continuous source of irritation to the delicate membrane lining the bronchial tubes and cells, must, in either of these situations, be considered as a chief exciting cause in the production of this class of affections. The glovers of Worcester, no less than the miners of Cornwall, are constantly breathing an atmosphere loaded with the fine particles resulting from the mechanical disintegration which takes place in many branches of their respective employments, and the effects of such a practice are too evident to require here to be pointed out. It may not be uninteresting to compare the proportional prevalence of bronchial disease with that of tubercular phthisis, and we therefore subjoin a tabular arrangement, similar to that which we have already given of this latter disease.

| | Cases of Bronchitis, &c. | Cases of Diseased Lungs. | Ratio of Bronchitis. | Cases of General Disease. | Ratio of Bronchitis. |
|---|---|---|---|---|---|
| Land's End and Worcester, | 1337 | 3086 | 1 in 2.3 | 18082 | 1 in 13.5 |
| Worcester . . . | 736 | 1616 | 1 ... 2.2 | 9255 | 1 ... 12.5 |
| Land's End . . . | 601 | 1470 | 1 ... 2.4 | 8827 | 1 .. 14.6 |
| Plymouth . . . | 319 | ... | ... | 5648 | 1 ... 14.5 |
| Birmingham . . | 3541 | .. | ... | 18663 | 1 ... 5.3 |
| Ditto, excluding 684 cases of influenza | 2857 | ... | ... | 18663 | 1 ... 6.5 |
| Hobart Town . . | 1784 | 2502 | 1 ...1.4 | 30008 | 1 ... 16.8 |

A very curious, and (if at all approaching to correctness) a very important feature in the results exhibited in the preceding table, is the close approximation of the ratios of prevalence in four out of five of these localities. The mean ratio of prevalence of these four is 1 in 14.6, or precisely that of the Land's End. Now, the only circumstances which, as far as we know, are common to three of these localities,—namely, the Land's End, Plymouth, and Worcester,—appear to be a tolerably equable temperature, and an atmosphere frequently charged with humidity; and which, therefore, however they may seem to dispose to catarrhal diseases, as appears from the great prevalence of this class of affections in such situations, may yet probably exert a counteracting effect upon other causes presumed to be influential in inducing disease of the mucous membranes; as, for instance, upon the irritation arising from the presence of particles of dust in the respired atmosphere. That such may be the case seems to be rendered still more probable by the extremely high ratio of prevalence of bronchial diseases in the town of Birmingham, where they constitute 1 in 5.3, or, omitting the cases of influenza, 1 in 6.5, of the whole number of diseases. No doubt this high proportion is owing, in part, to the still more irritating nature of the particles of dust with which the air in many of the manufactories is charged, consisting of fine metallic particles, exerting both a chemical and mechanical effect upon the secretions and organization of the delicate membrane with which they come in contact; whereas, in Worcester, the dust consists chiefly of the finer particles of leather and minute angular fragments of the pumice-stone used in the grounding of the skins; and, in the Land's End, of particles of earth and ore, resulting from the operations of blasting, picking, &c. carried on in the mines. This cause, however, independent of any variation in the mechanical or chemical nature of the particles of dust, must be materially lessened by a humid state of the atmosphere, and may therefore be expected to exercise a more deleterious influence in the dry, heated air of the manufactories of a situation exposed to no extraneous sources of moisture, like Birmingham, than in one in which the air is preserved in a state of almost constant humidity, as occurs in the mines of Cornwall. We must still, however, bear in mind that both the Cornwall and Worcester ratios are calculated from a series of cases which embrace a considerable proportion of the agricultural population; while the ratio deduced from the Birmingham Reports refers to a population almost exclusively manufacturing. One other source to which the difference may possibly in part be owing requires to be noticed, and that is, the probability that many of the cases arranged as chronic bronchitis and catarrh in the Birmingham Tables may have been cases of tubercular phthisis; a supposition to which the extremely low ratio of prevalence of this latter disease, as deduced from the same tables, gives considerable support.

The length to which our observations have extended precludes us from extracting any detailed account of the differences which may exist in the nature of the phenomena presented by these affections as they occur in the several localities. The cases at the conclusion of the paper on the Land's End, in the fourth volume of the Transactions, may however be referred to for an illustration of the principal varieties of the *Miner's consumption*, as the disease is there emphatically called.

*Diseases of the Abdomen.* Many of these afford room for observation; but we can only remark at present that the difference in the Land's End and Vale of Severn, in respect to the cases of dyspepsia and hepatitis, which nearly compensate each other, are probably to be explained by the different views which different practitioners take of this class of diseases; some including under the term dyspepsia cases to which others, from a tender state of the epigastric region, combined with evidences of derangement in the hepatic functions, would probably give the name of chronic hepatitis.

The large proportion of cases marked diarrhœa, cholera, and dysentery, in the Worcester Tables, arises from the cases of the epidemic cholera being included, and not from any real excess of disease of that description. Taking the ratio of prevalence afforded by the Dispensary Tables from 1828 to 1831, which was previous to the appearance of the epidemic, we have the proportion of 1 in 31.4; and, making allowance for the cases of malignant cholera and epidemic diarrhœa included in the Penzance Tables, which the subsequent remarks of Dr. Forbes (*Prov. Trans.*, vol. iv. pp. 180, 181,) enable us to do, we obtain a proportion very nearly the same.

*Diseases of the Urinary Organs.* There is a much larger proportion of these diseases in the Penzance Tables than in those of the Vale of Severn; the ratio of prevalence in the former case being 1 in 83; in the latter, at Worcester and its vicinity, only 1 in 185. In Bristol, of 700 medical cases, there were only 6 of disease of the urinary organs, or 1 in 116.6; and, in Malvern, only 2 out of 578, or 1 in 289. The cause of this does not appear, but the cases are too few to derive any positive inferences from.

*Uterine Diseases.* Here, again, there is a ratio of prevalence in the Land's End district which differs greatly from that of Worcester and the neighbouring country; the respective proportions being in the Land's End 1 in 14.1, in Worcester only 1 in 39. At Bristol the proportion, as deduced from the 700 cases before referred to, is 1 in 26.9, and at Malvern as high as 1 in 13.7. Both these latter proportions are, however, obtained from selected cases; the ratio of Bristol, as has been already remarked, including only the medical cases, and the ratio of Malvern omitting the slighter cases, as well as those more strictly surgical. Dr. Forbes makes the following observations with respect to this subject:

> "The only diseases of this class in the Dispensary Tables that require notice are those comprehended under the head of chlorosis and amenorrhœa. It will be observed that the proportion of cases so denominated is extremely great, being, in fact, nearly three times as great as in London, and more than one-half more than in Plymouth, It will also be observed, that the disproportion is entirely owing to the very great number of cases thus distributed in the first column of the table. Taking the two last columns only, we find that the proportion of such cases (even when those classed under dysmenorrhœa are included) to the total diseases, during the same period, instead of being 1 in 21, is only 1 in 25, or, excluding dysmenorrhœa, 1 in 30. Still even this is a very large proportion, and I can only account for it by supposing that every case presenting any deficiency in the catamenial discharge was arranged under this head, without considering how frequently the amenorrhœa is a mere symptom of other diseases. I am borne out in this supposition by the result furnished by the second column, which gives the proportion only as 1 in 52. I do not doubt but that, if as rigid analyses had been instituted during the other years as was done by myself in classifying the cases during these three years, the general result would have approached nearer to that last stated. It is, however, not improbable that these affections are more prevalent in this district, although not to the extent alleged; and that this increased prevalence may be owing to the greater amount of dyspeptic affections, of the more inveterate forms of which amenorrhœa is often a consequence." (*Prov. Trans.*, vol. iv. p. 212.)

*Chronic Eruptions.* The chronic eruptive diseases constitute another series of morbid affections, which are proportionally more prevalent in the Land's End. These are attributed—and justly so, we have no doubt,—to defective nutriment and clothing, and general want of cleanliness, on the part of the inhabitants. The ratio of prevalence is 1 in 16.9; whereas, in Worcester and its vicinity, it does not amount to more than 1 in 40. The proportion in Van Diemen's Land is almost exactly double; being 1 in 20.5.

*Bronchocele.* This is another disease to which we must briefly allude, as it is one which has been supposed to be particularly prevalent in the midland counties. At Worcester, the number of cases reported is 47 out of 9255, giving the proportion of 1 in 198; in the Penzance Tables we have only seven cases, or 1 in 1261. The ratio of prevalence, 1 in 198, must be very considerably below the true ratio, as, from the trifling inconvenience experienced from this affection in its earlier stages and slighter forms, it is probably only the more urgent cases,—those in which either great deformity or some derangement of health results,—which become the subjects of treatment at public institutions.

We must not bring these observations to a close without directing attention to the excellent Statistical Observations on the Medical Charities of England and Ireland, by Dr. Walker, of Huddersfield, which deserve a fuller and more attentive consideration than we could possibly devote to them. For similar reasons we are obliged to pass over some other papers which require notice, but we cannot omit to refer to Dr. Black's paper on the Medical Topography of Bolton le Moors and its Neighbourhood, in the last volume of the Transactions, as possessing peculiar claims upon the attention of our readers. It is to be regretted, however, that the author of this excellent statistical sketch has not entered upon the more strictly medical part of it to an extent commensurate with its other portions, and especially that he has omitted to subjoin tables of disease. Should he have it in his power to rectify these omissions upon some future occasion, he will materially add to the value of what he has already written.

In conclusion we must observe that, although we could point to some papers in these volumes hardly suited to the object of the Association, or worthy of its reputation, we regard the majority of them as calculated at once to do honour to and to elevate the character of the provincial practitioner: they are not of the mere passing interest which is likely to excite the attention of the superficial reader, but require a deeper insight into the philosophy of medicine than either suits the purposes or is agreeable to the taste of the literary charlatan. To such an individual we can readily believe that the study required to develop the true principles of science must prove a wearisome and distasteful task: he cannot appreciate, because he is, both morally and intellectually, incapable of comprehending the solid truths upon which alone the superstructure of real, substantial knowledge can be erected.

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## ART. II.

*Mémoires de la Société Médicale d'Observation de Paris.* Tome Prem.
—*Paris,* 1836.
*Memoirs of the Medical Society of Observation, of Paris.* Vol. I.
—*Paris,* 1836. 8vo. pp. 495.

IN our last volume (No. IX. p. 154,) we gave a general notice of this volume, and of the principles of the Society from which it emanates. Opposed as we there showed ourselves to the inconsiderate extension of the numerical method to the practice of medicine, we bore testimony to its great value as applied to those departments of the science which rigidly admit of calculation. As further evidence of the sincerity of our opinion, we have carefully analysed the three principal memoirs in the volume, in order that we may make our readers acquainted with their very important contents. We think that, if any proofs were wanting of the benefit conferred on medical science by the more rigid enforcement by M. Louis of the inductive method in medico-statistical enquiries, the memoirs now before us would supply them.

### I. *Recherches sur l'Emphyseme des Poumons.* Par M. LOUIS. (*Researches on Emphysema of the Lungs.* By M. LOUIS.)

ALTHOUGH these Researches bear the name of M. Louis, they are not, as he himself informs us, founded on observations exclusively his own. A share of the cases was furnished by the late Dr. Jackson, of Boston, to whose brief and laborious career we have already alluded in this Journal as affording a strong illustration of the power possessed by Louis of infusing a portion of his zeal into those with whom he is brought into contact. That there are "minds fitted," as an eloquent writer expresses it, "to make their spirit pervade and elevate the smaller minds around them," none can doubt who have had an opportunity of visiting the wards of Louis. It is his example and influence that explain the patient toil with which numerous young men there pursue what must be admitted to be, unless with a view to its ultimate utility, the most irksome of occupations—bedside observation. But it may be said there is nothing uncommon in this, and that all men, separating in their doctrines from the multitude, have had ardent disciples among the young. True: but there is a wide difference between the sacrifice of personal ease required in practically espousing the method of Louis, and that of any other great medical innovator. Let us take, for example, the case of Broussais. We do not deny the extent to which his dogma found favour among medical men, especially in France, or the species of *acharnement* with which it was defended; but this attachment is easily explained by the encouragement doctrines like his give to indolence, and by the simplicity of the practice taught in them, which saves its disciples from the annoying process of reflecting on the nature of the cases submitted to them for treatment. There is no need to turn for its explanation to the influence of a master intellect, or to devoted love of science and abstract truth: but how else can the ardour with which Louis' method is followed up be accounted for?—a method in which that mental exercise of all others most pleasing to the young, indulgence in hypothesis and speculation, is as it were proscribed; in which the labour of hours is sometimes required to substantiate a single fact, while that fact is in itself looked on as valueless unless compared with a long series of similar ones!

Whether considered with regard to its gravity and frequent occurrence or to the interest attached to the rise and progress of our knowledge respecting it, Pulmonary Emphysema claims a prominent place in the catalogue of human diseases. Originally mentioned, in so far as its anatomical characters go, by Bonetus, Morgagni, Ruysch, and our own admirable Baillie, it was never methodically studied until Laennec described its symptoms, established its diagnosis, and paved the way for its acquiring the nosological status it now unquestionably holds. Created as its history was by that great observer, in everything relative to this affection we stand on ground almost peculiarly French. Yet even in France, it is far from being generally studied with the care its importance calls for. For while, in the wards of Louis, its symptoms and constituent lesion are examined and demonstrated with the minute accuracy belonging to his school, there are some hospitals in Paris where its name is never mentioned, and others where its very existence is steadily denied. It shall be our business to extract such particulars from the mass of evidence in the conclusive memoir, whose title heads this article, as shall convince the few who remain incredulous that pulmonary emphysema is indeed an entity.

The number of cases analysed in M. Louis' essay amounts to ninety. Of these forty-two relate to subjects who died in the hospital; the remainder to individuals who left it more or less relieved. Twenty-three of the fatal cases occurred from cholera.

The general description of the disease, defined by our author "a dilatation of the pulmonary vesicles," is a summary of the various facts dwelt upon at large in the pages that follow. It is therefore unnecessary for us to extract any part of it, but we take the opportunity of pointing out in it a model of the style in which a general description should be drawn up. Concise without obscurity, it contains everything important, and is loaded with none of that irrelevant matter in which systematic writers are so prone to indulge.

The extent to which the lungs were affected varied considerably in M. Louis' cases, as the following table shows:

| | |
|---|---|
| General emphysema of both lungs in | 12 cases. |
| — — of the left lung | 2 |
| — — right ditto | 1 |
| Emphysema of left upper lobe | 2 |
| — right inferior ditto | 2 |
| — of middle two-fifths of the right lung | 1 |

These calculations refer to the patients who died of other affections than cholera. In the victims of that epidemic, in whom the extent of the emphysema is also tabularly given, it was general in one-fourth of the cases; whereas, as appears from the above table, two-thirds of the other patients had both lungs emphysematous: this difference is to be explained by the facts that the mean age of the former series of subjects was only fifty, of the latter sixty; and that the extent of emphysema, as of chronic affections generally, is proved to be in the direct ratio of its duration.

M. Louis concludes that "very probably" both lungs are equally subject to dilated vesicles, and that in both the lesion is disposed to attain the same degree of development. He is borne out in this conclusion as regards the *entire* lung by the table from which he draws it; but it ceases to be true if applied to the corresponding parts of the two lungs.

Thus we find emphysema of the left inferior lobe noted in fifteen cases, of the right inferior in two only. And, by adding together the *partial* and *general* cases for each lung, we find forty-seven the total for the left, and thirty-four for the right. Where the elements for calculation are so limited, this surely is no very trifling difference. The difference in the comparative frequency with which the inferior lobes on both sides are attacked is extraordinary enough, yet M. Louis indulges in no comment upon it.

Whatever be the degree of dilatation of the vesicles it is never uniformly the same in the different parts of the lung. The internal surface and base of the organ usually present the largest cells; and in fifteen cases in which their relative dimensions were carefully noted, those of greatest size were found at the acute border. From this fact M. Louis, as we shall learn by and bye, draws a pathological inference of primary importance.

Our author frequently alludes to certain *appendices*, as he names them, probably answering to the sub-plural vesicles of Dr. Stokes, which constitute the most advanced stage of the emphysematous lesion. The post-mortem details of the first case reported in his essay contain a very close description of these.

> "The form of the *appendice* was renal, its length four inches, its breadth one and a half. When dried and cut across at an inch distance from each of its extremities its structure was found to be as follows: one of the sections presented, at its centre, vesicles smaller than a millet-seed, circumscribed by transparent and extremely thin septa; these cells increased in size the nearer the surface they were examined. Here they were as large as a hempseed, or larger, their shape irregular, oblong, and, as it were, denticulated. The other segment of the *appendice* was somewhat larger,

contained three principal cavities of the size of a nut, traversed by cellular filaments; besides smaller cells separated by lamellæ of exceeding thinness, shining like the coats of an onion. They were from three to four lines long, by very nearly the same breadth and crossed by several vessels, scarcely to be recognized by their colour." (p. 175.)

In this case, although the disease existed to a very marked extent, the difficulty of breathing was far from excessive.

In thirteen cases the bronchi were carefully followed to their terminations, and in four instances only were they found dilated. The smallness of this number of dilatations excludes, according to M. Louis, the idea of community of affection between those tubes and the vesicles, inasmuch as "it is perhaps not greater than that existing under different circumstances in subjects of the same age as those in question." We should have been inclined to consider it materially greater, but even if we were correct in this supposition, and we admit we are without the only true elements of a positive decision, namely, the examinations of the bronchi in a large number of patients dying of various diseases; still the following facts, brought forward by our author, prove that the bronchial and vesicular dilatations are in his cases independent of each other. First, the dilatation of the bronchi was confined to a small extent of lung, where that of the vesicles was general. Secondly, in two cases in which the disease had advanced to the formation of *appendices* the bronchi were not dilated; in one of them they appeared even of smaller caliber than natural. Thirdly, when both lungs were emphysematous, the dilatation of the bronchi was almost always confined to one side. Fourthly, though the vesicles were most enlarged near the acute border the bronchial dilatation did not affect any preference for that part of the lung.

The pleura was affected with adhesions in thirty out of thirty-six cases; but our author is induced to deny any influence to the emphysema on the development of these lesions on several grounds, which we shall give in his own words.

"In the first place universal adhesion of both lungs did not exist in a single case, and general adhesions of one side were only found in fifteen individuals. In the others they were confined to a single lobe and frequently to an extremely limited portion of the pulmonary surface. Hence, as regards pleural adhesion, emphysematous patients are found to be almost similarly circumstanced to non-tuberculous subjects unaffected with emphysema. To this must be added that the individuals whose cases are under consideration, were generally of a more advanced age than the aggregate of those who die of any given disease. This fact accounts for the trifling difference observed, for the general causes of pleurisy had been longer in action in the former than in the latter subjects. Moreover the lungs were totally free from adhesion in two of the three patients in whom the emphysematous disease had reached its highest point; and the pleural false membranes, when partially spread over the lung, were found, as is the ordinary case, *posteriorly*, even though the vesicles were most largely dilated at the anterior part of the lungs." (p. 179.)

Tubercles or grey granulations were found in the lungs in a smaller proportion of subjects affected with emphysema than the mean number found in individuals dying from various other diseases: this result is corroborated by many minor facts brought forward in the memoir.

We fully agree with M. Louis in the justness of the inferences he draws from these positions. They prove that pleuritis, tubercles, and emphysema do not stand to each other in the relation of cause and effect. They are also a pointed illustration of the capital importance of studying the state of every organ in large masses of subjects dying of all varieties of disease, before we can presume to decide on the reality of a supposed connexion between any coexisting lesions.

The heart was hypertrophous in sixteen cases. It had acquired double its natural size in one, and exceeded that degree of enlargement in two subjects. Six of these cases occurred in the twenty-three cholera patients, ten in the nineteen remaining subjects. It would seem natural to ascribe this difference simply to that in the mean ages of the two series of individuals. Nevertheless, M. Louis conceives it in some measure attributable to the emphysema "because the excess in the heart's size was much less in the cholera patients, in whom the dilatation of the vesicles itself was generally less advanced, than in the other subjects." Our author does not inform us which side of the organ was principally affected or whether both were equally so.

The second section relates to the symptoms. Dyspnœa, the most constant of these, as it existed in forty-two out of forty-four cases, first occupies M. Louis' attention. We may observe that in the two exceptional cases, in which neither the reality of the emphysematous disease nor the absence of dyspnœa can be questioned, there was no habitual pulmonary catarrh. Our author is disposed to believe that it is only in cases where the affection is of short standing that dyspnœa is wanting; an idea which to a certain extent is in opposition with the fact that atrophy of the vesicles, the most advanced stage of the lesion, coexisted in a case above alluded to with easy respiration. The difficulty may, it is true, be got over by supposing a very rapid development of the disease; but this, as we shall see, is, at the least, a rare occurrence.

M. Louis' details on this single symptom fill six pages. The following is an outline of the principal facts contained in them. 1st. The oppression was persistent in every instance, except in one individual aged eighteen, who was not subject to cold. The physical signs of the disease were all of them clearly made out both during and after the paroxysm of dyspnœa for the relief of which this patient came to the hospital. 2dly. The period of origin of this symptom varied exceedingly. In sixteen subjects it existed from the earliest infancy. In the others it commenced as follows: before the twentieth year in two patients, from the twentieth to the thirtieth year in seven, from the thirtieth to the fortieth in seven, from the fortieth to the fiftieth in eight, and from the fiftieth to the sixtieth in three. No cases were met with after that age, which seems to show that the *formation* of emphysema is scarcely to be feared at so advanced a period of life, though its *increase*, when previously developed, is then frequently met with. 3dly. The difficulty of breathing was usually slight in the outset and increased in some cases uniformly, in others irregularly, at a period more or less distant from its commencement. 4thly. In the majority of cases, thirteen out of seventeen, this increase in the dyspnœa ushered in another change, viz. habitual dyspnœic [asthmatic] paroxysms. These exacerbations were generally mild in patients whose dyspnœa commenced in early youth. 5thly. In many cases the exacerbations came on without apparent cause; they were, however, usually induced by an attack of acute pulmonary catarrh. 6thly. It is not known if atmospheric variations have any influence on their production; but if they have such influence, it is certainly not constant in its action. 7thly. The dyspnœa of emphysema is in itself alone exceedingly characteristic of the disease. It could not, in M. Louis' cases, be due to habitual bronchitis,—because that affection did not exist in every case, and because it only commenced in infancy in one solitary instance; nor to dilatation of the bronchi, as the dyspnœa, which is sometimes observed in that affection, is slight and not attended with exacerbations; nor to disease of the heart,—because in the cases in which a morbid state of that viscus was observed, far from having originated in youth, it had only existed a few years; nor to phthisis,—on account of the absence of hæmoptysis and other tubercular symptoms.

It is plain that the differential diagnosis is not in the last case, at least, founded singly on the character of the dyspnœa; and the following facts, discovered by the late Dr. Jackson, while they prove the vast diagnostic value of the symptom, show that it cannot be considered perfectly pathognomonic of emphysema. Of one hundred and twenty subjects, the state of whose respiration was carefully ascertained from infancy upwards, twenty-eight had been subject to dyspnœa from that period: of these twenty-eight individuals, one was affected with cardiac disease, two with phthisis, *twenty-five* with pulmonary emphysema.

There is a point of some importance noticed relatively to a case introduced in this part of the memoir. The subject of it had intercurrent pneumonia. After the resolution of the inflammation, there was no sensible increase in the dyspnœa symptomatic of the preexisting emphysema. "Hence it appears," says the author, "that we cannot with certainty of being correct attribute emphysema supervening shortly after pneumonia to that disease," (p. 192.)

The alterations produced in the form of the chest by dilatation of the vesicles, are among the most remarkable effects of the disease. In one case the entire thorax was altered in shape, having acquired a globular form. This general prominence depended at once on the altered position of the ribs "and of the intercostal spaces *which were not sunk*, as is, especially in thin subjects, their natural condition," (p. 195.) The change of form in the remaining cases, thirty-six out of thirty-seven, the only ones in which it was examined, was of precisely similar nature, but only partial. It usually extended from the clavicle to or even beyond the mamma, and measured from three to four inches in width. The great diagnostic importance, in M. Louis' opinion, of this prominence will appear from the following considerations: It cannot be mistaken for a congenital malformation, *because the ribs and intercostal spaces concur equally in its formation;* nor confounded with the result of pleuritic effusion, because in this the dilatation is general, and more marked inferiorly than superiorly; nor with an aneurism of the aorta, because the prominence produced by that affection is more circumscribed and more elevated. It might be confounded, on simple inspection, with the arch caused by effusion into the pericardium or hypertrophous heart, but percussion would of course at once remove all chance of error.

According to Dr. Stokes, "the intercostal spaces in this disease *are deeply marked, and present no indication of protrusion.*" Here is a direct contradiction of the result of Louis' experience. We acknowledge our inability to reconcile statements so utterly opposed to each other: but, in justice, we must add that our own acquaintance with the signs of the affection induces us to believe M. Louis correct; and we are confirmed in this idea by the results at which M. Woillez arrived in examining a collateral question. In the essay of that gentleman on the physiological and pathological prominences of the chest, founded on the careful measurement of one hundred and sixteen thoraxes, we find the following proposition. " If there exist a *physiological* prominence at a given point of the chest, and the part of the lung answering to it grow emphysematous, that prominence may be considered to have become *pathological* as soon as the *intercostal spaces are effaced;* but previously, while those spaces are still distinct, the elevation cannot be confounded with those produced by emphysema," (p. 53.)*

The prominence described was not observed with equal frequency on both sides of the chest. It occurred in eleven cases on the right side, in twenty-three on the left. This result, as the reader will perceive, is in opposition with a statement already made to the effect that the frequency and degree of the disease are equal on both sides. M. Louis endeavours to escape this difficulty, by supposing the inequality as to change of form only temporary, and likely to disappear in proportion as the development of the disease advances. The fatal cases do not, as far as we can perceive, favour his hypothesis.

A far more plausible solution of the difficulty has been proposed by M. Woillez. It appears, from the numerous measurements of that gentleman, that *physiological* prominences of the anterior surface of the chest are almost always found on the *left* side, and are tolerably frequent there. " Are we to believe then," he asks, " that the *morbid* prominence of emphysema is, in the outset, frequently *physiological;* or that the unknown cause, which favours the formation of physiological elevations on the left side, acts also on that of the pathological dilatation of the thorax produced by emphysema?" (*Op. cit.*, p. 37.) We should be inclined to go even further, and hazard a conjecture that some of the *left* prominences, observed by Louis, were solely physiological, and uninfluenced by the subjacent pulmonary lesion. An evident consequence of M. Woillez's discovery is, that, though occurring much more frequently at the *left* side, the prominence in question is of much more value, as a diagnostic mark of emphysema, when found on the *right.*

It is clear that explanations such as those suggested above, need only be had recourse to if the presumed equal liability of both lungs to the disease really exist. Now we have shown that, even from the evidence of M. Louis' cases, it is far from being satisfactorily proved to do so. And it is curious that the quantities representing, on the one hand, the number of times each lung was affected, and, on the other, the number of times each side was partially dilated, namely 47, 34, 23, and 11, are extremely close to the true proportionals as 47 : 34 : : 23 : $16\frac{30}{47}$.

The back of the thorax was only examined in six cases,—evidently an afterthought,—and in three of these, a partial dilatation was discovered. We can scarcely understand how, in accordance with his principles, M. Louis justifies himself in concluding, as he does, from data so limited as these, that "the emphysematous prominence appears to affect a preference for the anterior surface," (p. 196.) He tells us, it is true, that such a fact would be in harmony with what has been proved respecting the anterior border of the lung being the usual seat of the largest vesicles. No doubt it would. But is this *direct observation?* Would not M. Louis reject with disdain any such argumentation on the part of another in favour, for example, of the inflammatory origin of tubercle? We think he would,—we are certain that he ought. The researches of M. Woillez have, as it happens, shown that M. Louis' *à priori* notion was correct, but he cannot defend his departure from his principles by this *lucky chance.*

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* Thèse sur la valeur diagnostique des Déformations de la Poitrine, &c.: one of the most interesting monographs, though only an inaugural essay, that we have for a long time met with. M. W., we see, has since published a book on the subject, which we shall notice hereafter.—Ed.

Another prominence seated in the post-clavicular space, and originally discovered by our author, usually coexisted with the mammary dilatation. Its frequency was so great that it was found in every case but one, after its first discovery. It is especially remarkable in old meager subjects, in whom the region referred to appears to have regained its youthful fulness of form. As M. Woillez judiciously remarks, the state of the post-clavicular region furnishes a very useful guide in cases where the physiological or pathological character of the mammary prominence is a matter of doubt.

Again, Dr. Stokes' experience differs completely from that of Louis. After stating in his recent work that he had seen several cases in which the acromial, interscapular, supraspinous, and subspinous surfaces had become nearly horizontal, he continues: "Under these circumstances the apices of the scapula are remarkably projected; anteriorly we observe the clavicles arched and prominent; and the *triangular spaces which answer to the insertion of the sterno-mastoid and scaleni muscles are singularly deep,*" (p. 177.) Further observation must decide between these two eminent pathologists; meanwhile we may be permitted to state, that we have not, in a considerable number of cases attentively examined, met with a single instance of the appearance described by Dr. Stokes.

One important fact was elicited from the post-mortem examination of the fatal cases, the correspondence of the thoracic dilatation to the portion of the lung most advanced in disease. M. Louis enters into no enquiries as to the mode of production of the emphysematous prominence, a point of much interest, and one on which M. Woillez has given us some reasonings closely logical and of rare ingenuity. We regret our limited space prevents us from laying them before our readers.

The next symptom examined is the quality of the sound emitted on percussion over the affected parts of the lung. On this subject we are given no information of sufficient novelty to merit notice. The author judiciously insists on the diagnostic importance of a combination of anormal clearness of sound, in any point of the thorax, with dilatation of its walls. As might be anticipated, the disease, when confined to the internal surface or central part of the base of the lung, cannot be detected by percussion. Our author met with one such case.

M. Louis' remarks on the condition of the respiratory murmur are interesting. The feebleness of respiration characteristic of the affection was ordinarily confined to a limited extent of the chest, rarely spread over its entire surface. This character of the breathing persisted unchanged during the entire stay of the patients at the hospital, except in the instance of one individual in whom it decreased considerably. "In four cases the respiratory murmur was *slightly* hard, as though produced by the entry of the air into a much smaller number of cells than on the opposite side, where the respiration appeared finer and softer," (p. 212.) As these characters of the respiratory murmur are precisely those observed in dilatation of the bronchi, a doubt seems naturally to arise as to the propriety of attributing them to emphysema. M. Louis removes it satisfactorily, by informing us that there was no bronchophony in the cases alluded to. This feebleness of murmur was generally proportional to the duration of the disease, but the rule in this respect suffered some notable exceptions. In three subjects more particularly, in whom the affection had only existed from two to two and a half years, it was more marked than in some others that had laboured under it for ten.

M. Louis observed two kinds of rhonchus in his patients, the sibilant or sonorous and the subcrepitant. The former existed in thirteen subjects out of thirty-one. Usually general, it was in four cases limited to the prominent part of the thorax. Our author considers the latter fact to indicate the possibility of its having some special connexion with emphysema.

The study of the subcrepitant rhonchus possesses more interest, though, as we shall perceive, it is completely independent, according to M. Louis, of the emphysematous lesion *per se*. It existed in twenty-two of the forty-eight subjects who left the hospital relieved. It was characterized by being seated at the postero-inferior part of the chest, in the majority of cases at both sides, and was never detected in the mammary prominence. Nevertheless there is no necessary incompatibility between it and the enlargement of the vesicles. In many cases in which the rhonchus was audible posteriorly, dilatation of the cells certainly existed, according to our observer, in the same position. But the truth is that the subcrepitant râle occurring with the characters described, has been shown by M. Louis to be a pathognomonic sign of acute capillary bronchitis, and, in the cases under consideration, simply proved the presence of an intercurrent attack of that affection. The existence of this râle at the time of entry, and its gradual decrease in proportion to the relief experienced by patients during their stay at the hospital, shows that acute pulmonary catarrh is the *immediate* cause that forces emphysematous subjects to apply for medical aid.

M. Louis conceives, and we think to a certain degree on just grounds, though from the vagueness of Laennec's statements it would be difficult to prove the fact, that this râle is the identical one pointed out by that observer as pathognomonic of emphysema. The facts above adduced, showing that the subcrepitant râle of emphysematous patients follows the same laws as that of individuals exempt from that disease and affected with *simple* catarrh only, render it evident that the error lies mainly with the latter pathologist. But we do not wholly agree with M. Louis either. We have ourselves frequently heard, and pointed out to others, in the situation of the mammary prominence, minute bullæ of dry subcrepitant rhonchus, a medium sound between the *croquement* of the French and the *bruit de frottement*, and this at a time when there was no râle audible at the base posteriorly, or any other symptom present of pulmonary catarrh. It is just possible that these sounds are what M. Louis describes as "hard respiration," but, if so, his account of the phenomenon falls, in our estimation, far short of the reality.

We regret that our author's enquiries on the physical signs of the disease are confined to those of which we have just taken a rapid survey. He makes no remarks on the distinctive characters of the inspiration and expiration; he does not specify with which of them the rhonchi coexisted or if they attended both, nor does he allude to the altered duration of the time naturally elapsing between the two movements; yet these are points capable of accurate discrimination and, as we can from experience testify, likely to furnish a pains-taking observer with many curious diagnostic data.

The state of the motions of the chest and diaphragm are equally passed over in silence.

Some observations are made on pain of the chest as attendant on emphysema that strike us as novel. This symptom was noted in fifteen out of thirty-two cases, and in thirteen of these it was situated on the same side as the thoracic dilatation. It was increased by inspiring or coughing, but M. Louis was unable to determine its other characters; a strange fact, when we reflect that it had existed from one to three or four years and upwards, in the instance that fell under his notice. Rejecting the hypothesis of its being caused by pleural inflammation, or by the forced extension of the walls of the chest, he is disposed to ascribe it, admitting the want of demonstration, to the dilatation of the vesicles themselves. Several circumstances are favorable to this view of its cause: among them we may instance the correspondence of the principal seat and chronic course of the affection with the location and duration of the pain, together with the improbability that so extensive a dilatation as the cells sometimes undergo, should take place without exciting uneasy sensations.

The frequency of cough in these cases renders it a valuable symptom. Variable in its intensity and time of origin, persistent or intermittent, it was observed in every case but one. In twenty-two cases it commenced at the same time as the dyspnœa, fourteen times after and ten times before that symptom. Whether continuous or intermittent, it never commenced at the same time as the dyspnœa when the latter supervened in early youth, and did not appear until after that symptom in cases in which it manifested itself *for the first time* after the age of twenty. The bearing of these facts militates to all appearance rather strongly against M. Louis' correctness in fixing infancy as the time of origin of the disease in subjects whose dyspnœa commenced at that period. He succeeds, however, in showing himself authorized in doing so, but his arguments are too long to admit of insertion here.

The sputa were carefully examined in thirty-five subjects with the following results: "In twenty-three cases they were frothy, aërated, or liquid, resembling a solution of gum; in the remaining twelve subjects greenish, thick, opaque, but little aërated, and not in rolled masses; or greyish, with some striæ of blood," (p. 228.) The former sort of expectoration was observed in individuals affected with chronic pulmonary catarrh, producing sibilant or sonorous rhonchus; the latter in patients labouring under the same complaint in the acute stage, and presenting subcrepitant râle at the base posteriorly. Our author notes as a new illustration of the necessity of a *special* cause for the production of hæmoptysis, that, of these thirty-five individuals, only one, a female, aged fifty-seven, who had ceased to menstruate at the age of twenty-seven in consequence of a sudden fright, had suffered from that symptom. Our readers are no doubt aware that, according to one of M. Louis' laws, severe hæmoptysis is peculiar to individuals affected with pulmonary tubercles.

The state of the heart was examined into in fifty-two subjects. Thirty of these were found to be more or less troubled with palpitation. In some that symptom only came on after fatigue or during the asthmatic paroxysm, in others its occurrence had no connexion with either. With the exception of individuals in whom they were of temporary duration, the palpitations did not commence at the same period as the dyspnœa, and generally made their first appearance after the disease had half run its course. The volume of the organ was found to be greater than natural in subjects in whom its irregularity of action had been continuous. M. Louis hereupon alludes to the difficulty of ascertaining the exact state of the heart by percussion or inspection of the præcordial region, in consequence of its increased sonorousness and occasional arched form.

Enlargement of the heart was discovered on the dissection of every patient who had during life laboured under œdema of the inferior extremities; while, in not one of those unaffected with œdema, was the heart hypertrophous. Hence M. Louis' conclusion, that "œdema supervening in the progress of pulmonary emphysema must be ascribed to its complication with organic affection of the heart," (p. 232.) The diagnostic importance of this proposition is evident.

Several pages are occupied with considerations on the diagnosis, but as they are in the main a mere recapitulation of the more important passages that precede, we are dispensed from fully analysing them. To one or two points only it may be useful to advert. One of these relates to the distinction of emphysema from aortic aneurism. A case of the latter disease was met with by M. Louis in which there was no thoracic prominence, while the respiratory murmur was inaudible or almost so in a considerable part of one side of the chest (a phenomenon noticed some time past in the same disease, if we mistake not, by Dr. Stokes,) and percussion produced a perfectly clear sound.

The case is an important one from its showing that a combination of perfectly clear sound and absence of respiratory murmur are not pathognomonic of emphysema. The absence of respiration is easily explicable by the pressure of the aneurismal tumour on the larger bronchi; but we cannot acquiesce in M. Louis' conviction, that the absence of thoracic prominence would suffice for the distinction of the two cases. What is there to prevent our supposing the anormal state in question to be the result of *incipient* emphysema, in which the thoracic walls had not yet had time to give way? Nay more, of *advanced* emphysema,—for M. Louis has nowhere proved that the development of the disease involves the necessity of a change in the proportions of the chest; and M. Woillez has, in the thesis already quoted, clearly shown that such necessity does not exist. In the very same page, indeed, our author himself testifies to the force of our objection. After stating that all the symptoms described, with the exception of those announcing cardiac complication, existed in almost every case observed by Dr. Jackson and himself,—an important fact,—he proceeds to show the diagnostic value of different groups of them when others were absent. Now he commences with the case in which no partial prominence existed.

There is one view in which the power of diagnosticating emphysema is an acquirement of high value to the practical physician. It enables him to dispel the fears of many patients in whom the disease, from the habitual oppression and bronchitis accompanying it, assumes to a certain extent the character of pulmonary consumption. It warrants him, too, in exposing the quackery of those unprincipled men who, aided by the natural credulity of suffering patients, pretend, in the teeth of all scientific experience, to have the power of curing pulmonary tubercle.

An interesting fact is here brought forward connected with the coexistence of tubercles and emphysema. In his Researches on Phthisis, after stating that dyspnœa from infancy was ascertained to have existed in one-ninth of his tuberculous patients, and deciding that it could not be ascribed to the tubercles themselves, M. Louis deferred the explanation of the fact to a future day when some discoveries in pulmonary pathology might throw light on the difficulty. The presence of emphysema, a disease which our author had scarcely studied when he published his book on phthisis, was beyond a doubt the cause of the dyspnœa in the consumptive subjects alluded to. A precisely similar case is given in the present essay.

A sketch is drawn of the history of a patient, affected with both diseases, in whom the physical signs of emphysema gradually disappeared, while those of tubercles increased. This change, the natural result of the growth of the latter, explains how an observer might have been perfectly right in announcing the presence of emphysema in some cases, where after death scarcely a trace of the disease was discernible. The lapse of time required to bring about so material a change in the pathological state of things may, in cases of acute phthisis, be far from considerable. A case is also reported that seems to prove satisfactorily that extensive enlargement of the vesicles may take place in so short a time as twenty-nine days.

The remarks on the causes of the affection are limited in extent, and chiefly of a negative kind. Let it not be supposed, however, that we undervalue information of this kind. The following facts prove in M. Louis' opinion, that if bronchitis have any influence on the production of the disease, it must be very trifling and rarely brought into play. In our minds they show that Dr. Stokes has gone too far in styling emphysema "the result of bronchitis." First, in numerous cases the symptoms of dilated vesicles are not preceded by those of catarrh. Secondly, the habitual dyspnœa did not appear, in several instances, to suffer any increase after an attack of acute bronchitis. Thirdly, the maximum degree of the disease is found at the anterior border of the lung, while acute catarrh is most fully developed at the base posteriorly. A fourth argument on the same side may be found, as it seems to us, in a fact discovered by Dr. Stokes, and of which he appears to have overlooked the bearing on this question. He says, "the sign of feebleness of respiration is but little affected by the increase or diminution of bronchitis." Now, the increased volume of the lung, which, according to Dr. Stokes' views, causes the feeble respiratory murmur, is produced by dilatation of its vesicles, consequently so must the diminished force of respiration be the effect of that enlargement. But as the respiration is little affected by bronchitis, it follows that the dilatation of the cells, which is proportional to the weakness of that respiration, must be but little affected by it also. In general terms this is tantamount to saying that a cause which is powerful enough to produce a disease, (as inflammation of the bronchial tubes, according to Dr. Stokes, produces emphysema,) loses the capability even of influencing its symptoms when it has once brought it into being. Such a proposition as this cannot surely be assented to.

Laennec's well known hypothesis as to the mechanism by which the dilatation is immediately brought about, though sufficiently plausible, seems certainly little shaken by M. Louis' observation that, "whatever be the size of the enlarged vesicles, be it even that of a cherry stone, they are found empty, without either mucus or false membrane in their interior," (p. 254.) We have already, in our Number for last October, p. 311, alluded to M. Louis' objection, and stated our reasons for not completely giving in to it.

As regards the question of hereditary influence, Dr. Jackson's patient enquiries furnished him with the following conclusions: "Of twenty-eight emphysematous subjects eighteen were of parents who had suffered under the same disease, many of them having died during its progress. In some cases the brothers and sisters were similarly affected. Of fifty individuals unaffected with emphysema, three only were of parents that had laboured under the disease," (p. 255.) Pulmonary emphysema is by these facts clearly proved to be frequently an hereditary affection. They are corroborated by another series of facts discovered by the same zealous observer, showing hereditary influence to be much more marked in cases where the disease commences in early youth than where it first appears towards the twentieth year.

Of the various conclusions deducible from our author's pages, that for which the reader was possibly least prepared is the evidently great frequency of the disease described in them. The greater part of the cases were observed in the space of twenty months, in wards containing ninety beds. The large proportion of emphysematous subjects among the victims of cholera, twenty-three out of fifty, affords still further evidence of its remarkable prevalence.

M. Louis' section on treatment is exceedingly brief and unsatisfactory. As prophylactic means, he advises the avoidance of all known causes of diseases, of those of the lungs in particular. With respect to the therapeutic effects of polygala, the oxymels, medicinal soap, &c., which are earnestly lauded by Laennec, our author is disposed to deny their reality altogether. According to him, the amelioration observed by that physician during their exhibition resulted from repose, hospital diet, and use of diluent drinks. We confess that having ourselves seen the good effects of this negative treatment in the hands of M. Louis, we find it difficult to gainsay his statement. Nor is the opinion he has formed of the efficacy of bleeding, either in serious or slight cases, of a more favorable character. His confidence in the various preparations of opium is on the other hand very great. In twenty-six cases out of thirty, notable relief immediately followed its exhibition, an improvement which could be attributed only to its use, as the asthmatic paroxysm recovered its primitive intensity the moment it was, by way of experiment, discontinued. It is in the most severe cases, where the exacerbation is violent, and the presence of capillary bronchitis proved, that the excellent effects of opium are seen to the fullest advantage. Among the cases alluded to in support of its efficacy, there is one that too clearly shows its superiority to venesection as a remedy in this affection, not to deserve extraction. The patient to whom it refers "was bled three times in the course of two days without experiencing the slightest benefit, the dyspnœa remaining unaltered, and a fatal termination appearing more imminent after the third than before the first bleeding. Recourse was then had to the gummy extract of opium, which was given to the extent of two grains in a few hours. On the following day the dyspnœa was very moderate, the respiration much less frequent, and the patient so materially relieved that he considered himself cured," (p. 260.) In some similar cases, occurring subsequently to this one, no bloodletting was employed and the opium exhibited at once. The relief in this way was no less prompt and complete.

The cases interspersed through the memoir, which are only six in number but all of much interest, confirm, as far as their limited evidence goes, the general propositions of the essay. Their bearing on each question is carefully pointed out by the author, nor have we detected any instance in which more stress is laid on them than they seem to warrant.

On the whole, these researches will form a valuable addition to the libraries of those who are studious of correctness in diagnosis, and had they come from an ordinary observer would justly have been esteemed a very remarkable performance. As it is, they probably may not detract from but certainly do not add to their author's claims to our respect. They want that searching sagacity in induction that abounds in his other works, and their most valuable propositions are mainly deduced from knowledge—original and his own, it is true—but still previously acquired. M. Louis is indeed his own severest critic, for by what standard but his own labours should the author of the "Recherches sur la Gastro-Entérite" be measured? Assuredly none. In that almost unrivalled work, M. Louis has given evidence of the strongest faculties for observing, of which the mind may be believed capable, and seems, while he has produced results that will be more and more valued in proportion as men learn to appreciate the excellence and grandeur of inductive truth, to have reached the limits of probable perfection.

## II. *Recherches sur le Cœur et le Système artériel chez l'Homme.* Par J. Bizot, de Genève, M.D. &c. (*Researches on the Heart and the Arterial System in Man.* By J. Bizot, M.D.)

The memoir of M. Bizot consists of a series of minute investigations, from which general conclusions are drawn by the numerical method, on certain points of the normal and morbid anatomy of the heart and arteries. It may, on first view, perhaps, appear that a work on this subject was unnecessary, and especially uncalled for, just after the publication of M. Bouillaud's bulky treatise. An analysis of its contents will, however, we trust, convince our readers that the facts established in it are of the most part new, and peculiarly worthy of attentive study, from the accuracy and extent of the data on which they are founded.

The number of individuals examined by our author in the prosecution of his researches amounted to 157: of these, (of whom an equal number belonged to each sex,) 122 were upwards of, and 35 under, fifteen years of age. Considerable as this number of facts is, we conceive M. Bizot is not over scrupulous in viewing them as insufficient to furnish definitive laws.

The method followed by M. Bizot in obtaining the exact measurements of the heart, and accurate notions of its lesions generally, was the best calculated for the avoidance of error. As a proof of his determination never to sacrifice perfect accuracy to brilliancy or seeming perfection of results, we may notice the following fact: Finding the irregularity of form of the auricles to be such that a particular preparation of the venæ cavæ would have been required to enable him to judge with exactness of their interior, and, being unable to make this, he leaves the questions of their capacity, &c. open to future observers. It would have been easy for M. Bizot to obtain approximative results from ordinary data; but he felt that "the numerical method, applied to medicine, possesses all the imposing authority and all the dangers of statistics. Legitimately employed, it establishes truths; applied to materials of doubtful accuracy, it may propagate, under the appearance of close demonstration, the most baneful errors. Is it not, then, the bounden duty of the enquirer who avails himself of it to work on a perfectly solid basis, or refrain from its use altogether?"

M. Bizot's opening table proves the following facts: First, that we cannot obtain a *single* standard for the heart's volume, inasmuch as it varies according to the age, sex, &c. of the subject; secondly, it discloses a law of primary importance,—namely, that an indefinite increase in the size of the organ occurs in all subjects in the ratio of their age, and ceases only with life. This law, it must be remembered, is deduced from cases unattended with any symptoms of cardiac disease, and is therefore to be considered one of those regulating the economy in its normal state. The growth is regular and constant in both sexes, as respects length and breadth. In every series of ages, the female heart is smaller than the male; even in very young subjects: a result which coincides remarkably with the observation of M. Quetelet, that female infants weigh less than male infants at the moment of birth. This law of the indefinite growth of the heart is greatly at variance with one of Bichât's opinions. In one of his bold generalizations, he asserted that it is "from the twenty-fourth to the twenty-sixth year the muscles of organic life acquire their full development." Our author's researches satisfactorily prove his error in the case of the heart: careful investigation can alone determine whether the doctrine be less faulty when applied to other organs. The same law shows the inadequacy of the test of the heart's size to the normal state proposed by Laennec,—namely, its correspondence with the size of the clenched hand of the subject. It is hardly worth while to notice how it also refutes the absurd notion entertained by Richerand and others, that individuals, having proportionally the largest heart, are also the most courageous. It will hardly be maintained that animal courage is greatest in old age.

The influence of stature on the heart's size appears to be slight. Such as it is, however, it is exactly of the opposite kind to what we should, *à priori*, be inclined to suppose: for, "in individuals of the male sex above sixty inches, and in females above fifty-five inches in height, the mean dimensions of the organ, especially of its breadth, is less than in persons of lower stature." The difference in males averages upwards of three lines. The width of the shoulders furnishes a better proportionate standard of its various measures: the distance between the acromial point of the clavicles and the length and breadth of the heart increase in tolerably regular ratio.

The information supplied as to the influence of disease of the various organs on the heart's size is rather limited. The labours of a single individual could scarcely, perhaps, be expected to throw light, at least in all its bearings, on so extensive and complicated a question. M. Bizot has, however, collected in one group fifty-seven cases of phthisis, and compared the mean dimensions of the heart in them with similar measurements in sixty-five patients who died of various other affections. The result is that, both in males and females, the organ is smaller in all its proportions in tubercular subjects than in others. As phthisical cases are here compared with all other diseases united, there is no contradiction between the proposition now laid before the reader and the fact already established by Louis, that the heart is smaller in cancerous diseases, especially of the stomach and uterus, than in any other.

The above results, it will be observed, contradict the hypotheses of Laennec, Bertin, and Bouillaud, as to the agency of phthisis in producing hypertrophous heart; while they substantiate the induction of Bayle and Louis as to the independence of these two morbid states.

The general propositions established by M. Bizot relating to the extent of the ventricular cavities are the following: 1, their capacity is constantly increasing; 2, they are broader than long in both sexes and at all ages; 3, the mean length and breadth, especially the latter, of the right ventricular cavity considerably exceed the corresponding measurements on the left side, and the ratio of increase on both sides continues equal at all ages. By the latter fact, we are justified in rejecting as erroneous the opinion of Béclard that, at an advanced age, the right cavity undergoes a proportionally greater increase than the left. The cavities of the ventricles are smaller in females than in males, and in phthisical subjects than in those affected with other diseases.

The mean thickness of the left ventricle is shown to increase regularly with the age of the subject. Béclard is here again proved in error, in asserting that the tissue of the heart grows gradually thinner in advanced age. The same facts also indicate the imperfection of Cruveilhier's standard of hypertrophy,—a thickness exceeding seven or eight lines. Without adverting to the fallacy necessarily resulting from the variations dependent on age, &c., the tables before us fully prove that the walls of a left ventricle, seven lines thick in the male, or six in the female, would be in a commencing state of hypertrophy. The laws affecting the thickness of the walls of the right ventricle differ remarkably from those of its fellow. The age and sex of the subject, the heart's volume, the capacity of the ventricle itself, here exercise very little influence. Hence it appears that "the majority of writers, in taking the right ventricle as the point of comparison in estimating the proportional thickness of the left, have adopted the most defective method that could be selected. The reason of this is plain: the thickness of the right walls remains almost stationary, while that of the left is continually on the increase," (p. 289.)

Judging from M. Bizot's facts, the measure of hypertrophy proposed by Cruveilhier for the right ventricle,—any thickness exceeding four or five lines,—is even wider from the mark than that proposed by him for the left; for, "in the male subject, the maximum thickness of the right ventricle, from the fiftieth to the seventy-ninth year, was only 2⅛ lines, and in the female 1¼ lines," (p. 290.)

The walls of both ventricles are thinner in phthisical subjects than in others. It becomes a question whether this gradual and proportional diminution of all the heart's dimensions in tuberculous subjects should be considered a morbid condition of the organ. We coincide with our author in thinking that it cannot be esteemed a disease, any more than the atrophy of the muscles of animal life in the same individuals. Indeed, the coexistence of cardiac disease and phthisis seems very rare. Of fifty-seven tuberculous patients, not one suffered under functional disease of the heart. Even the œdema of the inferior extremities, observed in a certain number of them, was in every instance explained by the presence of organized coagula in the femoral and external iliac veins.

The same remarkable tendency to increase of size exists in the orifices of the heart as in its substance: they become wider as the subject advances in age. This fact is proved by our author's twelfth and thirteenth Tables, which also confirm M. Bouillaud's observations as to the excess of width of the right openings over the left. M. Bizot's researches, however, show that the difference between the sides is much greater than M. Bouillaud supposed. Thus, the mean circumference of the right auriculo-ventricular orifice exceeds that of the left by nine lines in the male, and seven in the female. The excess of the pulmonary orifice over the aortic is still greater, and is most marked, in the female subject.

The elaborate tabular exposition of the caliber, both absolute and proportional, of the various arteries, is too extensive for our pages. We must content ourselves with the short extract which follows, from the author's summary.

> "Hence it appears that, contrary to the general opinion, infancy is not the period of life at which the arterial system is of relatively the largest capacity. It attains its highest degree of development, both in diameter and thickness, towards the close of life.* The body, having grown for a time in every part, reaches the point at which all its constituent organs are accurately balanced in respect of their development. A retrograde action then begins. The muscles fall into a state of atrophy, the skin

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* This is a new proof of the universality of the law discovered by Louis, that membranous tissues thicken in proportion to their dilatation. It was this law, established for the œsophagus, stomach, intestines, ureters, &c., that induced that philosophical observer to doubt the occurrence of what Corvisart termed passive aneurism of the heart. The progress of our knowledge in cardiac disease has, as is well known, proved the justness of his doubt, by showing the extreme rarity of such a morbid state.

wrinkles, the sensations lose their acuteness,* the intellect fails; all gradually die. A single system of organs continues to grow—*the heart and arterial system.*"

In making known this law of the vascular system, which overturns such an abundance of theories on growth, reparation, &c., M. Bizot has rendered a most important service to the science of general anatomy.

In order to ascertain the form of the arterial tube with exactitude, our author measured the width of *two thousand one hundred and sixty-two* vessels, at different points of their course. The various shapes observed, and their proportional frequency, were the following:

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| 1. A cone, with its base turned towards the heart | 858 times. |
| 2. Two truncated cones, opposed at their truncated summits, | 610 |
| 3. A cone, with its summit turned towards the heart | 319 |
| 4. A perfect cylinder | 248 |
| 5. Two cones, opposed at their bases | 127 |

A fact which must forcibly strike the reader on a glance at the above table is, that the cylindrical, instead of being, as writers inform us, one of the most common, is, on the contrary, one of the rarest forms. Each vessel affects a certain form, which it presents much more frequently than any other. The figure of the popliteal, for example, is fusiform, or that of two cones opposed at their bases; a modification rarely found in any other vessel. It is worth considering whether the frequency of popliteal aneurism may have any relation to this fact.

The second part of the essay is devoted to the consideration of Structural Changes. Previously to entering on this branch of his enquiries, M. Bizot says a few words on a question of importance,—the pathological value of red coloration of the internal surface of the arteries. His experience has convinced him that such coloration, when unattended with thickening of the internal membrane, change of consistence, or any other morbid appearance, cannot be considered a morbid state. The results obtained by Trousseau, Louis, and Andral, are thus confirmed, and the fallacy of the notions entertained by Frank, Bouillaud, and others, still further proved.

M. Bizot has met with but one species of structural change of primitively acute form. It consisted of an exsudation on the internal surface of the vessel, varying in thickness, albuminous in appearance, and of the same consistence as a viscid jelly; perfectly transparent, smooth, either colourless or of rosy hue, and always unattended with injection of the subjacent or surrounding membrane. He conceives that all doubt as to this matter being the primary form of the white patches described in works by the term cartilaginous, is removed by the following considerations: Close to the above exsudation, or in some other point of the vessel, are found productions differing from it simply by their closer adhesion to the lining membrane; further on, the albuminous substance is more consistent, has a milky tinge, and adheres still more firmly to the arterial surface; lastly are observed whitish patches of different sizes, resembling in consistence and colour hard-boiled white of egg, or as dense as cartilage. All traces of the internal membrane are now lost, and the adventitious patch is in contact with the middle coat, or connected to it by atheromatous matter. It follows from this that those authors are in error who describe the cartilaginous patches to be contained between the middle and internal coats of the vessels, instead of on the surface of the latter. M. Bizot also rejects the prevalent notion that the osseous scales formed in the arteries are transformed cartilaginous patches: his reason for doing so is that, according to the hypothesis of transformation, the transition from the cartilaginous to the bony state should be traceable; whereas, the most laborious research failed in showing the least appearance of ossification in the patches alluded to.

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* Perhaps the exception claimed by Bichât for the sense of *taste* may be admitted. He certainly did not much exaggerate in terming that sense "le dernier fil auquel tient le bonheur d'exister."—*Vie et Mort.*

M. Bizot informs us that the albuminous matter, when secreted in isolated patches, does not cause any local or general derangement of function. The case is, however, widely different when an extensive surface is affected. Under these circumstances, a train of formidable general symptoms is the result. In confirmation of this fact, he gives the history and dissection of three cases of the affection. In these we discover a uniform course of symptoms; acute œdema, commencing by the inferior extremities, and thence spreading over the entire body, accompanied with fever, without any symptoms referrible to the heart or principal vital organs. The lesion found after death was the same in the three cases: an albuminous false membrane lining the whole internal surface of the aorta. We agree with the author in thinking that these cases are examples of acute aortitis.

The lesions of primitively chronic form are subdivided by M. Bizot into those common to the entire arterial surface, and those confined exclusively to the vessels of the extremities.

Every one who has attentively examined the surface of the aorta has often observed numerous specks, of the size of a grain of sand, of whitish yellow colour, unattended with the slightest redness in the adjoining tissues, and especially occurring in the neighbourhood of the coronary, carotid, subclavian, and intercostal arteries. Isolated in some places, they are in others united into groups forming yellow patches, and are evidently contained between the middle and internal coats; for, on removing the latter, part of the patch is found attached to it, while the rest adheres to the former. These patches go through a series of transformations, classed by M. Bizot under the two heads of *ulcerative softening* and *ossification*. He proves, by the closest reasoning, that the various lesions described under the names of pustules, abscesses, atheromatous, steatomatous products, &c., all spring from these almost invisible specks, and are developed without any trace of inflammation. He clearly shows that the term *abscess* is improperly applied in these cases of ulcerative softening: though the softened matter has some resemblance to pus, yet its mode of formation differs totally from this, and assimilates it much more to tubercle.

Morgagni and Haller long ago described the series of changes above alluded to, but they erroneously included the cartilaginous patch among the transformations of the yellow patches. Their account of the bony depositions also differs from M. Bizot's. According to him, the osseous formation commences by a small, hard, semi-transparent point, attacking at first the superficial layers of the middle coat, and gradually reaching the deepest, though rarely destroying that membrane altogether; while the internal membrane, after a time, completely disappears: and he maintains that this bony development "is, properly speaking, not a *transformation* of the spots, as Haller and Morgagni supposed, but that the yellow spots are in reality the *nidus*, in which the phosphate of lime is deposed," (p. 334.) He does not attempt to trace out the circumstances which severally cause the primitive lesion to end in ulcerative softening or the formation of bony matter.

The atheromatous matter, which we have already noted as being frequently formed under the albuminous patches, when in the cartilaginous state, sometimes undergoes the same transformations. Occasionally its abundance produces remarkable effects. In the smaller vessels, it causes obliteration of their cavity; in the larger, it destroys the middle membrane. The walls of the vessel so attacked lose their elasticity, from a double cause,—the destruction of the fibro-elastic membrane, and the growth of cartilage devoid of elasticity on their surface. Wherever this occurs, the vessel becomes an inert tube; a fact by which the dilatations of the aorta, without aneurismal tumour properly so called, have been plausibly explained.

Of the structural changes peculiar to the vessels of the extremities, our author describes two varieties. By comparing the following summary of his remarks with the account we have given of the other form of ossification, the delicate distinctions between them may be traced: In one of them the middle coat is primarily affected, and undergoes one transformation only,—the gradual solidification of its fibres; which at the same time grow transparent, and thinner than the original membrane. In the other, which is of less frequent occurrence, the middle coat itself appears to escape attack, while the ossified points are of granular shape, arranged in the form of an ellipse, and are not, in the outset, covered by the internal membrane.

The third part of the essay opens with some considerations on the Pericardium. The white spots so frequently observed on this membrane are of two kinds: of these, one species is produced by the transformation of the matter thrown out in inflammation on its external surface; the other, unknown in its exciting cause, is brought about by slow alteration of the membranous tissue itself, which gradually loses its normal transparence and thickness. The spots of the latter species are much more common than the former; three times more frequently observed in males than females; grow at a later period of life in the latter sex; increase in size and frequency of occurrence in the ratio of the age of the subject; are most commonly met with on the anterior surface of the right ventricle; and are uninfluenced in their origin or progress by the tuberculous affection. Such, at least, are the laws respecting them which M. Bizot's facts allowed him to deduce.

Relatively to fatty deposition under the pericardium, our author ascertained that its well-known greater frequency in females cannot be ascribed to their being usually fatter than the other sex: "for, of twenty-nine women without a trace of subcutaneous fat, fourteen had the heart surrounded with the maximum quantity of adipose tissue ever observed; while, of thirty men, equally spare in the body, three only were at all remarkable for fatty pericardium." Further on, we are told that, in six out of eleven phthisical men, there was a complete absence of fat under the membrane spoken of, and a very small quantity in the remaining five. The maximum of fat was found in eleven out of twenty-five tuberculous women; a moderate share in eleven others; while it was completely wanting in three. M. Bizot also found the heart itself fatty in eight subjects out of fifty-seven: all these eight were females, and died tuberculous. Connecting these remarkable facts with what is already known on the subject of fatty liver, the reader will not fail to perceive that they furnish valuable materials for investigating those strange aberrations from the normal state of the nutritive secretions observed in phthisis.

It is well known that Bichât, in his General Anatomy, guided by the difference of appearance observed, after a certain period of life, in the internal membrane of the venous and arterial systems, gives it as his opinion that they are dissimilar in original structure. M. Bizot rejects this doctrine because, 1st, in infancy, the two membranes are in every respect perfectly alike; 2dly, the ossifications of the arteries do not, as Bichât supposed, originate in its lining membrane; 3dly, the venous membrane, though very rarely, is occasionally affected in the same manner as the arterial. In forty-four pulmonary arteries, M. Bizot detected spots nine times. The reader to whom the researches of the modern French school are familiar will feel no surprise in learning that, in forty-four out of 155 subjects, M. Bizot found the foramen ovale permanently open, and in every case unaccompanied with symptoms of cardiac disease. Former observers had distinctly proved the fallacy of the old doctrines concerning the connexion of cyanosis with this malformation.

It appears, from a table showing the periods at which the spots and ossifications were first detected in each artery, from the earliest age upwards, that a remarkable difference exists in the time of life at which these lesions are habitually developed in any two given vessels. Thus, as regards the simple spots, the primitive iliacs were the earliest affected in our author's cases, and this at the age of seven; while no similar lesion was found in the brachial artery before the age of forty-eight; thus making a difference of thirty-one years between these vessels. They occupy the two extreme places in the scale. An analogous distinction exists with respect to ossification: the first bony plate met with was found in the posterior tibial arteries of a subject thirty-five years old; while in the internal carotid no trace of similar formation was detected earlier than the sixty-sixth year. The other vessels take intermediate places in the scale. Another important fact disclosed by this table is, that the vessels nearest the aorta are those first attacked by the *yellow spots*, while those farthest from it are the earliest to present *osseous deposits*. Among the physiological inferences drawn by M. Bizot from these data is an ingenious mode of accounting for the force and hardness of the pulse in old subjects.

> "A great part of the arterial system," he says, "having lost its elasticity in individuals of advanced age, from the development of organic changes, the entire force of the heart's action—which would be partly spent in dilating the whole system of arterial tubes, had they been all elastic,—is transmitted to whatever vessels remain sound. Now, as the radial is among the latter, it is perfectly natural that the pulse should be stronger in the old than in the young," (p. 393.) . . . . "Another probable consequence of this want of elasticity is, that bloodletting must weaken

much more in advanced age than in youth: for, as the arterial coats do not return on themselves in old subjects, it follows that, by removing a certain quantity of blood, we take away from that which remains a considerable part of the sum of pressure exercised on it by the large vessels. Now, this effect can only be produced to much less extent in youth, inasmuch as, from their great elasticity, the walls of the vessels adapt themselves to the diminished quantity of fluid, and continue to compress it." (p. 394.)

By comparing the different parts of each vessel with respect to the frequency of their lesions, M. Bizot ascertained that they occur most commonly in the neighbourhood of the origin of other arteries, and especially of large trunks.

The essay terminates with an account of a most interesting law of morbid development, called by its discoverer *the law of symmetry of lesions*. On inspection of the tables introduced into the work, we find a close similarity in the figures indicating the number of morbid changes found in any given artery on the right and left sides. But, further, when each pair of vessels was studied carefully in a series of *individuals*, the exact resemblance of the right and left lesions in each subject, not only as respects their number, but their exact position, proved the law in question one of the most general of morbid anatomy. It is true that to it, as to every other law, there are exceptions; but they are few in number, and, for the most part, easily accounted for. M. Bizot assigns three reasons for the occurrence of exceptional cases. These are—the change of structure not commencing simultaneously at both sides; the want of symmetry in some of the vessels themselves, as, for example, the common carotid and subclavian arteries; the nature of the lesions, the albuminous patches being less under the influence of the law than the other morbid products. Now, as the yellow spots have a strong tendency to grow underneath the cartilaginous plates, the existence of the latter is a cause of absence of symmetry in the former. We cannot follow M. Bizot in his details relative to each vessel, but extract a few points from his summary. In 2171 cases of simple spots, symmetrical arrangement was wanting in only sixty-one instances. In 659 instances of consecutive lesions, (including those of inflammatory nature,) exceptions occurred fifty-one times only; and, if we exclude from these fifty-one, twenty-nine cases of albuminous or cartilaginous patches, there remain but twenty-two that do not acknowledge the law.

The reader cannot have failed to expect that the collation of our author's results with the various facts hitherto ascertained on the history of aneurism should illustrate the cause and mode of growth of those tumours. He himself has not neglected this view of his subject. The first point, well deserving the consideration of those who are disposed to include the various structural changes of the vessels among the causes of aneurismal disease, is the extreme frequency of the former compared with that of the latter. The result with respect to age, too, is remarkable. The ages of 108 subjects affected with aneurism, and ascertained from various authors, were as follows:

| | | | |
|---|---|---|---|
| From 10 to 19 | 1 subject. | From 50 to 59 | 14 subjects. |
| .. 20 .. 29 | 15 .. | .. 60 .. 69 | 8 .. |
| .. 30 .. 39 | 35 .. | .. 70 .. 79 | 2 .. |
| .. 40 .. 49 | 31 .. | .. 80 .. 89 | 2 .. |

Now, judging from this table, and from the fact that it was not at the time of the origin of the disease, but at that of the patients' seeking advice, these ages were noted, we must admit that the laws regulating the period of development of the two morbid conditions are almost the converse of each other. Next, passing to the influence of sex, we find that, of 189 subjects affected with aneurism, 171 were men, and only eighteen women; while M. Bizot's tables of arterial lesions do not in any case give such a majority to one sex over the other. If the law of symmetry be taken into consideration, a new point of discrepancy is discovered; for, of 138 subjects labouring under aneurism of the extremities, six only were affected with symmetrical tumours. Lastly, it is well known that non-traumatic aneurism of the limbs is much more frequent in the popliteal than in any other artery. M. Bizot found seventy-five cases of popliteal among 145 aneurisms. Yet, on the other hand, that vessel is far from being the most frequently affected with consecutive lesions, occupying in this respect only the seventh place in the scale. However, the quantity of movement to which the popliteal artery is exposed must be taken into account, as well as the fact that the posterior tibial artery holds the *first* place in frequency of structural lesions, and that many aneurisms, called popliteal because seated in the ham, may (as our author suspects) really belong to the origin of the former vessel. Be this as it may, however, the general force of these considerations seems to warrant the conclusion drawn from them by the author: "Although there be strong reason for admitting that lesions of the vessels, and even 'occasional causes,' are of great importance in the etiology of aneurism, yet there must exist some other cause beyond these, the nature and mode of which are unknown, and still need deliberate investigation." (p. 411.)

We cannot close our sketch of this valuable memoir without confessing that we have by no means been able to do it justice. It may truly be said of it, as of Haller's Physiology, that each line contains a fact. Enough of its sterling matter has, we trust, nevertheless, been transferred to our pages to induce the reader to study the original. If its perusal have no other effect, it will at least convince him of the vastness and dignity of medical science. Of the host of writers who have taken the arterial system for their theme, how few have done more than glance at any of the points investigated by M. Bizot; and yet how much has even he left undone? And what can be better calculated to elevate our notions of a science than to prove that the pettiest points connected with it may be invested with importance, when they are, in a multitude of cases, accurately established, examined, and compared?

## III. *Essai sur quelques Points de l'Histoire de la Cataracte.* Par TH. MAUNOIR. (*Essay on certain Points in the History of Cataract.* By M. MAUNOIR.)

M. MAUNOIR, author of this memoir, is nephew to the celebrated Genevese oculist of that name. Familiarized by early education with researches on the eye,—trained for original observation under the guidance of Louis,—and animated with a strong desire of coupling his name with some useful and sound discoveries, M. Maunoir brought to his task the most important requisites for the investigation of truth; and the result of his labours is of that valuable kind which might have been anticipated where such elements of success existed.

The extensive hospital practice of Professor Roux supplied our author with a wide field for his enquiries. The celebrity enjoyed by that surgeon for his operations on cataractous eyes (whether their ordinary event justify it or not) brought numbers from all parts of France to La Charité: and, as he is in the habit of confining his operations for cataract to the spring and autumn, no better opportunity for studying the affection could be had than that afforded by his wards during those seasons. The number of cases on which M. Maunoir's researches are founded amounts to 121, collected in four seasons, giving a mean amount of thirty operations a season.

There is, perhaps, scarcely any human ailment better understood than cataract, as far as regards the nature of the disease, its principal symptoms and varieties, and the surgical means fitted for its cure. M. Maunoir was fully aware of this, but did not, in consequence, despair of adding to our *accurate* knowledge of the affection by bedside observation and the numerical method. He saw that the exact proportion in which a given symptom ordinarily occurs, for example, and the side on which truth really lies where the opinions of first-rate men are at variance,—(let us instance the comparative excellence of extraction and depression,)—might be discovered by their help, and by it alone. In fact, with respect to the history of this disease, as of every other, though in a less degree than is the case with many, there are many lacunæ to be filled up, no few errors to be rectified, and, in almost every instance, positive values to be substituted for vague approximations.

Our author commences his analysis with an enquiry into the influence of those agencies generally looked on as *causes* of the disease. Sex, in the first place, cannot be said to affect its development; for, of his 121 patients, placed in wards containing an equal number of beds for each sex, sixty-one were males and sixty females. This confirms a statement, published some time since as the result of an examination of some cases of Dupuytren, 207 in number: of these, 135 were males, seventy-two females—a proportion exactly coinciding with M. Maunoir's, as the number of beds in Dupuytren's men's ward precisely doubled that in the women's.

Our author's tables give a good idea of the influence of age in causing opacity of the lens. The fourth or fifth of the total number of cataractous subjects is found between the ages of fifty and sixty; the third between sixty and seventy; a little less than the fourth beyond that latter age. These proportions, which, of course, refer to the period when the patients applied for assistance at the hospital, are confirmed by his results respecting the time of commencement of the disease.

In regard to this statement, it is, however, important to observe that, in making calculations as to the comparative frequency of occurrence of cataract at different ages, it is of essential importance to distinguish the particular forms. In early life, opacities of the lens or capsule, or both together, occur; but, for the most part, under pathological conditions differing from those which are found to hold in after-life. Capsular cataract also may be connected with a very different condition of the eye from lenticular. In aiming at accuracy, it is therefore not enough to say that, of a certain number of cases, so many occurred between fifty and sixty, and so forth; but we ought, first of all, to describe the exact form of these cataracts,—whether lenticular, or capsular, or capsulo lenticular, and whether with or without complication.

It is impossible to doubt, from the evidence of M. Maunoir's cases, that cataract is, to a certain extent, an hereditary affection. Hereditary influence was, indeed, traced in upwards of one-fourth of the individuals examined: and, with respect to cataract, there are none of those especial difficulties in establishing such influence which in other cases interfere with the validity of our apparently just conclusions. It is not, for instance, one of those affections, such as phthisis, which are so extremely common that, without any tendency being inherited from parents but from the simple fact of their habitually attacking a large body of the population, they must frequently occur in several members of the same family.

"Of thirty-nine subjects (twenty-two male, seventeen female,) who were enabled to give certain information on the point in question, twenty-two affirmed that their parents had never had the slightest complaint capable of giving rise to a suspicion of cataract. Ten others, on the contrary, (four men, six women,) assured me that one or more members of the family had suffered from the disease. In almost every instance the operation had been performed, so that there could be no question of the correctness of the diagnosis." (p. 79.)

Various other points, usually examined by writers in their etiological chapters, are passed in review by M. Maunoir: nor has he failed to come to positive results respecting them. Thus, he found individuals following certain trades more frequently affected with cataract than others; and certain habits of living, constitution, &c., coexisting with the disease in larger proportion than others: but he wisely abstains from concluding thence that the various circumstances alluded to should be numbered among the *causes* of cataract.

The commencement of the disease (a point of its history requiring examination, perhaps, more than any other,) receives tolerably full investigation from M. Maunoir. In the majority of cases, cataract commences slowly; the patient distinguishes objects, as it were, through a mist, and the morbid condition is confined to one eye. Such was the course of the affection in *fifty-two* cases out of *sixty-two:* in the other ten subjects, (eight men, two women,) the sight was lost more or less suddenly. In one instance, the patient went to bed without his visual powers being in the least impaired, and in the morning was scarcely able to guide himself; both eyes being equally affected. In *sixty-three* cases out of *seventy-two*, the disease commenced in one eye; the right being the first attacked *thirty-five* times, the left *twenty-eight*. In *nine* subjects, the faculty of vision became weakened in both eyes simultaneously. In *eight* out of *seventy-two* patients, the use of one eye was even completely lost before they perceived its being affected at all. The discovery of its condition occurred from accidentally closing the sound organ.

"May cataract often remain confined to *one* side during a series of years, or even for a whole lifetime; or, once developed in either eye, are the chances extremely strong that the other shall be similarly affected within a certain time? On first thought, this question may appear one of mere curiosity, but a little consideration will show its practical interest. If it were proved that the formation of a *second* cataract may be prevented, or that, when in its commencing stage, a spontaneous cure may be induced by simply operating on the eye first affected, the importance of rejecting the rule of practice usual in such cases—not to operate until both eyes are affected—becomes manifest." (p. 89.)

M. Maunoir is aware that the spontaneous cure of cataract is doubted by the majority of surgeons, but contends that they are rarely placed in circumstances suited for ascertaining the fact with precision. Besides, all writers do not agree on the point. Wenzel and Saint-Yves have both related cases of complete restoration of sight in persons who had only one lens operated on, though affected with perfect double cataract.

The opinion current in the profession respecting the latter question is, that, in the majority of cases, when the disease seizes one eye, the other ends by being similarly affected. This opinion has grown out of the great predominance of cases of double cataract observed in the hospitals, and even among the better classes. The conclusion is, however, not fairly drawn from the premises. Is it not quite as likely that the greater number of subjects presenting themselves for treatment with double cataract may arise from the fact that persons who continue to see with one eye refrain from seeking surgical aid? In this way, cases are met with of individuals that have been affected with single cataract for years without thinking of its removal. Why, in the same manner, it may be said, should not opacity of one lens exist in a number of persons during their *whole lives*, without their taking steps for its treatment? Spite of this rather plausible reasoning, M. Maunoir is induced to conclude the vulgar opinion to be in the main correct. His reasons for doing so are logical and forcible.

"First. In nine subjects out of seventy-two, the disease commenced in both eyes at the same time. 2dly. Eight only of these seventy-two individuals had completely lost the use of one eye when they detected a weakness of sight in the other. Hence there were only eight cases in which we are justified, strictly speaking, in supposing the first cataract to have existed *alone* for any considerable time: but even this is neither proved nor, from what follows, likely. 3dly. In every other case, the second cataract commenced growing a few weeks, months, or at the most from three to five years after the first. In two persons only was this period extended to ten and twenty-five years. 4thly. The mean space of time, in forty-seven cases, between the commencement of the disease and the period at which the patients grew so blind as to require a guide, was five years and a half." (p. 92.)

In whatever way this question be ultimately settled, the extreme frequency of double cataract is a remarkable phenomenon. Nothing, as our author remarks, is more rare than to see cancer attacking *both* breasts or testes: yet there is as complete a similarity in structure and functions in the case of those organs as of the lenses; and, as far as we can judge, the cause of cancer, when once brought into action, is as deeply rooted in the economy as the cause that produces cataract. This peculiarity is not, however, by any means confined to cancerous disease. Acute affections present it also; and, indeed, it seems to be more closely connected with the nature of the tissue than of the morbid action. Thus, Louis has shown that *double* pleurisy is almost unheard of, except in cases of organic affection of the lungs; pneumonia rarely becomes double,—we have found it so in only forty instances out of 321 cases, collected from various authors,—and is almost never so primitively; there are but two or three authentic cases of double non-calculous nephritis on record; while, on the other hand, in *sixty-two* out of *seventy* patients affected with tonsillitis, and observed by Louis and Rufz, both amygdalæ were inflamed together.

M. Maunoir's chapter on the Symptoms gives evidence of patient enquiry; and we agree with him in attaching importance to the study of the anomalous visual sensations occurring in cases of blindness, from whatever cause it may arise. A close acquaintance with them might very possibly often clear up a doubtful diagnosis, where the examination of the organs themselves, as too often happens, had failed to give absolute certainty. There is little, however, sufficiently novel in its contents to repay the trouble of extraction. In detailing the state of the pupils, and the degree to which the power of seeing was retained by his patients, M. Maunoir relates cases distinctly proving that immobility of the iris, irregularity of the pupil, and almost total incapacity of distinguishing light from darkness, are insufficient to warrant the surgeon in diagnosticating amaurotic complication.

Relatively to their appearance before extraction, cataracts are divided into four classes. Of 115 lenses removed from sixty-four patients, the following was their relative proportion:

| | | | |
|---|---|---|---|
| Gray | 47 | Blueish | 22 |
| White | 41 | Blackish | 5 |

In addition to its colour, the uniformity or striated appearance of the cataract is worth attention. The latter character is not rare: it existed in twenty-six of the sixty-four subjects alluded to, most frequently on one side, seven times only on both. The early writers on the disease attached great importance to the colour and uniformity of aspect of the lens. Maître-Jean, taking them as guides in prognosis, drew up a list of favorable and unfavorable colours. In more modern times this view of the question has been abandoned; but the generality of oculists affirm, as is well known, that, from the appearance of the cataract, we are enabled to determine if the opacity be capsular or lenticular, soft or hard. Thus, Beer and Travers assert that *membranous* cataract is always bright in colour, and of spotted or variegated appearance; never uniform. According to the former ophthalmologist, again, the tint of *caseiform* cataract is never uniform, but always maculated, the spots resembling fragments of mother-of-pearl. The opinion of Travers on the latter point is to precisely the contrary effect. In order to test these opinions by accurate observation, our author commences by enquiring into the frequency of capsular cataract. If we take the general sense of writers on the question, we must consider it almost as common as the lenticular variety. Dupuytren goes even further. He declares that the membranous occurs in the ratio of 1 : 1½ to the ordinary species: yet, on referring to a table of 264 operations for cataract, (eight only by extraction,) performed by that Professor, we find but fifteen noted as membranous, which gives a proportion of 1 : 16⅗! M. Maunoir, in order to account for this monstrous difference, suggests the probability of an error of impression,—an explanation of the difficulty which is hardly defensible, as the same statement is repeated in two works published under Dupuytren's own eye. Turn we to our author's cases. In these, *extraction* was performed 179 times, and in *five* cases only was the capsule evidently opaque. Here we have a proportion of 1 : $34\frac{1}{5}$ between the varieties in question. In six other cases the pupil was not perfectly clear after the lens had been removed; but in these instances the trifling opacity observed depended quite as probably on some fragments of the lens remaining *in situ* as upon commencing opacity of the capsule. Granting them to have been capsular cataracts, however, the proportion is still as 1 : $15\frac{1}{11}$. Now let the reader bear in mind that M. Roux's operations are all by *extraction*, and that, consequently, error on the question at issue was impossible on the part of M. Maunoir; whereas Dupuytren's cases were, with eight exceptions, couched, and consequently himself liable to be deceived respecting the number of membranous cataracts among them. We need not adduce any stronger evidence of the error of Travers's and Beer's diagnostic instructions than the facts just learned from our author's experience. We have only to refer to them to see that, of 117 cataracts, five only were evidently capsular, while thirty-three had the maculated surface in question. In one only of these five cases had the opacity of the capsule any peculiar or distinctive appearance: in this instance a small white square spot, situated on a brownish base, presented itself in the centre of each pupil.

Beset with difficulties as enquiries respecting the duration of chronic diseases usually are, those relating to cataract are especially so. Many individuals are so utterly careless about themselves that they take no note of what perhaps can alone be looked to with any confidence as a guide in the investigation,—the period when weakness of sight commences. Nor, it must be confessed, can we be certain that the time of origin is thus faithfully indicated, where patients have been watchful enough to notice it. Similar sources of error belong to other diseases. Among those peculiar to cataract is the fact already alluded to, that one eye may be affected for a considerable time before the individual discovers its condition, which he eventually does by chance. Nevertheless, these and other obstacles, which it is needless to enumerate, do not render the final settlement of the question impossible, but simply increase the number of cases; and hence the time necessary for its elucidation. M. Maunoir's results on the subject are derived from the histories of forty-seven patients, the only individuals, of those examined by him, on the accuracy of whose statements he could fully rely. They are as follows:

> "1st. (*No distinction of sex being made.*) The mean duration of the disease from the moment of origin, as far as this was determinable, to the time at which the patients ceased to be able to go about without a guide was about five years, one month, and four days. 2dly. In thirty out of thirty-two cases the cataracts on both sides occupied a different time in reaching maturity; and in twenty-one of these thirty cases the progress of the disease in the eye first attacked was the slower. 3dly. The mean period of maturation for the eye first affected was two years, five months and a third; for that last attacked one year, eight months and a half. 4thly. (*The sexes being distinguished.*) The total duration of the disease, as well as of *each* cataract, was greater in females than in males. 5thly. The second eye remained perfectly healthy for a shorter or longer time after the first was lost, in thirteen men out of eighteen, and in seven women out of fourteen; and, generally speaking, that time was much longer in the former than in the latter. The disadvantage here observed ou the side of females will be found to exist with respect to the prognosis also." (p. 118.)

Our author now passes to that portion of his subject which will doubtless appear of most importance to the majority of our readers: the operation, its difficulties, the accidents which may interfere with its completion, and its consequences. This chapter derives much interest from its containing the first analytical enquiry into the practice of the most dextrous operator on the eye of whom the Parisian school can boast, Professor Roux. Exclusively devoted as this gentleman is to the method by extraction, his cases necessarily furnish no data for solving the problem respecting the superiority or inferiority of that method, as a general practice, to couching. Considerable light is, however, thrown in the pages before us on several questions, which have long divided our best oculists; but, in the first place, it may be well briefly to sketch M. Roux's mode of proceeding in his cataract cases.

The period for operating, as we have said, is confined to spring and autumn. The patients, on their admittance into the hospital, are put on whey diet; mustard pediluvia are used daily, venesection being very rarely had recourse to. The morning of the operation a blister is applied to the nape of the neck, and kept open till the patient is permitted to leave the hospital. No means are ever, or scarcely ever, employed to dilate the pupil. If both eyes are in a fit state they are operated on at the same time. With respect to the operation, for the minute details of which we must refer to our author's description, the following particulars seem to require notice. It is always performed with Richter's triangular knife. The puncture of the cornea is practised considerably above the external extremity of the transverse diameter of the eye. The exact point varies: when the eye is favorably disposed, it is so managed that the incision is as much external as inferior. The point of the knife is brought out at such a point on the other side of the cornea, that the section, when completed, embraces a little more than the semi-circumference of that membrane. The section is made altogether by pushing the knife onwards parallel to the iris, and never by bearing from above downwards. So important does M. Roux consider this mode of dividing the cornea, that he constantly employs it, even at the risk of pricking the skin or conjunctiva of the internal canthus. This done, the operator introduces a *kystotome*, with blunt convex back and concave edge, and with it incises the double membrane investing the anterior surface of the lens. The usual means are then had recourse to for the gentle expression of the latter body.

The dressing applied after the operation, consisting of a white linen compress, followed by a black silk bandage, is removed for the first time on the fifth day. From that time the patients are directed to bathe the eye frequently with cold or lukewarm water, whitened with a few drops of solution of acetate of lead. On the first day they are left completely without food; from the second to the fifth, they are given broth; then soup; and lastly, the half or three quarters of the hospital allowance.

There is no ward set apart for ophthalmic affections at La Charité. The beds of the patients operated on are, as a substitute for that convenience, surrounded with thick sheets to exclude the light. In consequence of this plan, there is an exceedingly imperfect circulation of air about the patient; and M. Maunoir, with much plausibility, conjectures that some cases of failure of the operation may, in a great measure, be ascribed to the fatigue and restlessness resulting, in warm weather especially, from this circumstance. There is another incommodious arrangement at La Charité, adverted to by our author, but which affects the female patients only. From the position of the women's ward, they are obliged, after leaving the operating theatre, to ascend a staircase exposed to strong currents of air, in which we have ourselves frequently shivered, before reaching their beds. It is the more extraordinary that M. Roux should allow these drawbacks to the success of his operations to continue to exist, as the chief reason we have heard given for his confining his time for extracting to the spring and autumn is the greater evenness of temperature at those seasons.

The only difficulty alluded to by M. Maunoir, as occasionally thwarting the operator, arose from the extreme mobility of the eye in some cases. Many patients are, as is well known, totally unable to control its movements. He suggests a feasible method of preventing the occurrence of this obstacle; namely, directing the patient to practise, for some days previous to the operation, holding the organ steady. We are not aware whether such a plan has yet been made trial of.

The errors and accidents observed during the operation were the following:

> "1st. The section of the cornea was too small in one case to allow of the passage of the lens; it was necessary to enlarge it with scissors. 2dly. In some cases the operation was not finished from either of the following causes: *a*, after the introduction of the knife into the eye, a sudden movement of the patient caused it to slip out; *b*, a similar movement drove the point of the instrument through the iris or pupil into the posterior chamber, withdrawal of the knife being followed by evacuation of the aqueous humour; *c*, the pressure of the upper eyelid, intended to express the lens, caused the escape of some of the vitreous humour, while the cataract remained *in situ*. 3dly. The vicinity of the internal canthus was rather frequently wounded, at the moment of completing the incision of the cornea. 4thly. The iris was wounded a certain number of times under various circumstances. 5thly. A portion of the vitreous humour escaped either before or, as more frequently happened, after the expression of the lens." (p. 125.)

Here is abundant encouragement, it may be said, for our young surgeons: if such be the feats of the first operator in France, few of them need tremble for their reputations. And yet it must be admitted that the escape of some vitreous humour is not uncommon with every operator, nor is it, within certain limits, detrimental. Wounding the iris, or even cutting away a piece of it, is not deadly. These and similar accidents arise in some degree from the nature of the material we have to operate on, and cannot be avoided even in the best hands—in the hands of better oculists than Roux. The young surgeon who should begin to operate without being perfectly aware of these accidents, and without having seen them occur, would be to blame, because he would not be prepared for them when they did occur.

Independently of these blunders in the operation, certain other accidents frequently supervene after it, and mar its success. They are either local or constituted by intercurrent diseases totally unconnected in their origin with the condition of the eye: and, indeed, it is remarkable that the complete absence of morbid phenomena after the operation is extremely rare, even in cases of perfect success as regards the restoration of sight. Thus, of twenty-five patients in whom the operation was done on both sides, and twenty-six on one side, five only were altogether free from morbid symptoms during the progress of the cure. M. Maunoir devotes an extremely instructive and interesting chapter to the various phenomena observed in such cases; one of the most remarkable of which is a pain affecting the teeth, and apparently seated in the branches of the fifth nerve.

In estimating the results of operations for cataract, M. Maunoir acquiesces in Professor Roux's opinion as to the necessity of making a distinction between the eye itself and the individuals affected with cataract. Now the chances are always more in favour of the latter than the former; that is, there are proportionally more patients operated on who recover their sight than eyes on which the operation proves successful. From our author's experience, the following is the ratio between them: Of 115 patients operated on by extraction, either on one or both sides, seventy-three recovered their sight through the operation; or a little more than five out of every eight operated on. Of 179 operations, ninety-seven were successful, or a little less than five in nine.

The different lesions to which loss of sight was attributable in cases of failure may be comprised in the following statement:

> "1st. Fourteen eyes were destroyed by suppuration. 2dly. In nineteen cases, the cornea become so opaque that nothing could be distinguished behind it. 3dly. In nine cases, where the opacity of the cornea was not perfect, an opaque point was detected behind it in the field of the pupil. 4thly. There were fifteen cases of *membranous* cataract, unaccompanied with opacity of the cornea or deformity of the pupil. In three or four instances, these cataracts had an evident rosy tint, of perfect uniformity. 5thly. In six cases, there was an opaque body in the field of the pupil, with displacement or marked deformity of that orifice. 6thly. Complete occlusion of the pupil occurred in one case. 7thly. There remain eighteen cases that cannot be classed with the above: in one of these, though both pupils were perfectly black, the power of seeing was totally abolished."

In three or four cases only were symptoms of iritis noted: however, M. Maunoir could not affirm that they did not occur oftener, as he unfortunately neglected in some patients to observe the state of the iris. We are of opinion that, if M. M. had observed these cases more closely, as he ought to have done, he would have found many more instances of iritis. Jäger, of Vienna, is of opinion that some degree of iritis occurs in every case of extraction.

It appears from this summary, that membranous cataract existed in upwards of the third part of the unsuccessful cases. Such a proportion appears directly at variance with M. Maunoir's former conclusions respecting the rarity of that form of disease. This apparent contradiction, however, is easily explained. "For," says M. M., "I formerly spoke of cataract in general, and not of the patients who undergo unsuccessful extraction at La Charité. I have no doubt that their frequency in the latter subjects results from the mode of operating adopted by M. Roux. Is it not evident that, when a simple incision is merely made in the anterior part of the capsule, that membrane may become opaque, inasmuch as it remains in the eye; whereas, such a result would be impossible were it cut into pieces, as Beer advises, and brought out of the eye, in part at least, along with the lens?" (p. 146.)

We close our notice with a brief view of our author's valuable conclusions respecting the prognosis of the affection.

1. The age of the patients did not appear to affect the result of the operation from the twentieth to the sixty-ninth year; beyond that period

of life, its effects were less favorable than at an earlier one. 2. The instances of success were somewhat less numerous in females than males, whether the eyes or the individuals be considered. 3. From a comparison of forty-eight simple with sixty-four double operations, it appears the results obtained by operating on one eye were less advantageous than those derived from operating on both organs the same day. 4. Slight puncture of the internal canthus, observed twenty-seven times, had no bad influence on the event of the operation: of these twenty-seven cases, sixteen terminated by recovery of sight. 5. The effect of lesion of the iris was not more detrimental; for, in twenty-one cases of this accident, eight only proved failures. The quantity of blood effused was not sufficient, in any instance, to prevent the operation from being completed; sometimes not a drop was spilt. 6. The escape of a portion of the vitreous humour, on the contrary, is one of the most formidable mischances that can occur; for, of nineteen cases in which it took place, six only terminated favorably.

It is not a little remarkable that the two last propositions should be in direct opposition with the opinions usually maintained by writers. M. Maunoir's are the first results that have been obtained by accurate and numerical analysis on the questions referred to: to them, therefore, we give our adhesion in preference to the vague recollections on which the contrary statements are founded. M. Maunoir does not mention the quantity of vitreous humour lost, which is a matter of some importance: moreover, it must be remembered that there are certain states of the vitreous humour in which it escapes more readily than when perfectly healthy; these states of the vitreous humour are always accompanied by a constitution of the eyeball not absolutely opposed, but not favorable to success.

Pressed as we have been for space in this review of M. Maunoir's essay, we had either of two courses to choose in drawing it up. We might have noticed a few of his results; examined at length their bearing on the doctrines of former writers; traced their probable influence on opinion and practice; or we might have given a fuller analysis of his work, allowing the sagacity of the reader to supply such considerations as we might in consequence have necessarily omitted. The latter is the mode of proceeding we fixed on, convinced, as we are, that it is the best calculated to give a fair notion of the labours we wished to make known to our readers; and of fitting them for usefully reforming such notions as required reformation. But, even in this way, we have been compelled to pass over in total silence a quantity of minor details which at once contain facts of high interest, and, from the conscientious diligence that characterizes them, furnish a wholesome lesson to the practical enquirer.

## IV. *Mémoire analytique sur l'Orchite blenorrhagique.* Par M. Marc d'Espine. (*Analytical Essay on blennorrhagic Orchitis.* By M. M. d'Espine.)

Though not devoid of merit or interest, this production ranks infinitely below those with which it is associated: indeed, we question strongly the soundness of judgment evinced by the Society in allowing to appear, among its earliest labours, a work in which one of the very elements regarded by them as of paramount importance, namely, extensive and prolonged observation, is essentially wanting.

## ART. III.

*Guy's Hospital Reports.* No. V. *October*, 1837. Edited by G. H. BARLOW, M.A. and L.M., and J. P. BABINGTON, M.A. &c.—*London*. 8vo. pp. 260.

THE present Number of the Guy's Hospital Reports does no discredit to its predecessors. The papers by Dr. Bright and by Mr. Taylor, in this, as well as in some of the former Numbers, take the lead; but are accompanied by several others of very considerable value. The highly practical character of almost all of them is precisely that which should distinguish a book of the kind. Authentic and well-digested histories of individual cases of disease, unembarrassed by superfluous theory and vague speculation, tend in a much higher degree to the diffusion of useful knowledge among the profession at large than many of the more ambitious "systems" and elaborate dissertations of a former day. With very much to commend in the Number before us, we have still, however, to regret that more industry is not exhibited, in some of the papers, in the work of condensation. The number of medical periodical works which are now published throughout Europe, all claiming a share of public notice, is so great, that any one in which this indispensable qualification is overlooked cannot safely reckon on an extensive and permanent perusal. We shall notice all the papers that are of most importance as fully as our limits will permit.

### 1. *Observations on the Ganglionic Enlargement of the Pneumo-gastric Nerve; the probable Function of that Ganglion; and the Position which it occupies in the Human Subject, and in several of the lower Animals.* By Mr. EDWARD COCK.

THE object of this paper is to show that the pneumo-gastric nerve resembles somewhat, in structure and function, one of the fifth pair of nerves, or a spinal nerve. It contains, like them, two sets of filaments,—the one for sensation, the other for voluntary motion; and to these is added a third set, imparting to it its peculiar character as a respiratory nerve. The nervous fibrillæ composing the pneumo-gastric are found, at their origin from the medulla oblongata, to be in apposition with three sources of nervous power: the corpora restiformia, or common sensory columns of the spinal cord; those fibres of the corpora pyramidalia, or anterior motor columns, which Mr. Solly has described as passing into the cerebellum; and the olivary bodies. From the first of these origins proceed the filaments for common sensation, and for "affording to the lungs, pharynx, œsophagus, and stomach that faint but peculiar sensibility which they appear to possess; filaments which impart to the stomach the sensation of fulness when that organ has been distended with food; to the lungs the 'besoin de respirer,' and the sensory functions necessary for respiration." From the second origin arise those filaments which communicate the power of motion, as exemplified in the laryngeal muscles; and from the third the portion of the nerve "constituting its specific character." The ganglionic enlargement of the pneumo-gastric nerve, which, in the human subject, is situated just beneath the foramen lacerum, is considered by Mr. Cock to be analogous to the ganglion on the posterior root of a spinal nerve; and, by dissection, he has been enabled to ascertain, in the human subject, and much more distinctly in the sheep and some other animals, that the pneumo-gastric trunk, as it leaves the base of the skull, consists of "two orders of filaments,—viz. the ganglionic and the ganglionless. The former terminate in the ganglion, where their fibrous character becomes lost, after the manner of the posterior roots of the spinal nerves; the latter are continued downwards beyond the ganglion, having merely a cellular connexion with it, and resembling in this respect the motor portion of the fifth pair. Lastly, from the ganglion arise two sets of nerves: the one constitute the laryngeal; the other join the ganglionless filaments mentioned above, and form part of the trunk of the par vagum, descending to the chest." The superior laryngeal nerve arises from the ganglion, and appears to derive some very minute fibrillæ from the pneumo-gastric trunk above the ganglion, which may be either "specific respiratory filaments or motor-muscular filaments; perhaps both." It is distributed, with the exception of a small muscular branch, and of two branches which communicate with the inferior laryngeal, exclusively to the mucous membrane of the larynx, and is supposed by Mr. Cock to communicate to that organ its exquisite sensibility. The inferior laryngeal, or recurrent nerve, which is distributed to the muscles of the larynx, is presumed to be derived from the motor root.

The ganglion of the pneumo-gastric nerve is called by the author the "superior laryngeal ganglion," as being essentially connected with the superior laryngeal nerve: its situation varies considerably in different animals, being in some nearer to, and in others farther from, the base of the skull; but in all it corresponds to the point at which the superior laryngeal nerve is given off from the pneumo-gastric trunk, that nerve being uniformly derived from the ganglion. The appellation just mentioned was first applied to the ganglion by Sir Astley Cooper, who first suggested to Mr. Cock the views which are detailed in his valuable paper.

## II. *On the Distribution and probable Function of the Laryngeal Nerves.*
By MR. JOHN HILTON.

THIS paper forms an appropriate adjunct to the one which we have just considered, as it contains an account of the distribution of the superior and inferior laryngeal nerves, the sensory and motor nerves of the organ of voice. It is not necessary to enter into the details which it presents, as the facts disclosed, and the conclusions to be drawn from them, may be summed up in a few words. It appears, from Mr. Hilton's dissections, 1st, that the superior laryngeal nerve, with the exception of a branch to the crico-thyroid muscle, and two filaments which communicate with the recurrent nerve, is distributed exclusively to the mucous membrane, cellular tissue, and glands; from which it follows that this nerve is a nerve of sensation: 2dly, that the inferior laryngeal or recurrent nerve alone supplies all the muscles which act immediately upon the column of air passing to and from the lungs, and is therefore to be regarded as the motor nerve of the larynx: conclusions which accord with the views contained in Mr. Cock's paper.

## III. *Observations and Experiments on the Lungs of New-born Children, in relation to Medical Jurisprudence.* By Mr. ALFRED TAYLOR.

THE discrepancy of opinion which still exists amongst the best medical authorities, as to the utility of the hydrostatic test in suspected cases of infanticide, induced Mr. Taylor to put it to the proof afresh on the lungs of four infants, the circumstances connected with whose birth were known. His systematic mode of investigating these cases presents a good example of the manner in which such examinations should be conducted. The questions which he endeavours to solve experimentally, and altogether independently of his previous knowledge of the facts, are the following:

1st. *What was the age or degree of maturity of the child?* The elements for the determination of this point are—the length of the body; insertion of the umbilical cord, whether in the centre of the length of the body; the weight; quantity and length of hair on the head; firmness of the bones of the cranium, facility of their overlapping on compression; development of the nails; descent of the testes; presence or absence of membrana pupillaris; state of the skin,—whether desquamating, reddened, or discoloured in any part.

2d. *Had the child lived to respire?* For the resolution of this question the condition of the body must be noted: whether full and plump, or the reverse; the development of the chest; the size and colour of the thymus gland; the prominence of the heart; the degree in which the lungs advance over the pericardium,—their colour, whether livid or of a bright red, and whether crepitant on firm compression; the condition of the ductus arteriosus and of the foramen ovale; the weight of the lungs as compared with that of the whole body, (Ploucquet's test,) and the hydrostatic test,—i. e. their specific gravity. Before investigating this latter point, we must satisfy ourselves that putrefaction has not commenced.

After separating the lungs from the heart and thymus gland, we are next to observe whether each of them float equally well. They are then to be enclosed in a cloth and firmly compressed, in order to ascertain whether the air can, as in cases of artificial respiration, be partially expelled from them. One of the lungs is next cut into ten or twelve pieces; the colour of the sections, the crepitation (if any), and freedom from disease, are noted. If these portions likewise float, compression is to be repeated on each of them, (taking care not to carry it so far as to destroy their organic texture,) and we again try their buoyancy.

One of the lungs of the first subject examined by Mr. Taylor was inflated to the greatest extent, (to a degree, in short, which would have been impracticable whilst within the chest;) when it was found to become universally of a bright pink-red colour, not distinguishable (though the child was born dead) from that of lungs which had respired; and it was strongly crepitant.

Extravasation of air beneath the surface of the enveloping membrane is a frequent, but by no means invariable, occurrence in artificially dilated lungs. In the case just alluded to, the air could not be expelled from the lung whilst entire, in such a degree as to cause its sinking; but on being cut into pieces, and on these being similarly treated, they fell to the bottom. "When the lungs have been fully distended with air by respiration, on the contrary, it is impossible to force out that air by the mechanical compression of any of the divided portions." Compression of the lung was long since proposed by Béclard, and also by some of the German medical jurists, as furnishing a diagnostic sign of artificial dilatation. Mr. Jennings and Mr. Taylor, about five years ago, and almost simultaneously, brought forward this test in this country; each being unconscious, at the time, of the other's investigations on this head, as well as of the previous ones of their continental fellow-labourers.

Very forcible inflation of the lungs of living animals has a fatal effect, as was shown many years ago by M. Leroy d'Etiolles; and Martini enumerates this amongst the modes of perpetrating infanticide: but the production of such a result implies a very great degree of violence in the operation, and such as we can hardly suppose would ever be employed where the object was legitimate,—namely, to resuscitate the child. Now it is only when the process has been carried to this extreme length that there will be any great difficulty in expressing the air from the artificially dilated lung; and, consequently, Béclard's test remains of considerable practical value. When animals have been killed by violent insufflation of the lungs, if death be immediate, the air-cells are not found lacerated; but, in those which live for some minutes, air is extravasated into the chest. The full inflation of the lungs by artificial means is not so easily effected as is commonly thought; and the more immature the child, the more difficult is it, as Béclard has remarked.

3d. *Was the child born alive?* To answer this query independently of the state of the lungs,—for a child might be born alive, and put to death before it had yet made a first inspiration,—we must examine the condition of the cord; the changes occurring in which, as diagnostic of life, are laid down at length by M. Billard, Devergie, and others: yet, as Mr. T. well remarks, these changes are often not by any means well marked, nor much to be trusted to even when they do exist, if respiration has not taken place; and, where it has, they become of very secondary importance.

In his first case, though the child was born dead, yet there was, at the junction of the sound with the shrivelled skin, a faint line of redness, like that of incipient inflammation, gradually diffusing itself towards the skin of the abdomen; and the only difference between the cord in this instance, and another where the child had survived twenty-four hours, was in the dryness and transparency of the latter. Our author is altogether sceptical as to the possibility of telling, from the appearance of this part, the exact period of survival.

A diffused redness observed about the neck and shoulders in the first case detailed, was admitted as sufficient proof of the child having been alive about the period of birth. The pulmonary vessels were in the same case distended with blood, so that those jurists are in error who look upon this as a proof of respiration having commenced.

"The mean weight of healthy lungs, calculated from their weight in six mature children which had died without breathing, I found," says Mr. Taylor, "to be 569 grains; while the weight of well-developed organs, which have fully respired, is rarely under 1000 grains." Respiration differs from all other causes which render the lungs buoyant in water, in the fact that this process increases their absolute weight, while it diminishes their specific gravity. Of all the physical changes in the lungs as evidence of respiration, the least liable to fallacy is that connected with the increase of weight. The lungs may be of a light red colour, fully prominent in the chest, highly crepitant to the feel, and very buoyant in water; yet, if their absolute weight be not raised above that of the fœtal condition, it is almost, if not absolutely, certain that respiration has not been performed.

"Ploucquet's test," says Mr. Taylor, "although very far from being infallible, is capable of serving as a good corroborative proof." The mean weight of the lungs, in three children which had breathed, was found to be to that of the bodies as 1 to 35: thus coinciding exactly with Ploucquet's results. But the ratio in regard to those which had not breathed (deduced from four cases of mature infants) was as 1 to 55; and not as 1 to 70, as laid down by the German author, who seems to have formed his conclusion from inadequate premises; namely, from two children born dead and an immature one which had breathed. M. Devergie has found the test inapplicable to children which have not reached the eighth month of gestation. In one which had lived twenty-four hours, the ratio was only 1 : 41.

Mr. Taylor has never known lungs artificially inflated to be of an uniform light red colour throughout, except when the process has been carried to its very fullest extent; and then there is commonly distinct crepitation. An uniform light red colour, without crepitation, he looks upon as characteristic of lungs which have respired.

When respiration has been imperfect, Mr. T.'s experiments induce him to believe, contrary to some authorities, that, by compression, the air may be so far expelled from the divided portions as to cause them to sink in water. In such cases of feeble respiration, the air has probably passed only into the larger divisions of the bronchi, and not into the minute ramifications and the air-cells. Notwithstanding all this, compression must still be considered a valuable adjunct to other tests: it will, at least, distinguish all cases of complete respiration from those of artificial inflation. "It must, I think," he continues, "be admitted that there are no means of distinguishing feeble respiration from artificial inflation which has been carried only to a slight extent. In a case of this kind, I apprehend, the only course left open to a medical witness, is to state to the jury that the evidence derived from experiments on the lungs left it uncertain whether the child in question had respired or not. The jury will then know how to return their verdict; for it must be remembered that they have always circumstances to guide their judgment as well as medical opinions; and it is upon the whole, and not upon a part, of the evidence laid before them that their verdict is founded." In point of fact, the plea of artificial respiration can rarely be put in with any degree of plausibility in defence of the prisoner. Mr. T. knows of but one case on record (in Bohn, and cited by Meckel,) in which the mother artificially inflated the lungs. Hence the hydrostatic test retains a high degree of practical value; and, in the rare instances where it fails, its deficiency can, at the worst, only lead to the acquittal of the guilty, and never to the condemnation of the innocent.

The invisibility of the air-cells is mentioned by most writers as a proof of the air in the lungs having been derived from respiration; whilst, where they are large and obvious to the naked eye, if decomposition has not commenced, extravasation from violent inflation has been generally presumed. This, however, is not an infallible criterion, as, in one of the cases in the present paper, "the air, although undoubtedly derived from respiration, was scattered over the whole surface of the lungs in large visible vesicles."

The absurdity of the English law of infanticide, according to which the jury cannot find a verdict of murder, provided the child, though wilfully put to death, and that too, perhaps, after it has fully breathed, was not yet altogether separated from the mother, is commented on with merited severity.

The second case detailed exemplifies the possibility of a child living and breathing for so long a time as half an hour after birth, without there being any evidence, even after a careful examination of the lungs, ductus arteriosus, foramen ovale, &c., to show whether it was born alive or dead.

The third case is likewise an interesting one, as the lungs sank in water, though life had been prolonged for six hours. The greater part of this time, however, the child was in strong convulsions, during the continuance of which it made violent efforts at inspiration. The lungs were crepitant and obviously contained air, and the cells on some parts of the surface were quite visible to the unassisted eye. After incisions had been made, so as to allow some of the venous blood with which these organs were gorged gradually to escape under water, they floated: on dividing them into portions, and compressing them anew, they again sunk. The hydrostatic test as well as Ploucquet's, and the colour and volume of the lungs, would all in vain be appealed to in such a case to decide whether the child had breathed. The ductus arteriosus and foramen ovale, moreover, were still unchanged. In short, by a rash observer, the case would most readily have been mistaken for one in which artificial respiration had been practised. Instances like this prove the absurdity of concluding positively, as is so often done, that the child was dead-born because the lungs sank in water. Desiccation and transparency of the chord are considered by Mr. T. to afford better evidence of the child having for some time survived its birth than the line of redness occasionally observed at the point of future separation. In the case just alluded to, it was found flattened, horny, and transparent; the umbilical vein being contracted and dried up, as it were, to a filament of coagulated blood.

The fourth case likewise is somewhat remarkable, inasmuch as the lungs, and even small divided portions of them, sunk in water, though the infant had survived its birth for twenty-four hours. Such exceptional cases, however, are less rare than some have supposed. Similar ones have been recorded, as Mr. T. remarks, by Bernt, Remer, Orfila, and others. Most of such instances have been furnished by premature children, but by no means all. The recent researches of Dr. Joerg, of Leipzig, are thought by Mr. Taylor to throw some light on the cause of these obscure cases. He conceives that the act of parturition, and especially its duration, seems to prepare the system of the child for the efforts it has to make in inspiration. If parturition be too rapid, the child may be born before it is yet endowed with the organic stimulus necessary to induce the first act of inspiration. "A parturition of the natural duration (on the contrary) gradually checks the placental circulation, and limits that of the fœtus chiefly to its own system; while it engenders in the latter a gradually increasing and finally an urgent want of some new mode of inspiration. Supposing the first inspirations to be, from any cause, feeble or imperfect, then the organs will become only partially distended: the remaining portions will preserve their fœtal condition. Dr. Joerg considers this as a positively diseased state of the lungs in the new-born child, and he has given to it the name of *atelectasis*. Into its supposed causes we have not room to enter. Dr. J. has remarked that those portions of the lung which are not speedily distended by air afterwards become consolidated or hepatized, so that all traces of their vesicular structure are lost. The length of time which the child survives will depend upon the degree to which its lungs have become dilated. Mr. Taylor alludes to a case which he met with himself, in which the child survived for six months with a large portion of the lung in this unexpanded or fœtal condition.

To conclude: though the pulmonary tests do sometimes fail us in our endeavours to prove live-birth, yet such cases are merely exceptional. In a very great majority of instances, the signs thus furnished are so decisive as to leave us nothing to wish for. Thus, if "in the body of a healthy full-grown child, which has but recently died, we find the lungs filling out the cavity of the chest, of a light red colour, spongy and crepitant beneath the finger, weighing at least two ounces, and, when divided into numerous pieces, each piece floating in water, even after violent compression, is it possible," asks our author, "to doubt that respiration has been performed?"

To discard such tests altogether, as some would have us, merely because they are not in every case infallible, would be just as wise as to disregard the ordinary chemical and pathological proofs of poisoning because these are found in some instances unsatisfactory. We need hardly add, after this analysis of Mr. Taylor's paper, that it is most valuable, and highly deserving the attention of every medical jurist.

## IV. *Cases by* Mr. BRANSBY COOPER.

*Gangrene of the Hand.* A carpenter, aged eighteen, of delicate constitution, struck the end of the nail of one of his fingers against a piece of lead: there was only a slight ecchymosis under and above the nail. On the third day, a numbness was felt in the finger adjoining the injured one; and, before a fortnight, all the fingers of that hand were in the same state—numb and bloodless, but painful; the pain complained of being principally referred to the ends of the fingers. During all this time he suffered pain in the course of the median and external cutaneous nerves, but the general health remained unaltered. On admission (October 7,) three weeks after the accident, he was very weak; his hands and feet cold. The pulsation of the brachial artery, high up, was cord-like; at the bend of the elbow, it was undistinguishable; and, in the centre of the fore-arm, it could be felt again in the radial, but not in the ulnar artery: in several places the artery was tender to the touch. A rough, sawing noise could be heard by the stethoscope in the subclavian artery, at a spot where the vessel was pressed forward by a small tumour, probably an enlarged lymphatic gland. The fingers and thumb were livid, cold, and insensible; the end of the injured finger in the condition of dry gangrene: no attempt, as yet, at a line of separation. During a week he had bad rest; quick, small pulse; headaches; sickness of stomach, and even hiccup: the hand and fore-arm swollen, pitting on pressure, painful, and marked with blue lines in the course of the veins. On the 20th, the line of separation commenced on the dorsum of the hand above the roots of the fingers. From this time both the local and constitutional symptoms began to abate. On the 31st, the line of separation had gone through all but the tendons and bones; and, on the 11th November, the hand was removed by Mr. Cooper at the radio-carpal articulation. Henceforward the patient rapidly convalesced; the gland in the axilla disappeared, and the pulsation in the subclavian artery became much more natural. January 6th, discharged cured. It is not stated whether the brachial and ulnar arteries regained their pulsation after the cure.

The recovery of this patient leaves us nothing but speculation as to the cause of the gangrene. Can we believe, with Mr. Cooper, that the mechanical pressure of the enlarged gland on the axillary artery could have produced such an effect? Scarcely. Even he himself regarded this enlargement as secondary to the gangrenous inflammation. To say that it may have been the consequence of "a general constitutional derangement of the vascular system," is only to apply a vague expression, which we cannot well understand, to the explanation of an incontrovertible fact. Or, to suppose that the trifling injury sustained in this case, even though accruing to a limb while under the influence of both these assumed conditions produced it, is bringing us little nearer to the mark. What then was the cause? Mr. Cooper alludes to the possibility of the presence of *arteritis*, but at the same time gives his objections against such, as the cause of the gangrene: objections, however, in the validity of which we do not fully concur; for we have seen a case of *spontaneous* gangrene of the hand taking a course very analogous to that related by Mr. Cooper, in which intense inflammation of the axillary and brachial arteries was the real and only cause, though scarcely suspected during life. We are, therefore, inclined to consider the gangrene in question as the consequence of arteritis.

*Popliteal Aneurism;—Relapse after Operation.* An Italian gentleman, aged thirty, consulted Mr. Cooper for an aneurism in the left popliteal space. Eight months previously, he had been operated on by a surgeon in Manchester for the same disease. The pulsation in the tumour had, on that occasion, ceased as soon as the ligature was applied, but returned in twelve hours after, and has continued slowly increasing ever since. Mr. Cooper tied the vessel close above the tumour. "Immediately the ligature was applied to the artery, the pulsation in the tumour ceased, but in a few minutes, became again as distinct as before the operation:" however, "the patient had entirely lost the painful sensation in the limb which existed prior to the operation." Considerable febrile reaction followed, with pain, tension, and throbbing in the wound and tumour. On the third day, the pulsation in the tumour again ceased, and the sac felt empty, as if no coagula had existed in it. Up to the eighth day, some fluid blood could be felt in the sac, though admitting of being squeezed out by pressure. On the twenty-second the ligature came away. An inordinate action of the heart and arteries, which, from the time of the operation, disturbed the patient, and gave rise to apprehensions of internal aneurism, subsided completely by a fortnight's residence at the seaside.

Why did the first operation in this case fail? Without supposing that there was any error committed in its performance, (for there is every reason to believe that *an* artery was tied,) many explanations of the cause of the failure may be offered. In the *first* place, there may have been a double femoral artery with reunion of the branches above the aneurismal sac, as noticed by Sir C. Bell, in Anderson's Quarterly Journal; and by Dr. Houston, in the Dublin Hospital Reports: this is the most probable explanation. *Secondly*, the return of the pulsation may have arisen from an enlargement of the anastomoses connecting the profunda with the anastomotica magna; though the earliness of its reappearance can scarcely be accounted for in this way. *Thirdly*, a branch arising from the femoral, high up, and either rejoining that vessel or opening directly into the sac itself, as observed by Sir A. Cooper and Scarpa, might have perpetuated the pulsation.

As regards the temporary recurrence of pulsation, after the second ligature, it may be accounted for on the very probable supposition that an enlargement of the anastomosing branches had been brought about by the first attempt, sufficient to carry, for a time, a small quantity of fluid blood into the aneurismal sac. The rapid subsidence of the pulsation almost justifies the inference that none but collateral vessels were engaged in its production.

*Two Cases of Hernia.* Mr. B. Cooper is an advocate for relieving the stricture without opening the sac. In one of two cases which he relates the practice succeeded well; in the other it comparatively failed. The first patient had no secondary bad symptoms; the second was near dying. Mr. C. states that, in five cases on which he operated in this way, "his patients suffered no more after the operation than they usually do after the successful application of the taxis, while, on the contrary, if the hernial sac be opened, and the peritoneal cavity necessarily exposed, a high degree of constitutional derangement, and subsequent prostration of power, almost always followed." It is but right to add, that Mr. Cooper and his colleague, Mr. Key, differ in opinion on this point of practice. Mr. C. considers that, in the first of the cases reported, the patient obtained immediate and permanent relief, *because* the sac was not opened; and that, in the second, dangerous symptoms followed, *because* it was opened. But, in coming to such a conclusion, the relative positions of the two patients should be fully weighed; as the great secret of success in such cases lies in early operation. Now, it appears, that, in the cases related by Mr. Cooper, the operation in the first was performed on the second day after the occurrence of the strangulation; whilst, in the second, it was delayed to the fifth day. We do not attempt to disapprove of Mr. Cooper's method, but nevertheless we are not satisfied that these two cases, with such differences between them, can settle the point at issue. Regarding the five other cases alluded to by Mr. Cooper, we should have presented to us five of precisely a similar kind treated in the usual way, before we can take them in evidence of the superiority of his peculiar mode of practice. There is every reason for believing that the plan is good: all we ask is, a comparative demonstration of its utility.

*Ununited Fracture of the Humerus; cured.* A healthy young woman with ununited fracture of the humerus, of six months' standing, was admitted into Guy's Hospital. Mr. Cooper, judging from her report that the ordinary means of cure had been used properly, though without success, produced friction of the ends of the bones on each other for ten days; but no union followed. He then passed a seton through the new joint: "the seton was worn ten weeks, but the irritation which, for the first few days held out a prospect of success, soon subsided; and proved ultimately to have led to no happier result than that of the former attempts." A yolk-of-egg bandage was then applied, and persisted in for six weeks, but equally without success; and, as a last resource, an envelopment of plaster of Paris was had recourse to. About the fourth month after admission, when there was no attempt at union, Mr. Colles, of Dublin, who happened to pay a visit to the hospital, suggested the use of mercury. Hydrargyrum c. creta was administered, (four grains, three times a day.) Ptyalism supervened in four days. "In a month, perfect union of the bone had taken place, affording satisfactory proof that the mercury had produced an altered action of the capillaries of the affected part, and exemplifying the alterative influence of that metal." About five months afterwards the same arm was broken, below the seat of the original fracture, and again united in the usual time and under ordinary treatment.

*Wound of the Tongue; fatal.* A tobacco-pipe in a young man's mouth, being accidentally struck in the bowl by a comrade, penetrated and wounded the tongue and pharynx. It was not considered at the time that any part of the pipe, though broken, remained in the mouth. On the sixth day, he discharged apparently from the stomach, about a pint of grumous blood. This discharge recurred repeatedly from the eighth to the twelfth day. The patient became extremely reduced, "but no wound could be discovered to point out from whence the hemorrhage proceeded." He sank fast from repeated discharges of coagula from the stomach, "which reduced him to so low a state that transfusion was contemplated." He died on the fifteenth day, almost immediately after throwing up coagulated blood. On examination, a cicatrix appeared on each side of the tongue, and "a small irregular opening was discovered behind and below the left tonsil." Two inches and a half of the end of the pipe was found imbedded in the substance of the tongue. Blood was found in the stomach and bronchial tubes. The source of the hemorrhage could not be ascertained; but there can be little doubt that the blood had flowed from the laceration in the pharynx, (and not from the wounds of the tongue, which were cicatrized,) and had thence trickled down into the œsophagus and trachea. Mr. Cooper hints that, had the source of the hemorrhage been known during life, a ligature on the carotid might have saved the patient.—Nothing more likely.

*Stone in the Bladder.* Mr. Cooper extracted a calculus from a child, seven years old. The calculus was oval-shaped, and about an inch long. The outer layer consisted of bright, transparent crystals of oxalate of lime, of an octohedral form, with rectangular base; the next, or subjacent layer, of oxalate of lime, with a minute proportion of phosphate of lime; and the third, of carbonate and phosphate of lime, with lithic acid. The deeper layers and nucleus were not examined.

Every one of Mr. Cooper's cases contains some useful practical information; but, for our own sake, and for that of every other reader, we beg that, in future, his cases may be more condensed: few men can wade through three or four reports a day of any disease. This is, however, no farther a fault of Mr. Cooper, and of others whom we might name, than that they are content to print the notes of their pupils instead of their own. Far be it from us to discourage the practice of note-taking by pupils: on the contrary, we believe it to be the best of all exercises in which they can be engaged. All we require is, that medical men, in giving to the world the results of their experience, shall give their own thoughts in their own language. Proxies, in such cases, are apt to be injudicious by their prolixity.

Although not strictly in order, we will here notice a case by Mr. Key, related in another part of the volume.

*Sphacelated Intestine and Omentum in Femoral Hernia.* A lady, aged forty-four, consulted Mr. Key for a femoral hernia. He found it to consist of a small portion of omentum, irreducible from adhesion to the sac. A hollow pad with a truss relieved, for a time, the apprehension and uneasiness. Several months after, the tumour became suddenly enlarged and painful. The taxis failed in giving relief, and the patient would not consent to an operation until five days had elapsed; when the abdomen was hard and tender, the pulse 100, the matters vomited fæcal, and the coverings of the tumour inflamed. As soon as the sac was punctured, a dirty brownish fluid oozed out, and a darkened mass of omentum, in an advanced state of decomposition, was discovered. Beneath this lay a knuckle of intestine, with a patch of sphacelus about one inch in diameter, but without any line of demarcation between the dead and living parts. Having divided the stricture, Mr. Key pressed the intestine gently back towards the abdomen, leaving the major part external to the neck of the sac. The omentum, too, was left in the sac, and the whole covered with a poultice. Little amendment followed for some time. On the fifth day from the operation, a discharge of fæces took place from the wound; on the seventh, the sloughs had cleared away; on the seventeenth, a few hard balls of feculent matter passed, for the first time, from the rectum. In six weeks the wound was completely closed over, and the fæces had taken their natural course. The cicatrix opened slightly on two occasions, but it soon became permanently closed. "The patient continues well to the present time, wearing a truss as a matter of precaution."

In the individual in question, Mr. Key considered that the vomiting had so far emptied the stomach and intestines as to remove the necessity for any further abstraction of their contents by incision. He apprehended that a state of collapse, such as he had often seen follow the free evacuation of the canal by a proceeding of this kind, might have changed his patient's present elastic state to one of depression; and he judged that nature would work her object with more security if allowed a respite after the division of the stricture, than if disturbed by the operation of incision into the mortified intestine. As regards the treatment of the mortified omentum, many surgeons adopt the same practice as that recommended by Mr. Key; but the plan of leaving the deadened portion of the bowel to empty itself by the separation of the sloughs differs from that usually adopted. How far such a plan is to be acted upon as a general rule remains yet to be determined.

### V. *On the Diagnosis of Organic Diseases of the Uterus.*

### By Dr. ASHWELL.

THE avowed object of Dr. Ashwell's paper is to render more easy the study of this complicated portion of pathology. He does not profess to put forward much that is new; his aim being classification, not discovery. The methods of enquiry he arranges as follows:—the history of the symptoms; examination by touch; the application of the speculum; the stethoscope; and the investigation of the discharges. In regard to the value of pain as a symptom, he justly observes that, "in chronic structural disease, there is generally little acute, early, or continued pain; while in functional disorders, such as irritation and inflammation, these are invariable conditions." The duration of pain, as also the existence or not of emaciation, as signs in themselves of organic disease, he considers, though essential to be attended to, yet incomplete without the addition of what he states to be the most valuable means of diagnosis,—viz. examination by touch, more especially when aided by the speculum and stethoscope. The directions for conducting the examination by touch are good: he dwells at some length on its efficacy as a means of diagnosis in cases of organic diseases complicated with pregnancy or doubtful pregnancy, but does not appear to us to attach a sufficient degree of importance to the aid afforded by the stethoscope in these investigations. The uterus being liable to be distended or enlarged from various causes, much stress is laid on the vaginal examination. We quite agree with the author in his remarks on the peculiarities or non-morbid varieties of the cervix and os uteri. A large uterus, especially at its lower part, a large and soft cervix, a patulous os, fissured, indurated, and cicatrized, may all exist, Dr. Ashwell says, without organic, and especially active organic disease. To the latter part of this proposition we assent, but do not feel so satisfied with the correctness of the former.

The remarks upon the speculum are short, and contain nothing new: the author has selected a favorite form of instrument, and of course likes it best. It is a conical tube, made of tin, with a highly polished inner surface: he uses a series of these of various sizes, and judges of the size best adapted to the individual case from the previous introduction of the finger. The stethoscope, as an aid to diagnosis, is dealt with very concisely. He seems afraid of reposing too much confidence on the "placental souffle," considering it liable to be confounded with a *bruit* in a large artery, the result of pressure, and *vice versâ;* and yet in the two cases he adduces, one of pregnancy, and one of hard tumour of the uterus, he states that this sign (that is, the presence of this "bruit,") did not embarrass the diagnosis: the bruit ceasing with the altered position of the tumour.

Dr. Ashwell objects to a diagnosis derived from an investigation of the discharges simply, and with justice; but, as he purposes again treating of this subject, we shall reserve any remarks we might feel disposed to make for a future opportunity.

## VI. *Observations on Abdominal Tumours and Intumescence.* By Dr. BRIGHT.

THIS is a most admirable paper; but, as it is only the first of a series of the same kind, we shall postpone the analysis of it to a future occasion. The following is the general outline, given by Dr. Bright, of the various subjects which he purposes to comprehend in his survey of this most interesting field of pathological and practical enquiry. It is gratifying to know that the powers of the enquirer are commensurate with the difficulties of the subject of investigation.

"1. The Integuments; including various cutaneous changes, polysarcia—anasarca—malignant deposits—abscesses—protrusions.

"2. The Peritoneum—the various results of inflammatory action; as effusion, including ascites—adhesions, and various depositions of adhesive matter—tubercular deposits—malignant diseases—hydatids.

"3. The Stomach; including flatulent distention—chronic disease—malignant changes.

"4. The Intestines; including flatulent distentions—retained fæces, and other matters—mechanical obstruction—malignant strictures.

"5. The Liver; enlarged from congestion—forced down by the lungs or by effusion—distended with bile—enlarged by various changes of the structure generally—enlarged by malignant growth.

"6. The Spleen; enlarged by congestion—changed in structure.

"7. The Pancreas; enlarged, or hardened.

"8. The Mesenteric Glands; simple enlargement—scrofulous, malignant, and osseous changes.

"9. The Kidneys; enlarged by vesicles, by abscess, by malignant disease—distended ureter.

"10. The Bladder; distended—thickened—forced forward.

"11. Uterus; enlarged from pregnancy—chronic increase—scirrhous disease, and other changes.

"12. Ovaries; enlarged by simple cysts—by malignant growths.

"13. Extra-uterine Fœtation.

"14. Aneurismal Tumours; cæliac—aortal—iliac." (p. 433.)

## VII. *On the Influence of Electricity as a Remedy in certain Convulsive and Spasmodic Diseases.* By Dr. ADDISON.

DR. ADDISON confesses that he formerly attached as little value to electricity as a remedial agent as is ascribed to it by the profession in general, being "led greatly to underrate its efficacy, either in consequence of its vague and indiscriminate recommendation, or from the inefficient and careless manner in which it had been applied." The convulsive and spasmodic disorders of females, in which, under a happier direction of its use, he has, with the assistance of Mr. Golding Bird, recently and successfully employed it, were, for the most part, such as are connected with some irregularity in the menstrual discharge.

The mode in which electricity was employed was either by taking sparks along the course of the spine, or in the form of shocks passed through the pelvis. In the former case, the patient being insulated, the sparks were taken in rapid succession, at the distance of about an inch, for about five to ten minutes, until an eruption followed, which closely resembled Lichen urticarius. In the latter case, a large-sized Leyden jar was discharged through the pubes and sacrum, the intensity of the charge being regulated by the electrometer. In one case only, when the patient's strength was not sufficient to admit of the ordinary mode, the magnetic-electrical machine was substituted. The seven cases detailed are, as we are informed, not selected ones, but all those indiscriminately in which he has lately tested this remedy; so that they may be considered as affording a fair estimate of its value in such affections. In the first case, (the only one which we have space to enter into,) a girl of seventeen, well developed, but rather of a nervous temperament, who had been from the age of fourteen the subject of painful menstruation recurring every fortnight, on being frightened during one of her periods, experienced a sudden interruption of the evacuation, which was followed by hysterical fits, with continual trembling of the limbs. Pain in the head, back, and loins supervened, along with palpitations. Her mouth became liable to frequent distortions, and the limbs, the upper ones especially, were violently agitated; the hands revolving round each other at regular intervals, in a very systematic manner. These symptoms, after having been long ineffectually combated by a host of remedies, including bleeding, blistering, purgatives, creosote, prussic acid, sulphate of zinc, carbonate of iron, cold affusion, &c., yielded, in a great degree, to the sulphate of iron in half-drachm doses, aided by a combination of calomel, colocynth, aloes, &c., and the use of the shower-bath. Two months after leaving the hospital convalescent for the sea-side, she returned in a worse state than ever; epileptic paroxysms, accompanied with episthotonos during the fit, having taken the place of the chorea. The legs appeared paralytic, and the amenorrhœa persisted. The same tonic and purgative remedies which had formerly been used with success were again employed, but now without benefit. Electro-magnetism was had recourse to; and, after three weeks of its employment, her general health was much improved: she could now hold her needle, and the fits had become slighter, though not yet less frequent. Electrical sparks were now drawn from the spine on alternate days, each exhibition inducing a vivid eruption. In a week she was able to walk across the room unassisted; her countenance was less anxious, and the fits less frequent. Twelve shocks through the pelvis, every second day, were next substituted: their very first employment was followed by severe abdominal and pelvic pain, and by the reappearance of the menses. In the following month, the uterine evacuation was recalled by a repetition of the same means; soon after which she left the hospital entirely free from chorea, though still subject to fits of diminished force and frequency: a result which, though not altogether satisfactory, proves at least that the agent employed was one of no inconsiderable efficacy.

In the second case, that of a girl of fourteen, for years afflicted with chorea, a complete cure was effected by taking sparks from the spine for the space of three weeks. The third case was a similar one, in a boy of the like age, and the result equally favorable, and as rapidly attained. In the fourth, a girl of sixteen, the menses being suddenly interrupted by cold, an hysterical attack supervened, accompanied by a sense of numbness and coldness in the left side of the body. The left eye soon afterwards became amaurotic, and the lid dropped; there was defect of muscular power over the whole left side, along with headach and giddiness. After cupping, blistering, and aperients had failed, electricity, in the form of sparks, was ordered. The power over the eyelid and limbs was soon regained, and the menstrual function reestablished. The amaurosis, however, was permanent. The fifth case, which somewhat resembled the first, though much milder, had been considerably benefited by electricity, but was still under treatment. In the sixth and seventh cases, both likewise instances of chorea, electricity was quite successful.

The above cases are certainly of a nature in some degree to revive our expectations from electricity employed as a remedial agent, when used in appropriate circumstances, and with sufficient energy and perseverance. We trust it will lead others who have extensive hospital opportunities to test it still more fully in cases of the kind just described. The period which seemed necessary to the production of its full effect varied from three to six weeks. For fuller details as to the precise mode of its exhibition, we refer to the paper itself.

The remaining papers we are compelled to pass over with little more than a mere notice of the subjects treated of.

VIII. *Case of Disease in the Fœtus.* By Mr. T. W. King.

IX. *Description of the Sacculus or Pouch in the Human Larynx.* By Mr. John Hilton.

X. Under the head of "*Ophthalmic Cases occurring at the Guy's Hospital Eye Infirmary,*" we have two instances of imperfect amaurosis (chronic retinitis), treated by Mr. Morgan, in which inunction of the temples, night and morning, with the ointment of veratria, coupled, in the first case, with fractional doses of the extract of the same plant taken internally, appeared to be useful: but, as this remedy was used in combination, in one or other of these cases, with such powerful auxiliaries as cupping, mercurials, and continued purgatives, it would require much more extensive and unequivocal premises to enable the medical public to decide on the efficacy of veratria in amaurotic disease. We think, however, evidence sufficient has been already brought forward to justify, if not very strong hopes as to its utility, at least further and fuller trials of this powerful agent. Besides the gradual improvement of vision, it is stated to cause a sensation of pricking in the temples, and an increased frequency of the flashes of light.

## XI. *Experiments and Observations on Albuminous Fluids.*
By Dr. Babington.

In this paper the changes which the fluids in question undergo by a union with the pure alkalies, and also with the neutral salts, are investigated. On the addition of a strong solution of pure potass, for example, to pus or to white of egg, a transparent, tenacious, gelatinous mass is formed, which is very little soluble in water, and is not precipitated by dilute nitric acid. With serum, also, a thick tenacious fluid is the result; approaching less, however, to the solid condition than in the preceding instances. Dr. B. considers that we may turn the above facts to account in practice; as, for instance, in those cases where we are in doubt as to a urinary deposit, whether it be purulent or not. In the albuminous urine of renal disease, however, he has hitherto in vain endeavoured to produce the above appearance.

It was long ago stated by John Hunter that pus is coagulated by sal ammoniac: but, in the possession of this power, the hydrochlorate of ammonia does not stand alone; for Dr. B. finds that the hydrochlorate of soda, nitrate of potass, the sulphates of magnesia, of soda, or of potass, all equally form viscid compounds with pus. Dr. B. conceives it probable that natural mucus is formed by some combination analogous to that which results from the action of a pure alkali or neutral salt on pus or albumen. But, as Dr. B.'s experiments and conclusions have been in a great degree anticipated by others, (a fact of which he became aware only just before publication,) we do not think it necessary to enter further into them here.

Since this article was sent to the press, we have received the Sixth Number of the "Reports," the notice of which we must postpone to a future opportunity. We are delighted to perceive that the medical officers of Guy's Hospital bid fair to falsify our prophecy in a former Number, that the supply of materials worthy of the high reputation of the "Reports" cannot be long expected to continue. The Number just published seems worthy of its predecessors, and of the distinguished institution whence it emanates.

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## ART. IV.

*Ueber Virilescenz und Rejuvenescenz thierischer Körper. Ein Beitrag zur Lehre von der regelwidrigen Metamorphose organischen Körper.* Von Dr. Carl Wilhelm Mehliss, praktischen Artze, &c. in Liebenwerda.—*Leipzig*, 1838.

*On the Virilescence and Rejuvenescence of Animals: a Contribution to the Knowledge of the regular Metamorphoses of Organic Bodies.* By Dr. Charles William Mehliss, of Liebenwerda.—*Leipzig*, 1838. 8vo. pp. 114.

In animal bodies there are three distinct periods or epochs,—viz. of formation, of growth, and of decay. The less perfect animals cease to exist as soon as they have attained their full growth, and become capable of propagating their species: the act which marks the perfection of their development is but the prelude to their destruction. In the higher animals, however, a longer or shorter period elapses after they are no longer capable of procreating their species; a period of gradual decay and progressive exhaustion of all the powers of life. This period is longer in man than in any other animal, and presents some phenomena which are well deserving of attention. Dr. Mehliss has chosen two of these as the subjects of his work: the assumption of the characters of the male by aged females, and the reappearance of the characteristics of youth in the aged of both sexes: to the former he gives the name of *Virilescence*, to the latter that of *Rejuvenescence*. He might likewise have added another new term, indicative of the appearance of some of the virile characters in the female, as he has noticed this circumstance in his book; and he might have named this condition *Feminescence*, from the same analogy on which the other terms are framed.

On the subject of Virilescence, the author quotes the authority of Hippocrates and Aristotle, and traces the origin of the fables of the middle ages concerning a "mutatio sexus" to the facts which they have related, and to the mythological sayings of the Grecian poets: yet such a change was not entirely without support from ascertained facts: old women had been seen with beards, and their voices had assumed the manly character; persons who had been educated as females had been seen to assume all the attributes of men; and others, who as women had remained barren in marriage, became the fathers of children. Such instances as these, which a careful enquiry would have robbed of all their value as facts, were strengthened by the opinion of Galen that the parts of generation are the same in both sexes, and vary only in position, the same parts being situated externally in men which are placed inwardly in females: by that of Aristotle, that a woman is merely an imperfect man; and by the scholastic dogma, that nature, under all circumstances, aims at perfection. Eusebius Nieremberg and Ulysses Aldrovandus had done their best to confirm these errors, by tales of exotic animals in which this conversion of sexes was a natural characteristic; and the doctrine was not entirely abandoned, even by anatomists, before the end of the sixteenth century. The seventeenth century, however, saw the error completely dispelled; and since then the whole subject of mutation of sex, instead of being a tissue of fables, has become an interesting branch of natural history.

*Virilescence.* Those appearances which our author includes under the general name of virilescence occur in females in whom one or more of the peculiar characteristics of their own sex have passed away, and consist in the assumption of some peculiar qualities of the male. The assumed characters differ in different cases, but agree in bearing a close resemblance to some part of the male frame, other than the organs of generation themselves. Virilescence, therefore, can take place only in those animals which continue to live some time after the power of generation has been lost, and in which the sexual differences are sufficiently strongly marked, not merely in the organs of generation themselves, but in other parts of the frame. Consequently, it cannot occur in vegetables; for the difference of sex is here only marked in the sexual organs themselves. Kob, indeed, has compared the regular metamorphoses of the organs of generation of plants into other parts, (as, for instance, of styles into stamens, and the occurrence of male blossoms on the female plants of the class Diœcia,) with the appearance of virilescence in animals; but these deviations from the normal condition are original, and do not admit of comparison with the animal phenomena, which distinctly require the complete development of the sexual character at a former period of life. This condition is fulfilled only in the more perfect animals,—in birds, in the mammalia, and in man. In insects, the sexual difference is sufficiently clearly marked in the external conformation of their bodies; but their life, closing with the process of generation, is too short to exhibit the phenomena of virilescence. In some of the Crustacea, in fish, and in amphibia, observation will probably yet detect the existence of these changes. In all animals, however, and under all circumstances, virilescence is extremely rare.

In birds, the chief distinction between the sexes, after the differences between the parts of generation, consists in the plumage, which is more developed, and possesses more lively and varied colours, in the male than in the female. The size of the body, the character of the voice, and the occurrence of spurs on the feet, establish other, but less striking, distinctive marks. In each of these particulars, the female bird may simulate the outward appearance of the male, and to such an extent that a careful observation shall scarcely distinguish the sexes. Our author collects abundant proof of this fact from undoubted authorities: of these, the greater part occurred in the domestic fowl and common pheasant; others in other species of pheasant, in the turkey, peacock, and common duck. The partridge, wood-pigeon, starling, bustard, chaffinch, and four or five other birds, have been observed to assume the same peculiarities. The change of plumage was the most remarkable, as well as by far the most frequent, phenomenon; the alteration of the voice was nearly as common; the growth of spurs, comb, &c., is reported in more than one case. These changes began after the bird had ceased to lay eggs, and became more marked as it grew older. In some instances the transformation was so complete, that not only was it difficult to make a distinction between the real and the assumed sex, but the difficulty was scarcely less for birds of the same species; especially as the deception was increased by the masculine behaviour of the transformed birds. In a few cases, eggs were detected in the ovary; and, in a large proportion of instances, the parts of generation were found partially or entirely wasted away.

In mammalia, the signs of virilescence are neither of so frequent occurrence nor so strongly marked as in birds; and hitherto they have been seen only in the stag and roe. The male of these animals, as is well known, is distinguished by horns, of which there is no trace in the female. These parts make their first appearance with the first complete development of the organs of generation; fall off annually, at the conclusion of one rutting season; and are again reproduced, in a more perfect form, at the beginning of the next. Virilescence consists chiefly in the growth of these parts in the female: it shows itself, however, occasionally in other parts; for instance, in the hair. More than one of these changes have been seen in the same animal. In some instances both horns were produced; in others, only one, and that usually on the right side. Two instances only are adduced in which the phenomena of virilescence showed themselves in the hair of the animal. In one case, the hair of the head, neck, and abdomen, the shape of the ears and extremities, and the odour of the animal, gave it the closest possible resemblance to the male, and it followed the other females as if urged by sexual desire. Valmont de Bomare has described an animal which had one horn on the left side, and organs of generation closely resembling those of the male; the ovaries hung down like testicles, the clitoris was elongated, and the vagina contracted. In three instances, the animals were in calf. The case quoted from Valmont de Bomare is the only one in which the parts of generation underwent those changes which take place so frequently in birds. The statement that horns have made their appearance on the heads of animals which do not usually bear them,—as, for instance, the hare,—is justly treated as a fable; and the growth of horny excrescences on the skin of men and animals is shown to have no connexion with the phenomena of virilescence. The change of the colour of the skin, too, which is observed to take place in old animals of both sexes, is shown not to be allied to virilescence.

In the human species, the phenomena of virilescence consist in the growth of hair, partly on the face, in the form of a beard, and partly in other situations where, in ordinary circumstances, it does not exist to any extent, even in men; and in a strengthening of the tone of the voice, so as to resemble that of the male. The facts which are quoted by our author are not very numerous, amounting only to eight detailed cases and a few references to others. With the exception of the case of extirpation of the ovaries, related by Pott, the phenomena of virilescence are referrible to suppression of the menstrual discharge, occurring as well at an early period of life as in more advanced age. The growth of hairs on the upper lip in young persons, a short time before the appearance of the menstrual discharge, is regarded by the author as an event of not unfrequent occurrence.

The following general conclusions are drawn by Mehliss from the facts adduced by him:

1. The changes which take place in animal bodies during virilescence are either organic or dynamic. To the latter belong the changes of the voice and of the sexual characters. The former are of two kinds: in some cases a degeneration of structure occurs, as the gradual wasting away of the sexual organs; in other instances there is an actual development of parts which, in the natural state, exist only in males. The new growths which thus take place, either already existed in a rudimentary form, and are merely more fully developed, (for example, the comb in the common fowl, hair in women, crista pectoralis in the turkey;) or they already existed in the female under a different form, and merely undergo a change, (for instance, the feathers of birds;) or, lastly, they were originally absent, and must be considered as entirely new formations, (as horns in the hind, spurs in the common fowl.)

2. The changes take place gradually, obey the laws to which the development of the same parts in the male is subject, and follow the same order.

3. In all the individuals which have exhibited the phenomena of virilescence, the characters of their own sex were already fully developed; the greater number, too, had been fruitful; and it is only in some individuals of the human species that the power of reproduction has been wanting.

4. The appearance of virilescence was, in all cases, attended either by an entire loss, or a remarkable diminution of the power of reproduction; and, as the virilescence increased, the power of reproduction diminished, at length entirely disappeared, and never returned.

5. On the other hand, virilescence seems to have commenced, in most instances, before the usual period of the disappearance of the sexual functions; so that it did not begin with the commencement of *old* age but a longer or shorter time before it. In men, and in the mammalia, it occurs proportionably earlier than in birds.

6. All the individuals in whom virilescence has been observed have been placed under favorable circumstances. They all appear to have been remarkably robust, powerful, and healthy, and to have reached a good old age.

7. In no case was the health of the individual impaired by virilescence; nor was the occurrence of the phenomena accompanied by feelings or symptoms of sickness.

*Feminescence.* After closing his account of Virilescence, the author notices more briefly the reverse condition, or the assumption of some of the male characters by the female, which we might term *Feminescence.* The two principal circumstances noticed under this head are, the appearance of the catamenia, or, at least, of a sanguineous discharge in the male; and the secretion of milk by the male breast. Our author quotes the authority of the ancients for the occurrence of the first-named phenomenon; but not a single credible instance, as might be believed, is adduced. The most curious statement under this head is the assertion by certain authors that the catamenial lustration from the penis was inflicted on the *Jews* as a divine punishment! On the other hand, the secretion of milk in the breast of males is a fact sufficiently attested, both by ancient and modern writers. Here, also, credulity has added her legends. In the sixteenth century, some missionaries in Brazil asserted that there was a whole Indian nation whose women had small and withered breasts, and whose children owed their nourishment entirely to the males! It is, however, true that one of the best authenticated instances of this fact occurred in a South American, and is related by Humboldt. Francisco Lozano, aged thirty-two, a peasant of a small village in Cumana, nourished his child with his own milk. His wife, immediately after her delivery, fell sick; and Lozano, in the hope of quieting the child, applied it to his breast. A secretion of milk took place, which gradually increased until it afforded sufficient nourishment for the infant. Humboldt and Bonpland were assured by eye-witnesses that during five months the child took no other nourishment whatever. Humboldt saw both the father and son; and states that the breasts of the former closely resembled those of a female. We may add the following very similar fact, on the authority of one of our most enterprising travellers. A young Chipewyan lost his wife in her first pregnancy: he applied the infant to his breast, to still its cries; and "the force of the powerful passion by which he was actuated, produced the same effect in his case as it has done in some others which are recorded: a flow of milk actually took place from his breast." "Our informant, Mr. Wenzel, added that he had often seen this Indian in his old age, and that his left breast even then retained the unusual size it had acquired in his occupation of nurse.*" Some remarkable examples also are given of male animals who had given suck either to the young of their own or of other species, or had furnished milk to man. The occurrence of this abnormal secretion was in no case accompanied by a change in the functions or structure of the parts of generation.

We cannot follow our author in his examination of the somewhat analogous, but really very different subject of *Hermaphrodism:* neither can we give his discussion on the causes of the singular change to Virilescence. We give the conclusion to which he arrives, in his own words, and we hope it may be more satisfactory to the reader than it is to us. "Virilescence," says Dr. Mehliss, "like many other degenerations to which organic bodies are liable, is a means of compensating a disproportion which exists in the organism between the energy of the vegetative life of the whole body and that of individual organs."

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* Franklin's Voyages, vol. ii. p. 54.—*Richardson's Journal.*

*Rejuvenescence.* The idea of the assumption by aged persons of the characters of youth was familiar to the ancients, and probably formed the groundwork of the fable of Medea and Æson. Pliny and his successors have related instances of the kind, and modern writers have added to our stock of facts. No one, however, we believe, before Dr. Mehliss, has attempted to collect the scattered cases to ascertain their credibility, and thus to make the whole subject a branch of physiology. He arranges the phenomena of rejuvenescence under five distinct heads:—1. The secretion of milk by aged females. 2. The return of the menstrual discharge. 3. The cutting of teeth in old age. 4. The growth of hair similar in colour to that of the young. 5. The sharpening of the intellect, and restoration of the vivacity of youth to the old.

It would occupy too much space were we to notice, however briefly, the various facts which are here detailed; and we must refer our readers to the original work. The subjoined table shows, at one view, the number and nature of the principal facts.

| | Number of instances of | | | |
|---|---|---|---|---|
| | Dentition of the aged. | | Lactation by aged females. | Menstruation of aged females. |
| Age of the individuals. | Males. | Females. | | |
| Between 40 and 50 | 0 | 4 | | |
| — 50 ... 60 | 1 | 4 | 2 | 1 |
| — 60 ... 70 | 3 | 2 | 1 | 0 |
| — 70 ... 80 | 3 | 2 | 0 | 7 |
| — 80 ... 90 | 6 | 2 | 0 | 0 |
| — 90 ... 100 | 1 | 1 | 0 | 1 |
| Above 100 | 6 | 1 | 0 | 1 |
| Total | 20 | 16 | 3 | 10 |

It is stated by Dr. Mehliss to be a necessary condition for the appearance of the phenomena of rejuvenescence, that there exists complete energy and integrity of vegetative life at the period of decrepitude. It is also shown that the mode in which this energy shows itself depends upon local causes. The secretion of milk, the return of the menses, and the growth of the teeth, may in most cases be attributed to an irritation applied to the parts concerned. In some instances, these changes are accompanied by symptoms of constitutional irritation, and fatal results have followed in more than one case; a fact which connects the physiology of the subject with practical medicine.

Dr. Mehliss's book is, on the whole, curious and interesting; and contains some important physiological views: but it is disfigured with much that is fanciful, and fails not to exhibit abundant examples of the great defects of German literature—prosy tediousness of detail, endless repetitions, and vast wordiness.

## ART. V.

1. *The Cyclopædia of Practical Surgery.* Edited by WILLIAM B. COSTELLO, M.D. Part I.—*London, April,* 1837. 8vo.—*Article,* ABORTION, by Dr. RYAN.
2. *The Cyclopædia of Practical Medicine and Surgery.* Edited by ISAAC HAYS, M.D. Part I.—*Philadelphia, July,* 1833. 8vo.—*Art.* ABORTION, by Dr. DEWEES.
3. *A Dictionary of Practical Medicine, &c.* By JAMES COPLAND, M.D. &c. Part I.—*London,* 1832. 8vo.—*Art.* ABORTION, by Dr. COPLAND.
4. *The Cyclopædia of Practical Medicine.* Edited by JOHN FORBES, M.D. F.R.S., ALEXANDER TWEEDIE, M.D., and JOHN CONOLLY, M.D. Part I.—8vo. *London, January,* 1832.—*Art.* ABORTION, by Dr. LEE.
5. *Dictionaire de Médecine et de Chirurgie pratiques.* Par MM. ANDRAL, BEGIN, BLANDIN, BOUILLAUD, &c. &c.
*Dictionary of Practical Medicine and Surgery, &c.* Vol. III.—*Paris,* 1829. 8vo. *Art.* AVORTEMENT, by Prof. DUGÈS.
6. *Encyclopädisches Wörterbuch der medicinischen Wissenschaften. Herausgegeben, von den Professoren der medicinischen Facultät zu Berlin,* C. F. v. GRAEFE, C. W. HUFELAND, H. F. LINK, K. A. RUDOLPHI, E. v. SIEBOLD.
*Encyclopædic Dictionary of Medical Science, &c.* Vol. I.—*Berlin,* 1828. 8vo.—*Art.* ABORTUS, by Dr. VON SIEBOLD.

ON the appearance of Mr. Costello's Cyclopædia, we were led to compare some of its articles with those on the same subjects in other works of the kind; and it occurred to us that we might be able to lay before our readers some valuable practical articles founded on a critical survey of the whole of the publications on the same plan, recently published. The delay that has taken place in the publication of the Cyclopædia of Surgery has prevented us from carrying our plan into execution; but the present article affords a specimen of the information which it is capable of supplying. We have fixed on the subject of Abortion as the most important that occurs in the alphabetical arrangement followed in these works.

Although in the works before us the whole subject of premature expulsion is treated of under the general head of *Abortion*, yet, with the exception of Professor v. Siebold and Dr. Dewees, each writer has limited the strict meaning of this term to expulsions of the fœtus occurring before it has attained a sufficient degree of development to enable it to support an existence separate from the mother. Premature expulsion of the fœtus may therefore be considered under two distinct heads,—Abortion, and Premature Labour; it being only in the latter division that the child will be capable of maintaining an independent existence. The precise period at which the one division terminates and the other commences has been variously given. Thus Dr. Copland and Professor Dugès consider that the term premature labour is not applicable till the fœtus has passed the sixth month of utero-gestation. Dr. Dewees, who has not founded his division of the subject into abortion and premature delivery on the capability or incapability of the fœtus to support an independent existence, which we are rather surprised at, fixes the limit of abortion to expulsions of the ovum before the fifth month. The German authors have usually divided premature expulsion into three periods, "*abortus*," "*partus immaturus*," and "*partus prematurus:*" under the first division being classed all those expulsions which take place during the first sixteen weeks of pregnancy; under the second all those occurring from this period till the twenty-eighth week; after which time until about the thirty-seventh week they come under the last division, viz. premature labour. This arrangement, with a very trifling deviation, has been followed by Professor von Siebold.

The *causes* of abortion have been variously arranged by different authors; indeed to bring them under a simple, intelligible, and practical division is, we hold, by no means an easy task. We prefer the arrangement of the German and French authors, and are glad to find ourselves supported in this respect by Dr. Copland, who, in quoting from the article of Professor Dugès, above alluded to, says, "these may be divided into such as act primarily upon the mother or depend upon her, and into those which are connected with the product of conception, and are owing to diseases of the fœtus or its appendages." Each of these main divisions, as Professor v. Siebold observes, may be separately considered under the predisposing or the exciting causes. Neither Dr. Dewees nor Dr. Lee has given any distinct classification of the causes of premature expulsion, although in justice we must add that this subject has been ably and concisely handled by both these gentlemen. Dr. Ryan's method of arrangement is not very intelligible; he however divides the predisposing causes into those which "affect the mother or the fœtus respectively."

Among the *predisposing causes* of premature expulsion we are surprised to find that Dr. Lee commences with the remark, that "abortion is a frequent occurrence in the early months of pregnancy, particularly among women of the lower classes of society who are exposed to much bodily fatigue and mental anxiety," because a very different result has usually been observed by those in extensive practice among the better classes, viz. that abortion is of much more frequent occurrence among females of the upper ranks, on account of their more delicate frame and greater susceptibility of external impressions, owing to a luxurious life, little exertion, &c.

"Rigidity of the uterine fibres, and an unyielding state of its parietes," have been mentioned as a cause of abortion by all the authors except Drs. Lee and Dewees; the former of whom makes the following just observation: "Those who have insisted on rigidity of the uterine fibres as a cause of abortion have been led into error, by supposing that the uterus enlarges during pregnancy by the mere force of the mechanical distension of the ovum, and not by the gradual development of all the textures of the organ in exact correspondence with the growth of the organs of the fœtus." (p. 11.) The causes of premature expulsion are very fully and ably considered by Dr. Copland. His eighth paragraph on the predisposing causes, "chiefly referrible to the *constitution and habit of the mother*," is very complete and well worthy of attention; we would willingly have quoted the whole of it had our limits permitted. His next paragraph (misprinted 10th), on "*the causes which depend upon the fœtus*," is equally good.

Dr. Lee has insisted considerably on the fact of a diseased condition of the ovum being one of the most frequent causes of abortion. Taking the word abortion in its strict meaning, viz. expulsion during the first twelve or sixteen weeks of pregnancy, we perfectly agree with him, and have the powerful authority of the celebrated Baer, (*see our first Number, p.* 240,) in confirmation that by far the majority of abortions are connected with an anormal state of the ovum. Dr. Copland has very properly noticed the important practical fact that the various causes of abortion act with increased energy and effect at those periods at which in the unimpregnated state the catamenia would have appeared. The same observation is made by M. Dugès: we might also mention Dr. Ryan, but as by far the greater part of his observations on the causes of abortion, with very trifling alterations, are taken from Dr. Copland, with the exception of three sentences from Dr. Lee and one from Dr. Dewees, it would be mere waste of time to notice them as original. Dr. Ryan in some places forgets what he has copied, and contradicts himself not a little now and then. Thus, at the commencement of page 15, he says, "women in the lower class of life, who are exposed to much bodily fatigue and mental emotions, are also very liable to this disease;" while at the latter part of the same page he observes, "it is to be recollected, that women in the lower ranks of life, who make great exertion and who are frequently exposed to falls and blows, very seldom miscarry."

In speaking of the connexion between the uterus and the placenta, we regret that Dr. Lee should have stated that "the placenta adheres to the uterus by means of the deciduous membrane alone, which is directly applied to the openings of the uterine sinuses." Although he was perhaps justified in making this observation at the time that the first Number of the Cyclopædia of Practical Medicine was published, from the slight degree of doubt which he had for a short time thrown upon the accuracy of the admirable researches on these subjects by the Hunters, yet, as the observations of these eminent anatomists have been most clearly and satisfactorily proved to be correct, the above-quoted sentence appears rather as a blot of inaccuracy upon an otherwise excellent and practical article. The author of the article in the Cyclopædia of Practical Surgery has adopted these views which we believe Dr. Lee himself now considers as incorrect; and in a somewhat diluted form has given the observations which conclude the paragraph above alluded to.

In examining the symptoms of abortion, as described in the different essays under examination, we regret that so little attention has been paid to a proper classification of them. It is true that Professors Siebold and Dugès, and Dr. Copland, to a certain extent, have virtually done so, but still not so as to excite the notice of the ordinary reader. The symptoms will, in chief measure, depend on the cause of the attack; hence the symptoms may be those produced by the previous death of the child, those arising from local injury or irritation, and those which depend on certain conditions of the whole system, more especially as regards the circulation. Without some attempt at arrangement we have a confused medley of symptoms, which, however correct, are nevertheless not so easily retained in the memory, and are sometimes not altogether intelligible to a beginner.

"The decided approach of abortion is indicated by two chief symptoms, viz. by the discharge of blood from the vagina, and uterine contractions." (*Dugès*, p. 669.) The value of these symptoms has been very variously stated by different authors, among whom none have given more plain and practical observations than Dr. Dewees: we regret not to have found something of the same sort in his present article. Professor Dugès considers that hemorrhage most frequently *precedes* the appearance of the pains; and, as far as respects distinct uterine contractions, we perfectly agree with him; but, on the other hand, a patient may suffer pain in the back and loins for some time without the appearance of hemorrhage and without abortion being a necessary result. Pains coming on at regular intervals like labour pains, being the result of uterine contractions, show that the uterus has already begun to act towards the ovum as a foreign body, and where this is the case expulsion is inevitable. On these grounds Dr. Burns has founded the following rule. "When uterine pain precedes or accompanies the discharge, expulsion cannot be prevented; but where the discharge precedes the pain, if the child be alive, it frequently may." Still, however, this will necessarily be modified by the part where the separation has taken place. Thus if it has occurred in the immediate vicinity of the os uteri, the blood readily finds an exit; and thus we shall have a discharge of blood with little or no uterine contraction, and without much danger of expulsion. Where, however, the separation has taken place in or about the fundus, there will be pain, and at first without hemorrhage; by degrees, as the blood finds its way towards the os uteri, more and more of the ovum becomes detached, so that at length the greater portion is separated from the uterus; it becomes as a foreign body, and expulsion soon follows, with more or less hemorrhage. These points have been most ably treated by Dr. Dewees in his compendious system of midwifery, and are well worthy of attention. The temporary increase of congestion in the uterine circulation, which occurs at what in the unimpregnated state would have been a menstrual period, may directly excite expulsion by extravasation of blood between the uterus and ovum, or indirectly cause it by destroying the life of the embryo.

Professor von Siebold remarks that, "where abortion happens during the first weeks of pregnancy, the symptoms produced bear a strong resemblance to those of menorrhagia. These attacks usually occur at the periods when the catamenia were wont to appear: this discharge is very profuse, accompanied with severe pain and mixed with coagula and portions of membrane like decidua. After their expulsion the symptoms of pregnancy cease, and the patient usually recovers very quickly." This fact has been noticed by none of the other authors whose essays we are reviewing, except Dr. Copland; Dr. Burns mentions it, but adds a theoretical explanation, viz. that the ovum has not yet entered the uterus, which we deem wholly unnecessary, as the presence of a very small ovum in the discharge can scarcely be detected. The fact has also been noticed by Dr. Locock in his admirable paper on Dysmenorrhœa in the Cyclopædia of Practical Medicine, where abortion at a very early period has been mistaken for unusually severe attacks of dysmenorrhœa. "The degree of hemorrhage is not always in the ratio of the advancement of gestation; though, generally speaking, under similar circumstances, it may be laid down to be the case; for the loss of blood, for the most part, is governed by the period of conception, the force of the remote cause, the degree or extent of detachment, the vigour of the general circulation, the period that elapses before uterine contractions can be successful in the expulsion of the ovum, and the state of the ovum, that is, whether it remains entire or has been forced open so as to expend the liquor amnii." (*Dewees*, p. 90.)

In considering the *treatment* of abortion we decidedly prefer the arrangement made use of by Dr. Copland: he divides it into the preservative, the palliative, and the remedial. To consider this subject fully would require more space than we can afford; we cannot, however, but express our surprise that the observations on this subject by Mr. Charles White, of Manchester, have been so entirely unnoticed by our authors. By no one have the mischievous effects of indiscriminate bloodletting in cases threatening abortion been more clearly pointed out than by this highly practical author. "From the success (says Mr. W.) I have seen attend this practice, (viz. cold bathing,) in preventing miscarriages, and many of the disorders peculiar to the pregnant state, particularly nausea and vomiting, I am satisfied they are much more seldom to be attributed to a plethora than to weak, lax fibres." Although he has not noticed the observations of Mr. White, Dr. Copland has not been unmindful of the facts, and observes that "in every instance the preservative treatment must be based upon our views respecting the pathological state of the uterus, and of the whole frame at the time of prescribing it." His observations at § 32, 33, and 34, are very excellent. Dr. Lee's remarks are equally correct: "where there are no symptoms of local or general plethora and excitement, bloodletting is contra-indicated." Dr. Dewees seems altogether to have overlooked the cases of abortion depending upon a "weak, lax fibre," as described by Mr. White. We cannot agree with him in so general a recommendation of bleeding and strict antiphlogistic regimen, and must in defence of himself quote an excellent observation from his work on Children. "We are warranted by long experience to declare that, unless plethora produce some direct evidence of its mischievous tendencies, as headach, pain in the chest, a sense of fulness in the head upon stooping, giddiness, &c. the patient should not have recourse to bleeding without the express approbation of the physician. To women who are in the habit of miscarrying, this proscription of indiscriminate bleeding is particularly important, especially as it is the remedy almost universally resorted to for its relief, than which in many instances nothing can be more preposterous or improper. We are justified in saying it has very often produced the evil it was intended to prevent." (*Dewees on Children*, § 36 and 37.) Neither can we agree with him in the present article where he says, "the loss of a few ounces of blood is absolutely necessary where there is a painful aching in the back, a heaviness about the loins, a sense of weight and bearing down, even without pain," &c. because cases are not unfrequently met with, where, from great debility and want of tone, these very symptoms are best relieved by bitters, mineral acids, chalybeates, and the cautious exhibition of wine.

Dr. Copland has merely enumerated the various means recommended by different authors for plugging the vagina, without informing us what peculiar form of tampon he himself recommends. We have always preferred the simple sponge of Dr. Dewees as being the nearest imitation of the plug which nature makes use of, viz. a coagulum; it produces less irritation and is introduced and removed with much more facility than the other species of plug. In speaking of the means for exciting the uterus to full contraction Dr. Copland says, " When the embryo only is expelled, the appendages being still retained, or when the hemorrhage is great, the entire ovum still remaining in the uterus, the ergot of rye will often prove of inestimable service, and when given in the form of decoction with as much borax as it will dissolve will seldom disappoint our expectations." (§ 37.) This mention of borax deserves notice. Borax was a remedy many years ago in considerable repute for its efficacy in exciting uterine contractions, and certainly it does appear to possess this power to a considerable degree; the combination of it with ergot will probably render the latter a more certain remedy. In assisting the removal of the ovum from the uterus Dr. Dewees has given a description and drawing of the wire crotchet recommended in his compendious system. The author in the Cyclopædia of Practical Surgery has taken a faithful copy of this instrument, as also (with slight modifications) of the prophylactic treatment recommended by Dr. Dewees.

The *Pince à faux Germe* has been strongly deprecated by Professor Dugès; but he has not mentioned an excellent point of practice recommended by this celebrated accoucheur, viz. of throwing up a powerful stream of tepid water into the vagina to dislodge and bring away the ovum.

Several other points of interest present themselves in a comparative view of these different memoirs, but our limits forbid extending this notice any further. We consider the articles by Drs. Copland and Lee as by far the best. The first is a most valuable digest of the best information upon the subject, and contains almost every thing which is necessary to be known upon it, arranged with much simplicity of method and given in a clear perspicuous style. The article of M. Dugès is also of considerable merit, but it is very far from being complete; nor are the rules for treatment laid down in that clear and simple manner which we so admire in the above-mentioned essays. From Dr. Dewees we confess to have expected a first-rate article, because the excellent observations which have already emanated from his pen warranted such a hope, and we can only regret that he has not devoted more space to the consideration of so important a subject. Dr. Ryan's article is below the present level of our science and our literature.

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## Art. VI.

*Outlines of the principal Diseases of Females; chiefly for the Use of Students.* By Fleetwood Churchill, m.d., &c.; Physician to the Western Lying-in Hospital, (Dublin;) Lecturer on Midwifery, &c. in the Richmond-Hospital School of Medicine, &c.—*Dublin*, 1838. 8vo. pp. 402.

A work like the present has long been a desideratum in our medical literature, both for practitioners and for the student. The information which we possess on the diseases of women, is so scattered among a variety of monographs and journals in different languages; and much of it, however valuable, is so crude that it is scarcely available, even to the former class of readers, without much actual experience as well as study, and to the latter class it must, in many instances, be almost necessarily useless. The author informs us, in his preface, that "it has been arranged in the present volume that the *text* should contain an ample outline of the history, pathology, symptoms, and treatment of the diseases, without any detail of controversies or conflicting opinions, which are given in full in the *notes* appended to each page; so that the junior student, by confining his attention to the text, may acquire elementary information, which may be subsequently extended by consulting the notes and references." This is an excellent plan to go upon; because it not only affords a succinct view of the most approved opinions respecting the nature of these diseases, but makes the reader also, to a certain extent, familiar with the literature; a point which we hold to be of the highest importance, and which we think has not been sufficiently attended to in the education of the English medical students of the present day.

Dr. Churchill divides his work into two parts: 1st, the diseases of the external organs of generation; and, 2dly, the diseases of the internal organs. We cannot exactly agree with him as to the correctness of placing the diseases of the vagina under the second division, because, according to the most approved arrangement of these subjects, they belong to the first class: it is, however, a matter of no great importance, and we are far from feeling disposed to cavil about it. The symptoms, diagnosis, and treatment are distinctly arranged under separate heads; by which means reference to any peculiar point is greatly facilitated, and the immense mass of matter from which the author has had to selec his observations, better digested and condensed.

In describing "*Itching of the Vulva,*" (chapter v.) the author mentions that it occasionally takes place "during pregnancy, from increase of fluids" in "the genital system;" but he makes no mention of that form so admirably described by Dr. Dewees, where the disease depends upon an aphthous state of the mucous membrane of the canal: perhaps, he intends to bring it under the head of Diseases of Gestation, which, he informs us, will be considered in a separate volume.

In his 6th chapter, on "*Inflammation of the Mucous Membrane of the Vulva,*" he discusses the subject of infantile leucorrhœa. Having observed that it is occasionally met with as an epidemic catarrh, and described the celebrated case mentioned by Dr. Percival, of Manchester, where it was suspected to have been produced by criminal intercourse, he justly remarks that "the presence of this discharge is no proof whatever of such an offence." Circumstances, however, sometimes occur to render the diagnosis of this point extremely perplexing. We recollect a case of this sort where two sisters, the one six, the other four years old, were affected with this discharge, and where the extreme youth of the culprit would have led to the same conclusion, had not the discovery of well-marked phymosis placed the matter beyond doubt. Precisely similar circumstances, we know, occurred in the practice of one of our friends. The affection is well described by Dr. Churchill.

> "The commencement of the disease is marked by local uneasiness, itching, and scalding on making water; the mucous membrane of the vulva is found inflamed and puffy, but for some time there is no discharge. The uneasiness felt by the child induces an attempt to relieve it by rubbing the parts; which, of course, aggravates the

suffering and increases the inflammation. At a more advanced stage, there is observed a colourless, thin mucous discharge; speedily becoming more copious, thicker, and of a white or yellow colour. It is very often of an acrid character, and gives rise to a ring of inflammation, and sometimes of excoriation of the skin at the margin of the vulva. If the labia be separated, the mucous membrane will be found more vascular and of a deeper colour than usual; but in a very few cases does the inflammation extend up the vagina. The distress is increased with the progress of the disease; the smarting and scalding are very severe, and the little patient cannot walk without pain." (p. 11.)

In describing the symptoms of *acute Vaginitis* of adults, (p. 22,) which are given correctly, we think the author might have added pain in the back and loins, and throbbing in the pelvic region: they are, it is true, not constant, but nevertheless usually observed where the attack is severe; nor does he point out the analogy in the changes which the discharge undergoes, as the disease advances, to those which are observed in inflammation of the urethra, schneiderian membrane, &c. The gradual return to a more purulent, and at last merely mucous discharge, is not mentioned.

Although Dr. Churchill has described chronic leucorrhœa as resulting from chronic inflammation of the vagina, no mention is made of the simple leucorrhœa, so well described by Sir C. M. Clarke, resulting from debility and want of due tone and contraction in this canal. He briefly alludes to it in a note, in order to express his dissent from the correctness of these views, and doubts "whether general weakness and relaxation is any proof of debility of the vaginal mucous membrane." We cannot imagine any views more thoroughly confirmed by daily experience than those of the distinguished author to which we have just alluded. In large cities, the deranged and atonic state of the system, more especially of the chylopoietic viscera, is decidedly one of the most frequent causes of the above-mentioned form of leucorrhœa; which, in its turn, by increasing the general debility, aggravates the atony of these organs, the deranged state of which had primarily induced the discharge. From inattention to these circumstances can we alone explain the following observations, with the correctness of which we cannot possibly agree. "I have less experience," says Dr. C., "in general remedies, in consequence of the almost invariable success attending local treatment, consisting chiefly of different astringent solutions, thrown up the vagina by means of a syringe, or clyster-pipe and bladder." (p. 27.) We are surprised that no allusion has been here made to Dr. Locock's excellent article on *Leucorrhœa*, in the Cyclopædia of Practical Medicine, in which very different opinions are stated. Dr. Locock's observations on the treatment of the simple forms of leucorrhœa are well worthy of attention; and his directions for restoring the general tone of health are most judicious: and he concludes by saying that "local remedies are rarely required in these milder cases." We fully agree with these opinions; and can state, from pretty extensive experience, that, in most cases of simple leucorrhœa, the discharge will diminish in proportion as the tone and strength of the system is improved. We must equally dissent from the remark with which the author concludes this chapter: "The *consequence* of a long persistence of leucorrhœa is a relaxation of the parieties of the vagina, favoring the production of prolapsus uteri, and which can only be remedied by a diligent use of astringent injections." (p. 29.) Relaxation of the parietes of the vagina is seldom a *consequence* of leucorrhœa, but, in by far the majority of cases, leucorrhœa arises from relaxation of the vagina, the result of impaired health.

The next chapter is on "*Inflammation of the Glandular Structure of the Mucous Membrane covering the Cervix Uteri.*" After having quoted the symptoms of this affection, as given by Sir Charles Clarke, Dr. C. observes, "If, as Sir C. Clarke supposes, this state of the cervix always accompanied the white discharge, the disease would never be mistaken; but many cases occur in which the white discharge, exactly as described in the quotation above, is present without any puffiness or tenderness of the neck of the uterus." (p. 30.) He ought to have quoted the remark which this distinguished author has made a little further on, and to which Sir C. Clarke has expressly directed the reader's attention,—viz. that "even the *transparent* mucus of the vagina, when secreted in sufficient quantity to run down over the labia, (which have some motion over each other in the act of walking,) becomes also opaque and white. This change is attributable to the entanglement of air with mucus." Nor has Dr. Churchill paid any attention to a well-known variety in the appearance of the white mucous discharge, which we hold to indicate equally an inflamed state of the cervix: we mean where it is quite transparent, having the consistence of thin albumen; a fact observed by Dr. Locock, and we should have supposed familiar to every practitioner of any experience in these diseases. Sir C. Clarke makes a similar remark, viz. that "in many instances the white mucous discharge is much thicker than cream, having the tenacity of glue; and perhaps this is the state in which it comes away from the cervix uteri." Our own observations fully confirm this conclusion. The author states that "there are seldom any constitutional symptoms present," and in this remark he is to a certain extent supported by Sir C. Clarke: from very numerous cases, however, which have come under our notice, we cannot agree with this view; having found that the disease is almost invariably accompanied with much gastric derangement and general debility. The local symptoms are almost entirely omitted by Dr. Churchill. Dr. Locock correctly states that "there is much pain of the back, extending round the hips and down the thighs, and, though relieved, not removed, by the recumbent posture;" and Sir C. Clarke says, that it "is increased by whatever tends to call the neighbouring parts into action, such as riding, or by whatever produces pressure on the part affected. In this way the passage of a hard and large portion of fæces causes much distress; for not only are the blood-vessels filled in the act of expulsion, but, during the evacuation, constant, and sometimes considerable, pressure is made upon the cervix." Hence we find that patients of this class cannot sit down suddenly upon a hard seat or ride in a rough vehicle without considerable suffering.

In his treatment of *inflammation of the cervix uteri*, Dr. Churchill recommends abstracting blood, "either by venesection, leeches, or cupping to the loins;" and adds, in a note, "in almost all affections of the uterine system, I have found cupping the loins by far the most efficacious mode of taking away blood." (p. 30.) We could scarcely have supposed that an author who professes to write a book on the diseases of females, expressly compiled from the most approved authorities of the day, should have shown himself ignorant of one of the greatest improvements which have been made in the treatment of these affections: we allude to the practice of applying leeches, by means of a proper tube, to the os and cervix uteri, as recommended by Dr. Locock, and described by him in the article on Leucorrhœa already alluded to. If there be one uterine affection which requires this mode of locally abstracting blood more than another, it is this: we have for some years past used it in a very large number of cases, with the greatest relief. We do not agree with the author in regarding castor-oil as the best purgative in these cases, except, as Sir Charles Clarke observes, where the habit is very languid. The small doses of sulphate of magnesia which he recommends are infinitely better, as tending to allay irritation and assist in relieving the congested state of the neighbouring vessels. Neither do we consider a full dose of laudanum, with plenty of mucilaginous fluids for drink, the best means of relieving the frequent desire to evacuate the bladder: in many cases it is produced by acidity of the urine, the result of the gastric derangement; and in others, where it is produced more directly by the state of the cervix, it will subside as soon as we remove this latter condition. We regret that the author has paid so little attention to the consideration of this disease, which we hold to be one of the greatest importance, not merely from its frequency, but from the mischievous effects to which it too commonly leads. The whole chapter occupies only two pages.

Dr. Churchill divides the subject of *Prolapsus Vaginæ* (chapter v.) into two species; that of the anterior and posterior wall. In describing "*Prolapse of the anterior Wall of the Vagina,*" he informs us "that the mechanism by which this is produced is sufficiently intelligible:" we could have wished that he had made it so. "The vagina (or, according to Siebold, the inner membrane only,) becomes relaxed from some cause, such as repeated child-bearing, and, the urine having been allowed to accumulate, it distends and forces the bladder downwards, protruding before it the yielding vagina. Every time that this accumulation takes place, the bladder is distended to a greater degree; so that that which at first occasioned no inconvenience gradually increases, until complete prolapse, or protrusion through the external orifice, is the result." (p. 34-5.) This is nothing more than a description of prolapsus vesicæ. Was ever a fluctuating tumour, protruding from the external parts, and formed by the distended bladder, called by any other name than prolapsus or procidentia vesicæ? It is, of course, covered by the vagina, as is the tumour in prolapsus uteri—but what of that?

In his preliminary observations on the *Pathology of the Uterus*, Dr. C. gives a very excellent remark respecting the condition of the vessels and nerves of the uterus after delivery: "the vessels, which were so much elongated, become tortuous, their coats are found thicker, and their caliber greater than natural; the nerves, also, though not so large as during pregnancy, remain of a considerable size and tortuous. The substance of the uterus does not return to the same density as before gestation, unless the interval be very long." (p. 53.) The author observes, in a note, "It is a remark, I believe, of the late Dr. Parry, of

Bath, that the tortuosity of the vessels is not a provision for some function they have yet to fulfil, but the result of some previous condition or some function already performed."

The observations on the utility of the *Speculum* are very just; the real value of vaginal examination is duly appreciated, and the limits pointed out within which this instrument can be used.

> "We have seen that by the touch, in connexion with the local symptoms, we can obtain information on all points, except that of colour; and the accuracy of the knowledge so acquired is scarcely, if at all, inferior to that obtained by sight. It is very true that a delicate sense of touch and much experience is necessary before this degree of perfection will be attained, but it is equally certain that perseverance in availing ourselves of every opportunity (both on the living and dead body) will ultimately be crowned with success. The only deficiency in our means of diagnosis has been supplied of late years by the introduction of the *speculum;* and to this we undoubtedly owe the extension of our knowledge of uterine and vaginal diseases. Some new ones have been observed, and others, already familiar, have been more accurately described. There are, however, very considerable difficulties in the way of its use becoming common: it requires greater exposure, and is more revolting to feminine delicacy, than the other mode of examination; in some cases, also, it is much more painful. The information obtained by it is also much more limited, being confined to the state of the vagina and cervix uteri—still it is very frequently a most valuable adjunct." (p. 57-8.)

In describing that species of *amenorrhœa* which is produced by an imperforate condition of the os uteri and vagina, the author has entirely omitted to notice the admirable chapter on this subject in Richter's *Anfangsgründe der Wundarzneykunst,* which we hold to be by far the best account of any that has been given: indeed, we can scarcely imagine a more perfect specimen of well-arranged description, in the simplest language, than is exhibited in this valuable essay; and the author would not only have there found ample directions for the diagnosis and treatment of these cases, which occur under a great variety of forms, but might have quoted with advantage the fund of literature which this great author has added. The case by M. Amussat, which is given in a note, is highly interesting; but we cannot accept it as a substitute for what we have just referred to.

In the perusal of this volume, we have more than once had occasion to remark that the manner in which the different observations are put together is not calculated to render them always intelligible to a student, or give him clear notions upon the different subjects which are discussed. Compilation in many cases is allowed to be too evident; quotations are thrown together without sufficient care in arranging and blending them so as to appear as a whole. These remarks are suggested by the following observation respecting *dysmenorrhœa:* "Dewees," says Dr. C., "has tried the tinct. cantharid. with success, but the medicine upon which he appears to rely most confidently is the tinct. guaiaci, in doses of f℥ss three times a day. The pain is sometimes increased the first period after its exhibition, he says, but ultimately it affords complete relief." (p. 91.) Now, with regard to the tincture of guaiacum as a remedy in dysmenorrhœa, rather more than the simple fact is required to render the observation of any value. In the first place, it is *not* the plain tincture of guaiacum, but an ammoniated tincture, prepared after a peculiar formula; and, if the author had consulted Dr. Locock's article on this disease, he would have found that it was chiefly indicated in those cases where a rheumatic diathesis was present; a fact upon which the real value of this remedy in great measure depends, and which we have had opportunities of confirming.

We cannot recommend the arrangement which Dr. C. has adopted in discussing *menorrhagia:* it is not practical, and does not point out the two main conditions of the system under which this disease occurs, viz. the active and passive state: indeed, we must confess that we can see no real or important distinction between the three species which he has pointed out; and, even by his own account, the treatment in the first two species will be quite similar. The third form appears to be a state of local congestion, which he thinks that he has never observed in women under forty, or after the cessation of the menses. The usual signs of uterine congestion are correctly enumerated, and the ergot of rye recommended with much confidence; but we must beg to doubt the correctness of the opinion that the discharge in this form is produced by "the rupture of some of the vascular twigs which ramify on the lining membrane of the uterus." (p. 105.) The anæmic symptoms and effects of passive menorrhagia are indeed noticed, but not in the manner or to the extent which we could have wished. The author has alluded, in some comparatively unimportant points, to Dr. Locock's excellent article on this subject in the Cyclopædia of Practical Medicine, but the really valuable matter has been unnoticed.

The next chapter, (vii.) "*On the constitutional effects of the Disorders of Menstruation,*" seems to be intended as a sort of appendix to fill up the numerous gaps and defective points in those which precede it. When an author describes the symptoms, causes, consequences, diagnosis, and treatment (see Menorrhagia,) of a disease, we are surely justified in expecting that he has furnished a complete discussion of the subject. We do not deny that the chapter in question is well handled, and much valuable literature collected; but how can a student be expected to follow such an arrangement? A painter might almost as well attempt to pourtray a landscape by putting the foliage in one picture, and the ground, &c. in another; by such an arrangement the connexion between the disease and its effects upon the constitution is rendered obscure, and it becomes almost impossible for a beginner to form any clear views and general principles, or to make any useful or practical deductions.

Passing over several chapters we come to the 12th, "*On Moles.*" A considerable quantity of literature is collected upon this subject, but much has been omitted which ought to have been given: we allude more especially to the admirable essay on Diseases of the Placenta by Dr. J. J. Simpson, in the Edinburgh Medical and Surgical Journal. In speaking of the different species of mole, the author observes, "there is a variety of the fleshy mole which is worthy of distinct notice: it is figured in Denman's plates, in Granville's Illustrations of Abortion; and there is a specimen in the museum of the college of surgeons in this city, and another in Dr. Montgomery's museum. The texture of the ovum is much more dense than natural, especially the placental portion, which has very much lost its spongy feel; the membranes are unaltered, and, when opened, the inner surface of the placental portion consists of tuberculated projections of different sizes, from a pea to a walnut. Into one of these tubercles the cord is inserted, and the fœtus, in consequence, has perished." (p. 155.) This is not an uncommon species of mole, according to our experience. Although the author enumerates every variety of vesicular mole, yet we do not find it described as attaining the greatest bulk of these morbid growths; neither does he state distinctly that the hydatic mole increases in bulk with greater rapidity than the healthy ovum. The case of uterine hydatids, which is so graphically told by Dr. Gooch, in his work on some of the more important Diseases of Females, ought also to have been quoted.

Dr. Churchill's diagnosis of fibrous tumours of the uterus is very meager and imperfect: why not have quoted largely from Sir Charles Clarke on this subject, whose observations are so truly valuable and practical? Why omit the name of Mr. Ingleby, of Birmingham, whose excellent work on Obstetric Medicine teems with valuable cases, and not less valuable observations?

In chapter x., on "*Polypus of the Uterus,*" although the author appears to have consulted the admirable works of Levret, Herbineaux, and Richter on this subject, he merely mentions the names of the first two authors, with some others, in giving a table of the comparative frequency of polypus at different ages. Dr. Hamilton's opinion that the hemorrhage which attends polypus is from the internal surface of the uterus, and not from the tumour itself, is very properly stated. We consider that this view holds good in some cases only; but, where the polypus has attained a considerable size, we must agree with Dr. Gooch in attributing the hemorrhage to the rupture of those engorged vessels which are observed upon the external surface. As soon as a polypus passes the os uteri, it is well known that it swells rapidly; and this, in all probability, must be chiefly attributed to the obstruction in its circulation by the pressure of the os uteri which encircles its neck. The chief vascularity of a fibrous polypus is, we presume, in the mucous membrane which covers it; and, in all probability, this is the seat of those ulcerations which we occasionally observe in the lower portion of large polypi, precisely as is seen in aggravated cases of prolapsus uteri. That these tumours, therefore, "are seldom or never attacked with inflammation or ulceration," (p. 188,) we must venture to doubt. We regret to say that the diagnosis of polypus uteri is meager and faulty in the extreme. Thus, for instance, the author states that it is distinguished "from *scirrhous enlargement* by the absence of pain and ulceration, and by the existence of a pedicle." (p. 193.) We would ask, are pain and ulceration always absent in polypus uteri? is the portion by which it is attached to the uterus always sufficiently within reach of the finger, or deserving the name of pedicle, to admit of being taken as the grounds of our diagnosis? Again, he observes that it is distinguished "from *prolapsus uteri* by there being no aperture (os uteri) or canal at the lower part of the tumour, by the detection of the os uteri in the pelvic cavity, and by the insensibility (generally) of the polypus." (p. 193.) Is no aperture ever found at the lower portion of a polypus? We admit that a practised finger will distinguish it from the os uteri in prolapsus, or we may ascertain it by passing a catheter into the opening; but why is this not mentioned? Is the condition of the vagina unworthy of notice, as a means of diagnosis between this displacement and polypus? Is the capability in most instances of returning prolapsed uterus, and with great relief to the patient's sufferings,—and the impossibility of doing so in polypus, producing great pain on every attempt to raise it, a point of so little importance as to be undeserving of attention? In describing the various instruments for applying the ligature upon a polypus, Dr. Churchill has totally omitted the one which has been invented by Baron v. Graefe, of Berlin, and which is decidedly the most perfect thing of the sort yet imagined: it is an improvement upon the double canula, and must be looked upon as one of the neatest of the modern surgical instruments. The omission of this is the more remarkable, as it has been for some time in the hands of many of the practitioners and instrument-makers of the metropolis.

We gladly pass on to a part of the work which we have read with much pleasure and satisfaction. The author's description of the appearance of the uterus in a case of corroding ulcer is excellent; indeed, we would almost say that, as far as our own reading goes, it is the best which we possess upon the subject.

> "A *post-mortem* examination reveals clearly the nature and extent of the disease. The uterus is found more or less destroyed by ulceration, which sometimes extends itself circularly, so as to destroy the cervix and part of the body completely, leaving the remainder suspended by the ligaments, and unconnected with the vagina except by the surrounding cellular tissue; in other cases, it attacks the anterior or posterior wall of the uterus only, with the neighbouring portion of the vagina and the bladder or rectum. If the bladder be perforated, the vagina will be found more or less coated with matter deposited from the urine; if the communication be with the rectum, fæcal matter will be found in the vagina. I have never seen a case in which the bladder and rectum were both perforated. It is important to remark that there is no deposition of new morbid matter, either in the uterus itself or in the neighbouring parts. The portion of the uterus which remains undestroyed is slightly swollen and vascular." (p. 213-14.)

We cannot entirely agree with the author in his diagnosis of this disease from cancer, and we are inclined to side with Sir C. Clarke as to the essential difference of the pain in these two diseases: in other respects Dr. Churchill's diagnosis is well worthy of attention.

> "The true ground of diagnosis, and the marked distinction between these two formidable complaints, is discovered by a *vaginal* examination. In cancer uteri, there is an extensive deposition into the cellular membrane and glands between the vagina and rectum, and between the vagina and the bladder, as well as into the substance of the uterus itself, connecting them so as to form one large mass, and *rendering the whole immoveable;* the finger, on being introduced into the vagina, finds *very little space*, and no power of *moving the parts with which it comes in contact.* Whereas, in corroding ulcer, no deposition having taken place, *the uterus can be moved by gentle pressure*, and part of the pelvic contents having been destroyed by ulceration, *there is more space than usual in the cavity.*" (p. 215.)

In discussing the questionable measure of excising the cervix uteri where the disease has not attacked the body of it, Dr. C. observes, "Caustic injections may be employed, or the ulcer touched with solid caustic, by means of the speculum. As yet, I have had no opportunity of trying this mode of treatment in cases sufficiently recent to afford reasonable expectation of benefit. I have used vaginal injections of nitrate of silver in advanced cases, with temporary relief: it assuaged the pain, and deprived the discharge of its fetid odour. Ten, twenty, or thirty grains, may be injected twice a day, dissolved in two or three ounces of water." (p. 217.) In this disease we can with confidence recommend to the author the application of leeches directly to the part by Dr. Locock's tube, as a valuable adjunct to other treatment. The whole chapter on this subject is well arranged, and shows much judicious reading and considerable experience.

Dr. Churchill gives a very fair summary of the literature and different opinions respecting *cancer uteri.* He inclines to the views of Dr. Copland and others, who consider that the fibrous portion of the cancerous mass which forms the septa results chiefly from an altered state of nutrition in the seat of the disease; whereas the soft semigelatinous portion which fills the cells formed by these septa, proceeds from a morbid secretion. We regret that the author has not noticed the fact that the majority of cases of cancer uteri are preceded by symptoms of inflamed cervix. Our own experience has taught us to believe that there are few cases which do not commence thus. He owns that, "in the latter part of the first stage, pressure on the cervix is occasionally painful," (p. 231;) but we should be much more inclined to say that, at the *beginning* of the first stage, it is almost invariably so.

The connexion between inflammation of the cervix and the early stages of scirrhus has excited much less attention that it ought to have done; which is the more to be wondered at as the former disease rarely exists to any extent without being attended by lancinating pains; the knowledge of which fact is of great importance in the treatment. We cannot agree with the author in saying that, "up to the actual commencement of ulceration, the discharge does not vary from the usual vaginal secretion, it is merely augmented in quantity." (p. 234.) If we carefully investigate the early history of these cases, we shall find that, in most instances, the discharge has exhibited either the creamy or albuminous characters which are peculiar to inflammation of the cervix. We are aware that, in this disease, if the inflammatory action be running to any extent, the discharge will become more or less watery; and this, we think, is most likely to take place when the disease is passing into the state of scirrhous induration.

The author's description of the effects produced upon the constitution by the progress of cancer uteri is very good; we think that, if he had quoted Sir C. Clarke's observations on this subject, he would have added to the value of the chapter. The diagnosis of the disease, however, is, in our opinion, given very unsatisfactorily. In regard to the treatment of cancer, we agree with the author in stating that iodine, as well as the preparations of iron, deserve a more extensive trial than they hitherto have had. To the value of chalybeate medicines we can decidedly bear a favorable testimony. The vaginal injections containing lead, which have been recommended by Dr. Leake, are very valuable when there is any inflammatory action present, especially if combined with some demulcent narcotic; after which, a salt of iron may be substituted for the lead with advantage. We regret that no mention whatever is made of leeches in the treatment of the first stage (*scirrhus*); their great importance we have already pointed out. In the second stage (*ulceration*), it is true, the author proposes that "a very few leeches may be applied, if necessary:" this, however, does not seem intended for the cervix uteri, but for the sacrum, which would be but of little use. We may add that, even in far-gone cases of cancerous ulceration of the uterus, we have found the application of leeches to the part invariably attended with relief, the pain being diminished, and the discharge assuming a more healthy appearance. Dr. C. informs us that he has "seen but little benefit from injections," (p. 246;) and yet his own statements lead to a different conclusion.

> "Some time ago," he says, "I ordered injections of nitrate of silver (gr. x. to ℥i. of water twice a day) in a case of cancer, in hopes that it might arrest the ulceration; in this it failed, but I found that it afforded great relief in two particulars: first, it destroyed the excessive irritability of the ulcer, and diminished the pain; and, secondly, it entirely took away the fetid smell of the discharge: this latter effect was pointed out by the patient herself. I have tried it several times since, and always with the same good effect; I therefore feel justified in recommending it to the profession in this disease. The sympathetic and even distant pains which I have noticed (p. 233) are often and most effectually relieved by injections thrown up to the uterus; and, in the case of sciatica which has been mentioned, the injection of nitrate of silver was scarcely given before some mitigation was perceived, and after two or three more it ceased altogether for some time." (p. 246.)

The author has given an excellent digest of what has been published respecting excision of the neck of the uterus, and adds, when speaking of M. Lisfranc's operations, "it has been shown, however, by M. Pauly, that his operations (Lisfranc's) were fewer in number than was asserted; and that, so far from being either safe or successful, several died within twenty-four hours after the operation, and a considerable proportion (more than two-thirds,) were ultimately lost." (p. 248-9.) We think that M. Lisfranc owes to the profession a refutation of M. Pauly's most grave charges, if he deems the vindication of his honour a matter of any consequence.

The description of "*Retroversion of the Uterus*" is very confused, and the mechanism of it very imperfectly detailed. The names of Burns and Dewees do not once occur throughout this chapter.

In the chapter on "*Prolapsus Uteri,*" the author quotes authorities in a way which must inevitably mislead the student. Was Capuron the first to show that it was produced by frequent parturition? Did Clarke first point out that uterine hemorrhage, or Jourdan that menorrhagia, might be causes of it, &c.? Where is the great name of Richter? why are his admirable observations unnoticed? The alteration which takes place in the character of the symptoms when the disease passes from the *partial* to the *complete* form, is not pointed out. The peculiar direction which the stream of urine takes, and many other circumstances are omitted; and we may say that, taken altogether, his diagnosis is meager and not altogether correct. The treatment of partial prolapsus, or procidentia, is very full. In cases of complete prolapsus, where, from swelling of the organ, it cannot be returned by venesection, purgatives, &c., the author proceeds at once to recommend "incisions into the substance of the womb." Here again we must complain of his omitting the application of leeches to the tumour. It is a most valuable remedy and one which, from repeated experience, we hold to be certain of reducing the size and hardness of the uterus, and rendering it capable of being returned; the objections of Sir Charles Clarke to the introduction of a sponge (p. 298, *note*,) ought to have been properly modified and explained away by showing that, if the sponge be wound round loosely with thread or fine string, and then soaked in the astringent fluid, it cannot dilate the vagina in the way it would do if allowed to expand.

Dr. Churchill's chapter on "*Inversion,*" contains much valuable matter, collected from a considerable number of authors. In speaking of gradual inversion of the uterus from the attachment of a polypus to its fundus, he mentions Jourdan's name; altogether omitting that of Levret, who noticed, and even depicted this case at least seventy years ago; neither has he noticed the admirable observations of Dr. Dewees, in his compendious System of Midwifery, which contains more valuable matter on this subject in a short space than almost any work with which we are acquainted. Nevertheless, in justice to our author, we are bound to say that he has made amends for these omissions by the great extent and value of his quotations; the notes which he has appended to this chapter are numerous and highly interesting. His observations on the prognosis where there has been some delay are excellent. "But should the disease be of some days standing, are we to look upon the reduction as hopeless? Certainly not. There are cases on record of the attempt having been successful after days and weeks have elapsed: seventeen in Mr. White's case, twenty-four in Mr. Wynter's, twenty-seven in Mr. Dickenson's, three days in Mr. Cawley's, seven in Mr. Radford's (case 6), eight in MM. Chopart's and Ané's, eight in Mr. Ingleby's, ten or twelve in M. Lauverjat's, thirteen in M. Hoin's, and twelve weeks in Dr. Belcombe's." (p. 331.) We may add two interesting cases related by Boyer, where the uterus had resisted every endeavour to reduce the inversion, and which, in one case, remained fourteen days unreduced, and in the other more than eight years, and where, in consequence of a sudden and violent fall on the nates, reduction followed spontaneously and permanently.

The chapters on "*Diseases of the Fallopian Tubes and Ovaries*" are very excellent, and do the author much credit; he has quoted largely from our notice of Löwenhardt's Essay given in our second Volume, which contains much original and practical information. In speaking of encysted ovarian dropsy, he observes, "occasionally large veins are seen meandering over the surface of the tumour; but this is not generally the case. Arteries may also be felt pulsating sometimes; and in one case I observed a distinct *bruit de soufflet* like the placental *souffle*." (p. 364.) The literature on ovarian dropsy especially is very full; and the recent interesting cases of Messrs. Jeafferson and King, where the flaccid ovarian sac, after tapping, was drawn through the aperture and removed, are duly quoted.

Dr. Churchill's work is evidently the fruit of considerable labour; but in many cases this labour has not been so judiciously directed as could be wished. Authors of long standing and high reputation have been frequently omitted for those of later date, who have, in all probability, borrowed from them. It is but justice to say, however, that, with the exception perhaps of his diagnosis, wherever the author has given his own opinions apart from any quotations, they have been good and practical. The arrangement of his materials has not always been made with care or judgment: if we might so express ourselves, there is too little interstitial matter to blend them together and form a complete whole; and hence the value of the work to a student is considerably diminished. It is still, however, on the whole, a valuable work; and will prove useful to those who have not leisure or opportunity to consult more original writers. We doubt not that, when it reaches a second edition, the author will introduce many improvements into it; and also correct certain inaccuracies or peculiarities of expression, such as "the *accord* of symptoms," "a *grave* diagnosis," "*shedding*," for hemorrhage, &c. The work is a poor specimen of Dublin paper and print, but the price is proportionally small. Numerous typographical errors occur, and even the date on the title-page is put awry.

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## ART. VII.

*Medicine and Surgery one Inductive Science; being an Attempt to improve its Study and Practice on a Plan in close Alliance with Inductive Philosophy, and offering, as First-fruits, the Law of Inflammation; addressed particularly to the Medical Student and the Profession, but easy and intelligible to the Public also: the whole being the Introduction and first Part of a System of Surgery.* By GEORGE MACILWAIN, Fellow of the Royal Med. and Chir. Society; Surgeon to the Finsbury Dispensary; Consulting Surgeon to the St. Ann's Society, &c.—*London,* 1838. 8vo. pp. 551.

WE are not without hope that the exposition which we gave in our last Number of the objects and means of physiological enquiry may have, in a certain degree, answered the purpose which we had in view, by imparting to some of our readers sounder and more definite views than they before possessed of the character of the science, its connexion with other branches of research, and the best means of increasing its comprehensiveness and certainty. It was our intention to follow up the plan which we had there commenced, by laying before our readers, at the first convenient opportunity, a corresponding sketch of the position and prospects of some other departments of medical knowledge, on which it seems to us that similar ignorance and misapprehension prevail. Such an occasion has been presented to us by the publication of the volume of whose title-page we have given a faithful transcript above.

We have often had occasion to remark on the suspicious indications of a voluminous title-page; and we think the volume before us tends to confirm our judgment in such matters. We at once admit that the work contains a good deal of what is both correct and valuable, yet none of this appears to us to possess such claims to novelty as to demand publication in its present bulky and expensive form; still less to entitle the author to assume the credit of having founded a new system of medical philosophy, a distinction at which he evidently aims. We are quite ready to concede to him the disinterestedness of the motives which he sets before us in his preface, (p. vii.;) but, although not writing for present reputation or profit, Mr. Macilwain evidently looks to posterity for his reward. In this expectation, indeed, he does but imitate the Father of the inductive philosophy, whose pathetic appeal to future generations has been so nobly answered; but it seems to us that Mr. M., although full of the language of his great prototype, possesses too little of his spirit to stand much chance of compensation of any kind, for the trouble he has bestowed upon "the medical student, the profession, and the public also."

In noticing a former work by the same author, we bestowed upon him some friendly hints respecting the turgidity and diffuseness of his style: it does not appear, however, that they have produced the effect which we kindly intended, for the volume before us displays the same faults in an undiminished degree. Mr. M. will perhaps excuse himself on the score of his desire to render the results of his reflection intelligible to minds of ordinary cultivation; but we would assure him that plainness and simplicity are qualities more highly prized by that discerning portion of the public to which he addresses himself, than far-fetched illustrations or laboured attempts at profundity, especially when abstract truths are to be communicated and explained. In many parts of the volume, the author has adopted the familiarity of the colloquial style, with the idea, we presume, of engaging and maintaining the attention of his younger readers; but this is a dangerous experiment for a writer of Mr. M.'s powers; since it leads to the diffusion of a vast quantity of common-place over such an extent of surface, that the novel ideas, where they do occur, seem like scattered stars vainly attempting to shine through an atmosphere of mist, which not only obstructs but disperses their rays. In the perusal of Bacon's works, the reader is no less struck with the "boldness of his style, which unites the most sublime images with the most rigorous precision," than enlightened by the profound philosophy of the ideas so felicitously conveyed; and we venture, therefore, to recommend to Mr. M. that study of his great model which he loses no opportunity of enjoining upon his readers. In many parts of his book, Mr. M. shows us that he can write so plainly and unaffectedly that we are led to feel additional regret that he should not always do so. In the following passage from his preface, for example, we perceive not only the result of sound reasoning, but clearness and simplicity in its expression.

> "In common with many others of the profession, I have long been dissatisfied with the condition of the science in the cultivation of which the greater part of my life has been employed. I have sensibly felt the humiliating conviction that medical science has not kept pace with other departments of knowledge, either as to its progress or diffusion; and that, as a consequence of this, it is encumbered by many circumstances which impede its progress, abridge its utility, and derogate from the rank of those who follow it as a profession. We can in no way explain these circumstances by any reference either to the paucity of our facts (for of these we have abundance), or to any deficiency of industry in at least a considerable number of the cultivators of medical science; but it is easy to perceive that there is a great difference in the manner which has characterized our investigations, when compared with that observable in the cultivation of the other sciences. In these, there has been a pervading observance of inductive reasoning, as taught us by Lord Bacon: in medical science, we observe as pervading a violation of it. I speak generally: I do not mean that it is always observed in one case, nor always disregarded in the other. When we connect with the foregoing the fact that there is, besides, a very demonstrable correspondence in the progress of every science, and the observance of inductive reasoning in its cultivation, it is impossible to be otherwise than anxious to enquire, at least, whether the slow progress of medical science may not be attributable to that general disregard or violation of rules of reasoning which have so materially assisted the progress of all others." . . . "It has been usual to attribute the low state of medical science to the difficulties attending its cultivation. I can in no way subscribe to this opinion. Like other sciences, it has its difficulties, both general and peculiar, doubtless; but it has also great advantages. The difficulties of which we complain, as resulting from the interminable variety of form under which disease obliges us to investigate the laws of nature, show how little we have wrought in the

right way. Properly regarded, these varieties are of the highest utility: they are the strongest and most necessary tests of any law concerning which it may be our object to enquire; and furnish ready to our hands, in countless forms of disease, variations of conditions, which, in other sciences, we are obliged artificially to institute. Besides which, it is only through the multiform *gradations* in disease that we learn the true relations of this 'numerous and dissimilar family;' whilst the very diversity of character which they present produce this remarkable result, that those characters which they have in common (in all matters the most essential to know,) stand out in stronger relief, from the very diversities by which they are in different examples accompanied." (*Preface*, pp. v.-vii.)

Our readers will have no difficulty in tracing the correspondence between the views here expressed and those contained in the first article of our last Number. We there took some pains to contrast the present condition and prospects of physiological science with those of other departments of knowledge, and to analyse the causes which have operated to retard the advance of the former, and to give it a character of uncertainty. When we saw that a similar analysis of medical science was entered upon by Mr. Macilwain, with views which appeared so just, we hoped to be able to continue our former plan, by presenting to our readers the substance of his argument. We must confess, however, that his mode of conducting the enquiry differs so much from the line which we had previously marked out, that we can avail ourselves but little of his labours; and the results appear to us so vague and unsatisfactory, that we scarcely deem it worth while to encumber ourselves with them.

The title of Mr. Macilwain's book, comprehensive as it is, is likely to give rather an incorrect idea of the nature of its subjects. Our readers would scarcely have supposed that, out of the 551 pages which are contained in a treatise headed "Medicine and Surgery one Inductive Science," less than seventy are devoted to its professed object; the remainder containing discussions on the Vital Principle, Sympathy (which subject occupies about 120 pages), and not only the Law of Inflammation promised in the title-page, but the whole theory and practice relating to that disease and its various modes of action. On the present occasion we can only notice, at any length, the subject of the first discourse; but we cannot pass over the remainder of the volume without a few words of criticism.

The second discourse, on Life, displays our author's faults, both in reasoning and style, in a very remarkable degree. The subject is one which peculiarly tests the powers of a writer; and those who wish to compare the best and the worst disquisitions upon it that it has been our fortune to meet with, may place Mr. Macilwain's by the side of that of Dr. Fletcher. The first is as vague and unmeaning as the second is terse and pointed; the former is so full of irrelevant illustrations and laboured attempts at profundity, that whatever reasoning it contains is quite buried beneath the superincumbent rubbish; whilst in the latter there is scarcely a sentence which has not a simple and direct bearing upon the question, and not a single illustration but what is to the purpose. Perhaps the following passage may be within the comprehension of some of our readers: for ourselves we do not profess to understand it.

"The indisposition of the particles of matter to attraction at certain distances is very remarkable, and may be illustrated by a form of matter with which we are most familiar. Even steam will not resume the attractive force necessary to form water,

> unless heat is abstracted from it; but the tendency to form new combinations is best exemplified by water when it is decomposed. If, for example, we take oxygen and hydrogen, in the proportions in which they form water, we may conceive that, if we mixed them together in a confined vessel, they would combine; but we do not find that this is the case: *they have lost something or other on which their capacity for union depended;* and, in order to restore this capacity, we must subject them to some other influence. In their present state they present phenomena remarkably different from those afforded by water. Instead of being a ready agent for extinguishing fire, they are a most explosive mixture. We find, however, that, if we apply flame, a piece of spongy platinum, or electricity in its more cognizable form, the particles again unite, and water is produced; so that, by separating the elements of water from each other, we have deprived them of the power of reuniting; *and, whatever that is, it is evidently the principle by which the particles of water were held in combination. Deprive water of this principle, it no longer exists as water; its life, as it were, is destroyed.* Now, as regards our present knowledge, we have already arrived at an interesting peculiarity. It is not meant to be inferred that the principle subtracted must necessarily be the principle of life, but it seems reasonable to infer that it must be a link in approximation to it." (p. 75.)

What a mystification is here of the plain and simple doctrines of chemical affinity! Mr. Macilwain's intention would almost seem to have been to perplex his readers into the belief that the ordinary actions of physical laws are as recondite and difficult of investigation as are the operations of vitality; instead of endeavouring to reduce the latter, by a careful and rigid analysis, to the simplicity of the former.

Of the precision of his reasoning and the closeness of his inductions we will give another example; taking the liberty, as in the preceding extract, to indicate by italics the more remarkable expressions.

> "If we consider man, we find that he is at once connected with an infinity of other creations, by relations and resemblances which are very remarkable. In every animal, digestion, respiration, and circulation are very analogous phenomena; in many they differ not from those observable in man, either in principle or effect. In vegetables, we have nutrition, respiration, and circulation; and, in inanimate nature, the component atoms of bodies are subject to laws not less rigid and definite. All matter, whether animal, vegetable, or inanimate, is equally indestructible: *all matter* has a certain disposition to preserve its *life* in the form in which we find it in nature, until acted on by influences external to it, the particles of its masses cohering by mutual attraction: all matter has certain tendencies towards other matter. The result at which the mind arrives from this kind of contemplation is, *that the life of every thing, or at least that principle which immediately governs its phenomena, may be the same.*" (p. 84.)

We make the two quotations which follow, with the view of pointing out to Mr. Macilwain the necessity of taking more pains to ascertain facts and study principles, before he ventures to avail himself of illustrations from sciences with which he may not be familiar.

> "Some animals can breathe only in air; some only in water; and a few equally well in both. Amphibia, generally so called, are not here intended; these really breathe only in the air: but the Proteus and *Draco volans* are truly amphibious, having both gills and lungs, and appearing to breathe indifferently in air or water. (p. 83, *note.*)

We would willingly presume that *volans* is here a printer's error for *natans*, and that the designation has been applied by Mr. M. to some new species of perennibranchiate Batracian, or to one of the few already known, such as the Siren, Axolotl, &c. The *Draco volans*, as everybody knows, is a beautiful little lizard, which flutters, by means of its parachute-like appendages (formed by an expansion of the skin over its prolonged ribs), among the branches of tropical forests, in pursuit of its insect prey.

> "The powers of assimilation possessed by vegetables are very peculiar. In almost every instance, light is essential to them; by a kind of refined analysis, of which we can form no idea but by its result, they appear to derive from light various kinds of colour. That this is the essential mode in which their colour is produced, we infer from the fact that, if deprived of light, and placed under other circumstances not absolutely prohibitory of their growth, no colour is produced." (p. 80.)

The *facts* stated by Mr. M. are by no means correct; for, not to mention other instances, it is well known that Algæ of various species, which inhabit depths of the ocean so profound that they can receive but an infinitesimally small proportion of light, nevertheless exhibit hues of such brilliancy as to bear comparison with those developed under the uninterrupted influence of the solar rays. But what shall we say of his ignorance of the Newtonian doctrine of colours, or of that modification of it which is required by the undulatory theory of light, in supposing that, by some "kind of refined analysis," plants separate the component elements of that complex substance, and, keeping some of them, turn the rest adrift; just as the components of the atmosphere are separated by their respiration? We quite admit that a man may be a judicious and successful practitioner who knows nothing of one theory or the other; but one who comes forward to instruct the public, and aims at the prominent position sought by Mr. Macilwain, might be expected to take the trouble of ascertaining the possibility of his speculations before holding them forth as established facts.

In the chapters on Sympathy we find the same general doctrines inculcated as those contained in Mr. M.'s former work, on the Unity of the Body. He lays particular stress upon his superiority to other practitioners in attention to the subject of constitutional treatment, (on which question we must remain at issue with him;) and tells us (p. 140) of his success in curing "many patients," whom "the first surgeons in England" have failed to restore. In his history of individual cases, however, we are left much in the dark as to the particular means employed. Thus, we are told that, in one case, "his liver was generally solicited by applications to the bowels." Of the complaint of another we are informed, in general terms, that "it yielded to attention to the alimentary canal and skin." In another instance, we find that "simultaneous appeals were made to the skin and the alimentary canal;" and of a young lady, that Mr. M. had "much trouble with her stomach."—But we must pass on to the subject of our especial consideration.

"Surgery," says Mr. M., "is to be regarded as a branch of natural philosophy: it has well-marked and interesting relations to most of the other departments of science, and more or less of connexion with them all." "The idea that surgery is a sort of abstract science," he subsequently remarks, "is false, and therefore injurious. So far is it from being so, that there is no one which it would be more impossible to isolate from general philosophy. In no one branch of human knowledge do we find evidences of that connexion which exists between it and all others more obvious, more intelligible, or indeed more striking, than in surgery." The occurrence of such passages as these, and others which we shall presently quote, at the outset of the enquiry, leads us to enter a little more fully than we should otherwise have thought necessary into the precise character of its objects; for the nature of Mr. M.'s errors (as we, at least, deem them,) too clearly exhibits the vagueness of the ideas prevalent respecting them. We fully agree with him in the belief that, "the more we extend our views into the various departments of natural philosophy, the more are we struck with their mutual connexion;" but this connexion is but superficially and unsatisfactorily perceived until the highest and most general of the laws of the respective sciences are unveiled. To use an oft-repeated but illustrative simily, the branches of a tree may, in their endless ramifications, appear to interlace and unite; but it is not until we trace them back to their common trunk that we perceive their fundamental relation. We do not, therefore, regard the mere dependence of the actions of life upon physical agents, or the cooperation of physical laws in those actions, as indicating this fundamental relation between the sciences of life and those of inorganic matter, which must be determined by the approach of their higher generalizations. All the sciences of life may, and indeed must be, regarded by the philosophical enquirer as closely and fundamentally united; but they are separated by a distinct line from those of inert matter, by the cooperation in them of a set of laws to which nothing analogous is found in the inorganic world. We must, therefore, decidedly object to the classification proposed by Mr. Macilwain, unless the term natural philosophy be regarded as much more inclusive than it is commonly meant to be. We cannot allow that our view of the matter "precludes any enlarged enquiry into the laws, of which diseases are but the exemplification, and debars the science from that light which would otherwise be imparted to it by thousands of philosophically minded men, who are deterred at present from venturing on subjects hitherto represented to them as appertaining to a specific study." (p. 3.) The study of life, whether in the healthy or diseased state, necessarily involves that of its external conditions; and, so far as these are physical, they come within the province alike of the biologist and the natural philosopher. No reasoning founded upon these alone, and proceeding upon the ordinary laws of physics, can go far in explaining the phenomena of life; and these "thousands of philosophically minded men" must be content, therefore, to keep within their especial province, or to commence the study of vital laws in the peculiar method which they require, and on which we have already (in the article referred to) sufficiently enlarged.

But in what sense, it is to be asked, can surgery be legitimately denominated a science? How far can medicine even lay claim to this designation? What are their objects, what their respective limits? We think that we shall make it evident that the consideration of such questions is more practically important than would at first sight appear, since the whole method of our pursuit of these branches of knowledge is involved in it.

"The difference between an art and a science," says Mr. M., "seems to be essentially this: an art consists in the knowledge of certain means by which we can produce certain effects, but without our understanding the principles on which they depend, or, in other words, the laws of nature, of which they are the necessary exemplifications.

A science consists in the knowledge of a certain number of phenomena, and the power of referring such phenomena to the laws by which they are governed." (p. 7.)

Now, this definition is true as far as it goes; but it is by no means the whole truth. It is quite correct to say that, in proportion as a collection of facts is found to be amenable to certain general laws, the attainment of which enables us to extend our knowledge of similar facts, so is the *science* developed in its abstract form. But we hold it to be wrong to maintain that a perfect *art* is anything but the application of a science to practical purposes. It is very true that the *artist* may be himself ignorant of the principles upon which he works; but these, to be of *universal* application, must be deduced from laws of high generality, by a mind fully capable of perceiving their bearings. When this is accomplished, we find that an art assumes a character for perfection exactly proportional to that of the science upon which it is founded, after due allowance is made for the uncertainty resulting from the intervention of human agency.

We formerly spoke of astronomy as the most perfect of the sciences, since all its phenomena can be reduced to one general principle. Its most important practical application is the *art*[*] of navigation, which is a collection of rules framed by those profoundly conversant with the principles of the science, but capable of being employed by those whose knowledge extends no further than the mode of applying them. It is well known that, to such perfection has the art of navigation arrived, that it is quite possible for the place of a vessel to be determined within a mile, after she has been driven by winds and currents for many months subsequently to the last accurate knowledge of her position. For such precision two kinds of conditions are evidently requisite: the certainty of the rules, arising from the impossibility of their correctness being impaired by the intervention of any unknown cause, which can only be secured by the comprehensiveness of the principles on which they are founded; and the skill of the individual concerned in the application of these rules, the *artist*, and the perfection of the instruments which he employs. These conditions are obviously so independent that a profound mathematician, capable of admirably fulfilling the first, may be totally inefficient with regard to the second; and, in like manner, a correct observer may easily acquire the knowledge sufficient to avail himself fully of the rules of the art, although totally ignorant of the abstract principles on which they are founded.

We have purposely taken this illustration from an extreme case, that we might be able to enforce our doctrines with more clearness; but we shall find the results to be similar if we turn to other sciences much less advanced, and enquire into the state of some of their dependent arts.

In chemistry, as we have already stated, the facility of experimental enquiry has led to the establishment of laws of a high degree of generality; but these require so much qualification and restraint in their application to particular instances, that it is hazardous to predict unknown phenomena, especially where organic compounds (of whose nature we are more particularly ignorant) are concerned. Hence no art founded upon this science can be supplied with other than very limited rules. In the case of dyeing, for example, great improvement has been effected by chemical knowledge; but the greater part of its rules are *empirical,* that is to say, founded upon a limited induction, not comprehended in more general principles, and therefore quite uncertain in their application to unknown cases. Thus, if a new animal or vegetable dye were discovered, the modes of fixing and discharging it, and of varying its shades of colour, would have to be determined by experiment before it could be brought into advantageous employment. We can, nevertheless, imagine, and in some degree anticipate, the period when chemical science shall be so far advanced that a simple analysis of the material (analogous to the *observations* of the navigator) may enable the manufacturer, by a reference to his code of rules, to avail himself to the fullest extent of its capabilities, without being himself aware of the principles upon which those rules are founded. As long as science and art are in a progressive state, it is obviously advantageous that the *artist* should be cognizant of the principles of the science, that he may not neglect opportunities for the improvement of both; but, when once perfected, we have seen that no such utility exists.

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[*] Navigation may also be regarded as a *science*; but it is then only a branch of the science of astronomy.

An art, then, will be *scientific* or *empirical** according to the comprehensiveness of the principles upon which its rules are founded. A little consideration will show that this distinction has nothing to do with the certainty or uncertainty of its applications in particular instances. An art may be entirely empirical, and yet be perfect *so far as it goes*; but no unknown cases are provided for, no contingencies foreseen. It is in their adaptation to these that the triumph of a scientific over an empirical art manifests itself; and, in proportion as, from the nature of the subjects embraced by it, a greater or less variety of novel cases presents itself, in that proportion is the superiority more evident. The deficiency of higher or more comprehensive laws should not prevent us in any instance from making cautious use of those we already possess; and, where the demands of mankind require that an art should be practised even in its imperfect condition, we must be content with such means of satisfying them as lie within our reach. Contentment, however, by no means involves a tacit acquiescence in the infirmities of our condition; and the man of noble and elevated mind will not only aim at the perfection of his science from that abstract love of knowledge, which, as Sir H. Davy has beautifully observed,† "is, in fact, in its ultimate and most perfect development, the love of infinite wisdom and unbounded power, or the love of God," but may also safely cherish the belief that every contribution he makes to the establishment of general laws will ultimately have its practical bearing upon the condition of humanity, and that future generations, if not his own, will be benefited by it. It was beautifully observed by Lord Bacon, that "it is the office and excellence of all sciences to shorten the long turnings and windings of experience;" and we shall hereafter see how forcibly this remark applies to the topics of our present consideration.

We return to Mr. Macilwain's attempt to rank surgery as a science:

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* See Herschel's Prelim. Disc.; p. 70, &c.

† Consolations in Travel; 3d edit., p. 53.

"Medical science," he says, "is, in fact, the study of the laws and relations of animal bodies, in order to ascertain the modes in which nature relieves diseases or repairs accidents; and to determine the conditions of the whole body which favour or impede these processes in its various parts; with an especial view, in the one case, to the *maintenance* of such conditions; and in the other, to their *removal*. The achievement of this point is the common object of medicine and surgery. Surgery is sometimes called an art. If it be an art, however, it is one of the lowest description; inferior indeed to the most ordinary handicraft. As well might you call astronomy an art, because it requires the adjustment of optical instruments. Surgery is never an art but in consequence of its imperfection as a science; and even then it is one of very simple character. As an art, and one that can be acquired in less time than any with which I am acquainted, it will remove a stone from the bladder, will tie an artery, or will amputate a limb; but, as a science, it would prevent the formation of the stone, the changes in the coats of an artery, or the forces of the circulation which produced the aneurism, and put a stop to the morbid actions necessitating the removal of the affected member." . . . "In thus contending that surgery should be regarded as a science, it is necessary to observe that I speak more with reference to what it must necessarily be as a branch of natural knowledge, than to any *actual* possession which we have of such knowledge." (pp. 6, 7.)

In the foregoing observations, Mr. M. seems to us to have laboured under some difficulty, through confounding the two senses in which the term art is commonly employed. In many cases it is used synonymously with "skill, dexterity, or the power of performing certain actions acquired by experience, study, or observation." In this sense, it would be correct to say that the mere performance of many surgical operations requires art of the lowest description. We have seen many bones sawn through in a manner that would disgrace the most ordinary workman; and the lithotomic skill of Frère Jacques might easily have been acquired by a barber or butcher properly instructed in his method.*

But art, in its higher sense, implies "a system of rules serving to facilitate the performance of certain actions;" or, in the words of Sir J. Herschel, (the worthiest representative of Bacon in our time,) it is "the application of knowledge to a practical end." It is evident, therefore, that, as the essence of medicine and surgery consists in their practical applications, they are to be regarded as arts and not *sciences;* and it is to be lamented that their rules are as yet so little founded upon general principles, and stand so frequently upon the narrow basis of a limited induction, that we must consider them as rather empirical than scientific.

We formerly showed† that the changes which characterize living beings, and which in their totality constitute their life, are capable of being referred to certain general laws expressive of their uniform conditions. Organized structures possessed of vital properties, on the one hand, and the elements of the inorganic world on the other, afford these conditions; and, by the study of all the phenomena which are presented by the actions thus resulting, the science of physiology will, we doubt not, be gradually built up and perfected. To pursue it with advantage, an acquaintance is necessary with the characters of the conditions themselves; and thus arises the study of organized structures on the one hand, and that of much of this difficulty, however, to the backward state of the science of general physics on the other. Anatomy is not a science as long as it is a mere collection of facts, "rudis, indigestaque moles," but as soon as the laws regulating the structure and development of the countless varieties of living beings begin to be evolved, the chaos of materials assumes a definite order; and one of the most beautiful as well as most interesting —because most rapidly advancing—of all sciences, philosophical anatomy, presents itself.

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* We believe it to be a fact pretty generally known, that after this celebrated empiric was made acquainted with the anatomy of the parts which his knife incised, he was much less successful than before, in consequence of his more timid method of operating.

† British and Foreign Medical Review, vol. V. p. 328, et seq.

Physiology, then, being the science of *normal life*, if we may be permitted the expression, it will be enquired what is its practical application,—to what *art* does it furnish rules? The answer is easy: to the art of preserving the body in health. How successfully this has been accomplished of late years, and how rapidly the diffusion of sound physiological principles is spreading more rational ideas as to the education and management of the body and mind, we need not stop to point out. As physiology is still, however, but a very imperfect science, the rules of art deduced from its principles are, like those of chemistry, limited in their application, and not trustworthy in novel and untried instances. We do not anticipate the return of an epoch of patriarchal longevity, whatever may be the ultimate perfection of physiology as a science; but the experience of the last century has amply shown that attention to its simple and universally acknowledged truths will go far towards both the prolongation of life and, a not less important object, its emancipation from disease.

The slightest observation of the actions performed by living beings proves to us that, independently of those *secular* changes which cause the decay of individuals and races as constantly as their renewal, there are occasional deviations from this regularity, which constitute the phenomena of disease. The investigation of these phenomena, and the reduction of them to general laws, expressive of their conditions, is the object of pathology, the science of *abnormal life*. Here, as in the kindred science of physiology, the study of all the conditions is requisite; and hence we have to make ourselves acquainted, on the one hand, with the characters of all the external agents which can produce a deleterious effect upon the living body, whether their operation be mechanical, chemical, or vital; as well as with the results of the suspension, partial or complete, of the conditions by which its healthy action is maintained. On the other side, we have to investigate the changes of structure which manifest themselves in the body itself; and the countless variety of secondary results which arise from any disturbance of its regular train of actions. The difficulties which beset us in these enquiries are of a corresponding nature to those which we have set forth as encumbering the pursuit of physiology. We shall be comparatively brief, therefore, in the exposition of them.

The pathologist sets out with endeavouring to determine the individual phenomena of diseased action; and, when he has collected these in sufficient amount to serve as the basis of an induction, he endeavours to determine the conditions common to all, and hence to arrive at their laws. With what difficulties the first part of the investigation is encompassed is sufficiently evident from the discrepancy of opinion which prevails as to the nature of the simplest and most easily observed changes; such as those which occur in inflammation. We cannot but attribute physiology. Until the normal functions of the different organs are thoroughly understood, we cannot deem it probable that there will be much consent as to the character of the deviations from them; and, in the case we have referred to, it seems to us necessary that physiologists should agree upon the connexion between the motion of the blood in the capillaries, and the vital properties of the fluid and the changes it ought to undergo in them, before a correct statement can be made of the morbid phenomena of inflammation. Such a statement would not be entitled to rank as a *law;* it would only be a *fact*, as we formerly showed with regard to the circulation, &c.; but, if the nature of changes which can be brought under direct observation is likely to remain for some time a disputed topic, how much more difficult must it be to discover those which occur in the recesses of the system! Great as have been the improvements in diagnosis of late years, they too often only reveal to us the sensible *results* of the changes which constitute diseased action, rather than the original changes themselves; and, in reasoning backwards to the cause, we are obviously exposed to fallacy at every step, from the limited use of general principles to which our ignorance of vital actions restrains us. Thus, auscultation will reveal to us the deposition of tubercular matter in the lungs, and the *consequent* chain of phenomena; but it will give us no information of the character of the *previous* change in the capillary circulation which is the cause of the deposit; and our knowledge of it is therefore still more indefinite than of that concerned in inflammation.

It is evident, therefore, that the difficulties of diagnosis are not only felt by the practitioner (as they long must be,) who endeavours to make his scientific knowledge available for the exercise of his art; but they also obstruct the progress of those researches into the elementary phenomena of disease, upon which alone the general principles of pathology can be established. The observation of the morbid changes produced by disease, either when presented to our inspection during life, or discovered by examination after death, may, in some degree, supply the deficiency; but this too often supplies us with information of no higher character than that derived through other channels, revealing to us only the *results* of diseased actions, rather than the nature of the actions themselves.

But the difficulties are greatly enhanced when we consider that the cases of abnormal action, in which the structure of the part functionally deranged undergoes a sensible alteration, bear but an extremely small proportion to the instances of sympathetic disorder from irregularity of some other portions of the series of vital changes. We have said of nervous agency in the healthy state, that we know nothing of its nature, and can only judge of it by its effects; the same observation may be applied with increased force to the state of disease; for it would seem useless to hope for the detection of any physical changes in the nervous system corresponding with the results which disordered innervation produces through other organs: and that which must be kept in view in reasoning on morbid phenomena in general is especially applicable to the nervous system, that we must be careful not to connect too hastily the observed symptoms with the morbid appearances. From error in this respect many erroneous doctrines have arisen. Our object should rather be to discover what has been the primary morbid action of which the diseased condition is the sequence, and whether a similar morbid action may not have occurred in cases where we find no such *evident* result.* Thus, epilepsy is a manifestation of abnormal action of the nervous centres, of the nature of which we are profoundly ignorant: this abnormal action may originate in disorder of a distant organ, and may leave no recognizable manifestation after death; or it may be the result of some other change in the organs themselves, which, leaving manifest traces, may be hastily set down as the proximate cause of the disease; or, from its long continuance, it may produce some structural alteration, which may in like manner be hastily conceived to be the cause of the symptoms when it and the symptoms are the common result of some still mysterious cause.

The difficulties which impede our attempts to become acquainted with the changes in the external elements, and the operation of deleterious agents are sufficiently great to retard the progress of the enquiry; especially whilst we are still so much in the dark respecting the natural effects of vital stimuli. We cannot, however, regard them as by any means insurmountable; and, though we cannot weigh or measure the quantity of malaria in the atmosphere or bring into a tangible form, the fomites of a contagion, we may make ourselves acquainted with the laws of their origin and distribution, and the mode of their deleterious action on the body. We think it will appear, then, that, by a judicious subdivision of labour, the science of pathology may gradually be elevated from its present uncertain condition; and that, by the cautious generalization of carefully-sifted and correctly-observed facts, abstract principles may be developed, possessing a certainty not inferior to those of the so-called *exact* sciences. As we formerly observed, the process of induction is essentially the same in all instances. It is in the establishment of individual facts that the great difficulties exist, which have so much retarded, and will continue to delay, the progress of the sciences of life. We should recollect, however, that the infirmities of mankind are constantly presenting us with the opportunities for observation; and that the variety of diseased phenomena may be rendered, as Mr. Macilwain has justly remarked, a source of information, rather than of perplexity. It is the great dissimilarity of the facts so collected which prevents any one but a sagacious philosopher from discovering the common tie which connects them all. The brilliancy of Newton's genius was shown in the perception that the fall of a stone to the earth and the motion of the moon around it were analogous phenomena, subject to the same law; not in the mere deduction of the numerical law from the ratios supplied by these facts.

As the *art of preserving health* arises from the proper application of the *science of physiology*, so does the *art of curing diseases* or injuries depend upon the *science of pathology*. But its rules must result in part from the application of another science, that of therapeutics, which might justly, however, be considered a branch of pathology, or the science of disease. As life, in the healthy condition, is known to be maintained by the action of external agents upon organized tissues endowed with vital properties, so is it found that, in diseased states of the system, such a change takes place in the character of these actions as adapts them to its altered circumstances; and it is also found that, by artificial regulation of the natural actions, or by the substitution of new ones, the diseased condition may frequently be controlled, and a state of corporeal sanity be restored. The enquiry into the curative influence of external agents upon the phenomena of disease is, therefore, a branch of the general science of pathology (though not usually regarded as such,) which corresponds precisely to the study of the ordinary relations between organized structures and the external world, which is universally allowed to be a most important branch of physiology.

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* See British and Foreign Med. Review; vol. IV. p. 428.

The *ars medendi*, then, or the *practice* of the healing art, is based upon the *ratio medendi*, or its *theory*. Were the science of pathology more nearly perfect, the rules deduced from its general principles would be of easy application, and would require only correct observation of the circumstances which called for their employment: but this, unfortunately for us, is very far from being the case. Were we able to devote all our energies to the advancement of pathological philosophy, we might venture to anticipate the commencement of this medical millenium: but the infirmities of mankind continually distract our attention by the claims they put forward for the imperfect assistance which we are as yet able to afford them. Hence we find that the attention of those who devote themselves to the study is directed rather to the alleviation of suffering by the temporary expedients which a limited experience has shown to be useful, than to the establishment of higher and more general principles which might for a time be barren of practical results. From this it follows that medicine, as an *empirical art*, has advanced far beyond the science of pathology; and that it has little claim to be regarded as a *scientific art*. That its rules are based upon an extremely narrow foundation is evident to any one who attempts to extend their application to novel cases, as well as to those who have experienced the distressing uncertainty of medical practice in the inability to control particular forms of disease by the means usually efficacious. It is from the want of analysing the causes of this uncertainty, and from misunderstanding the present position and prospects of medicine, that many ardent and hastily-judging men have abandoned the subject in disgust, under the notion that no sciences connected with the constantly-varying actions of life could ever emerge from such a chaos of ignorance and uncertainty. We trust, however, that we have shown in this and a former article, that the actions of living beings, whether normal or abnormal, are as amenable to general laws as those of inert matter; and that the discovery of these laws is within the reach of those who search after them in the right track. That we are still so far from attaining them is not to be wondered at, when we reflect upon the errors and absurdities of the methods which have been employed in their pursuit: one class of reasoners hastily raising a pile of flimsy principles upon a few ill-observed facts, and calling upon the world to acknowledge their truth, and be killed or cured upon the rules of art derived from them; and another falling into the opposite extreme, and doggedly refusing to call in the aid of reason at all, or to advance one step beyond the bounds of certain experience, however limited. The latter error has been very properly reprobated by Mr. Macilwain.

> "In animals, and (from the superaddition of moral causes) especially in man, we have not the same opportunity of assuring ourselves that we have observed phenomena in any two cases under precisely the same circumstances, as we possess in the study of physical changes. No wonder, then, that we cannot as easily arrive at conclusions

> by strict induction as they can who pursue other sciences; but it is to be feared that the task, when its difficulty should have given rise to a corresponding increase of exertion, has been too often rejected as hopeless; and that, because we could not arrive at conclusions which were certain, we have not sufficiently striven at such approximations as are really within our grasp." (pp. 36, 37.)

Accordingly, in the practice of medicine, our aim must be to avoid, on the one hand, confiding too implicitly in general principles, however stable and comprehensive we may imagine them to be, until we are satisfied that we know not only the principles themselves, but the subordinate laws which regulate or modify their application to individual cases. Long after the highest laws of motion had been established by Newton, no astronomer could, on the faith of them, have predicted the situation of a planet with more than an approximation to certainty: the law of attraction had to be applied in numberless modes not contemplated by its discoverer, before perfect accuracy could be attained. There is great danger, then, in the present state of the science of pathology, in trusting to principles which we may consider unassailable, as our sole guides in the practice of our art; and hence it is that it is not always the *scientific* practitioner, as he is emphatically termed, who is the most successful in his treatment. Nothing, indeed, can more strongly display the present uncertainty of medicine than the superior efficacy often to be witnessed in the treatment of an acute and observant watcher of symptoms, who may not even pretend to form a diagnosis. The other extreme, however, is equally to be avoided, especially by those who aim at something more than individual success. We read in Celsus of the Εμπειρικοι of his day, who professed to confine their practice within the limits of experience only; but, as he very justly remarks, some kind of reasoning is necessary in the application of experience, however acquired, to particular instances. The recorded experience of ages, in its condensed form, must of necessity assume the appearance of general rules of practice: hence, no one, who professes to be guided by it, can avoid making more or less use of scientific principles. The *rational empiricism*, as it has been termed, which prevails in this country at the present time, may justly, we think, be regarded as the most advantageous means which could be pointed out for advancing the progress of pathology as a science, at the same time that use is made of medicine as an art. The value of *facts* as the only sure basis of general principles is duly appreciated; and yet there is no indisposition to make trial of such principles when announced, and to abide by them so far as they shall appear practically available. The *empirics* of the present day, however, whether within or without the pale of the profession, by no means take the ground occupied by those of the ancient schools. Instead of abiding within the narrow limits of experience, they erect the most hasty generalizations upon a few limited instances, and trumpet forth a remedy as endowed with panaceal virtues, or as curing all forms of particular diseases, which has been in some individual cases successful.

Whilst, then, we acknowledge the value of the contributions which have been made (and which we continue to expect) to the science of pathology, by those who apply medicine and surgery as arts, we cannot but believe that great advantage would result from exclusive devotement to the former of men qualified by original character and extended education to labour in its higher departments. We say extended education, because we believe, and we hope to have satisfactorily shown on a former occasion, that no man is so likely to reason philosophically in the investigation of vital phenomena, as one who has received a careful training in the school of physical science. We quite agree with Cullen and M. Louis in the belief that there are false facts as well as false theories in medicine; and that a long course of observation is necessary to establish the basis alone of future inductions. The band of observers, therefore, which the last-named physician is training up *may* contribute much to the advancement of *science;* but we doubt much whether they will do more, on their present plan, than render the *art* more empirical.

We will not do the author the injustice to close this article without noticing so very material a part of his work as that which enounces the *Law of Inflammation,* to the discovery of which Mr. Macilwain believes that he has been led by the application of the inductive process to the phenomena of disease.

> "The law, then, is, that inflammation is essentially a reparative process, and that, where the repair required is that of local injury, it is of course referred to the seat of such injury: in all other cases, it is a process instituted by nature to get rid of injurious influences, by determining to the surface of the body." (p. 285.)

To what degree of novelty this doctrine can lay claim in regard to inflammation in particular, we shall not stop to enquire; simply referring our readers to the works of John Hunter, and especially to section sixth of his chapter on the Fundamental Principles of Inflammation, where we meet with the following passage, which has been so often commented on as to be tolerably familiar, in substance at least, to most students of the present day: "Inflammation is to be considered only as a disturbed state of parts, which requires a new but salutary mode of action to restore them to that state wherein a natural mode of action alone is necessary. From such a view of the subject, therefore, inflammation, in itself, is not to be considered as a disease, but as a salutary operation consequent either to some violence or some disease."* Whatever may be the validity of the principle itself, we cannot but feel some surprise that Mr. Macilwain has brought it forward as the result of his philosophic researches, without any reference, that we have met with, to the well-known doctrines of Hunter; and our readers will probably be led to form no very exalted opinion of the powers of reasoning displayed by our author, when they compare this *law,* professedly deduced from the observation of phenomena, with the *postulate* on which he started (p. 95), and which is as follows:

> "That the body contains within itself certain powers of preservation, of maintaining an equilibrium under a variety of disturbing influences; *that diseases, in all their diversified forms, are, in fact, but actions of this preservative power;* and that the operation of the latter takes place, subject to certain laws of limitation, of which our present knowledge enables us to form no very correct idea."

To us it appears that the latter (if admissible) is the more general law of the two, and that the former is only a particular instance of it. It is a maxim based on common sense, as well as on higher authority, that we shall judge of things by their fruits: assuredly, the specimen we have had in the *first*-fruits of Mr. Macilwain's application of inductive philosophy to medicine and surgery, does not encourage us to look for any more valuable ones at a future time.

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* Hunter's Works, vol. iii. p. 296.

## Art. VIII.

*De la Morve et du Farcin chez l'Homme.* Par P. Rayer, M.D., Médecin de l'Hôpital de la Charité, &c. (*Mémoires de l'Academie de Médecine,* tome vi.)—*Paris,* 1837. 4to. pp. 878.

*On Glanders and Farcy in Man.* By P. Rayer, M.D. &c.

It is at once instructive and interesting to follow the gradual and steady progress from surmise to conviction, from a single vague fact to the imposing mass of evidence in the volume that lies before us, respecting the possible transmission of the poison of glanders from the horse to the human subject. We need, therefore, offer no apology for indulging in a tolerably full abstract of the well-digested historical survey with which that volume opens.

The earliest hint on the question of such transmission is said by M. Rayer to be contained in a work by Waldinger, published in 1810, in which the author enjoins the strictest caution in opening the bodies of glandered horses, as the most deplorable consequences, even death itself, may ensue, if the inoculation of pus from the diseased animal be allowed to take place through an abraded surface. In 1812, Lorin, a French surgeon, published the case of an individual who suffered under severe inflammation of the fingers of both hands, in consequence of a prick received in operating on a horse affected with button farcy. Observations resembling these, which allude simply either to local inflammations or affections similar to those resulting from the absorption of matter in ordinary dissection-wounds, had been made in various quarters, when (in 1821) Schilling's well-known case appeared in Rust's Magazine. In this case, designated by Rayer—justly, as it appears to us, though its nature is still a matter of doubt for some,—as "the first distinct and well-characterized instance of acute gangrenous glanders in man," the evidences of specific infection consisted in the peculiar glanderous discharge from the nostrils, a pustular cutaneous eruption, and a gangrenous state of the nasal integuments. In the Edinburgh Medical and Surgical Journal, for April, 1822, is to be found a brief and unsatisfactory report of the case of a man, who wounded his hand in cutting up a glandered horse, and shortly after presented all the symptoms of the disease, as observed in the horse: at the end of a week, he is stated to have died in violent delirium of "*confirmed glanders.*" The Number for January, 1823, of the same Journal, contains a letter from an anonymous London physician to Dr. Duncan, in which allusion is made to some important facts relating to the subject before us. The writer refers, as to a well-known circumstance, to the malady of a veterinary surgeon, that had recently terminated fatally, having been "contracted from a farcy horse, and characterized by ulceration in the part of the arm infected, and by what are called farcy buds extending up the arm." And, farther on, he informs his correspondent that there had lately been a patient, at one of the London hospitals, with a sore arm, resulting from a wound and contact with the leg of a farcied horse; that an ass was inoculated with matter taken from the patient's arm; that farcy, followed by glanders, ensued; and that, on the animal's death, ulceration of the septum narium was detected. In 1822, Remer, in an essay on the Diseases of Animals communicable to the Human Being, asserted that "the glanders of horses infects men." In the same year, an Italian practitioner, Tarozzi, published an account of a pestilential disease that originated in a stable where a glandered horse was kept. Of thirty-five individuals who visited the stable frequently, eleven were seized with an affection characterized by fever, and an eruption of boils and gangrenous phlyctenæ. No allusion is made in the description, however, to a morbid condition of the nares.

The next contribution of importance appears in Mr. Travers's essay on Constitutional Irritation. The work and its doctrines are so well known that we should consider it unnecessary either to notice the facts it contains bearing on our question or the author's deductions therefrom, were it not that M. Rayer has very clearly mistaken the nature and scope of the latter, and given Mr. Travers credit for opinions other than those he maintains. In the cases observed by Messrs. Travers and Coleman, affections possessing the characters of farcy and glanders were distinctly communicated from diseased animals to the human subject; and, on inoculation of the pus secreted by the latter into healthy asses, the specific disease made its appearance, and terminated fatally in the animals experimented on. M. Rayer naturally infers that, on evidence so conclusive as this, Mr. Travers must have admitted the possibility of the transmission of glanders to man: but the fact is otherwise. Mr. Travers, in spite of what he had seen, declares that "there is no evidence of the poison of glanders acting otherwise on the human body than as the poison of dead animal bodies." And, in order to reconcile his experience with the preconceived theoretical notion to which he evidently accords the preference, he has recourse to the extravagant hypothesis that the *specific* property of the poison is retained in its passage through the human system, though incapable of specifically affecting it!

The Medical Gazette for July, 1829, supplies the next valuable document in order of priority, in the form of a well-marked case of glanders, observed by Mr. Andrew Brown. The prominent features of the case were as follows:—Contact with a glandered horse; appearance of cutaneous pustules terminating in gangrene, and of tumours round the joints; "the right nostril contracted, and gummed with an inspissated discharge;" after death, "well-defined ulcerated tubercles discovered in the Schneiderian membrane, exactly the same as those of acute glanders in the horse;" pus in the muscles, under the skin, and in the frontal sinuses, &c. Mr. Brown regards his case as of importance in regard of a question first (we believe) mooted by him; namely, whether the disease be transmissible without the aid of an abraded surface.

This series of facts, joined to many others which we have no space to notice, was sufficient for forming the groundwork of a moderately complete history of the disease in the human subject, and, at least, for placing the reality of the specific transmission beyond the reach of doubt. Strange to say, however, they aroused little attention, and produced no conviction; nor was it until the appearance of Dr. Elliotson's paper in the Medico-Chirurgical Transactions (read in June, 1830,) that the subject received the notice due to its importance. In this respect M. Rayer does full justice to the deserts of our countryman, and he acknowledges that to Dr. Elliotson he owed his first notion of the disease. Besides supplying three additional and very distinct cases of the affection, the paper alluded to possesses the sterling merit of having made public what had previously been known only to the few. But, so far as we can ascertain, it contains no then-unnoticed fact bearing on the subject; while, it must be admitted, its cases are particularly valuable, as they were observed and registered without any preconceived notion as to their character on the part of their author. So inexplicable, indeed, did their phenomena appear to him at the time of their occurrence, that, when asked for his opinion of their nature, he "declared himself perfectly at a loss." The publication of Mr. Andrew Brown's case enabled him, as he avers, to clear up the diagnosis, and convinced him that the novel affection he had observed was none other than glanders.

From this period cases and papers multiplied in German and English journals. Of all these M. Rayer has shown himself a faithful chronicler. They have now accumulated to such an extent that it is hardly just to consider the disease a *rare* one absolutely, much less relatively speaking,—that is, if we pay due regard to the limited number of the individuals whose position exposes them to contagion. Dr. Graves, in a Lecture recently published, maintains it to be so common an affection in the provincial parts of Ireland, that, in his opinion, the legislature should, in imitation of the Prussian authorities, subject glandered animals to official supervision.

The author begins the proper subject of his essay by distinguishing in man, as in the Solipeda, three varieties of the acute form of the affection; the pustular, the gangrenous, and the pustulo-gangrenous.

> "In some cases, the most striking phenomena consist of a pustular cutaneous eruption, a thick and glutinous nasal discharge, and a typhoid aspect. In others, the symptoms of nasal lesion are obscure, while the external characteristics (the pustular eruption and gangrenous affection of the skin) predominate. Again, pains in the limbs, purulent deposition in various parts of the body, and inflammation of the lymphatic vessels and glands, form, in the outset, the most striking features of another set of cases, still more strictly analogous in character to the acute farcy glanders of the horse. Lastly, there are others more violent and promptly fatal, in which all these morbid changes and symptoms appear rapidly and at once, a few days after vague initiatory phenomena." (p. 642.)

But the classification adopted in the arrangement of his collected cases is founded on another, and perhaps more important, view of the subject,—the causation of the disease by inoculation, or, as all appearances seem to indicate, by infection without contact. The question of simple infection, as we have already stated, was touched on by Mr. Brown; and the case so diligently observed by that gentleman was of a kind fully to warrant his evident, though not professed, belief that his patient had been the victim of infection. He informs us that "the absorbent vessels of both arms, as well as their glands, were found in the natural state; nor was there the slightest appearance of either absorbent, glandular, or cutaneous inflammation; nor the slightest breach of integument or abrasion of skin by which the absorption of morbid matter into the system could have been facilitated." The first part of this statement contains the distinctive character of a "class of cases in which no local results of inoculation are observed; the first symptoms being those of a general infection of the system. In another, the earliest phenomena are those of angeioleucitis, local phlebitis, or diffuse cellular inflammation in some region of the body, usually the arm; while the lesions and symptoms of *glanders* do not appear until subsequently." (*Rayer*, p. 643.) This broad distinction in the phenomena observed, coupled with the fact that, in cases of the first class, the closest scrutiny failed in detecting any injury of the skin, constitutes M. Rayer's reason for admitting, provisionally at least, the transmission of the disease to man by simple infection. The English reader, prepared as he has been for it by the surmise of Mr. Brown and others, will, on the adduced evidence, probably accede to this conclusion without much difficulty. To the countrymen of the author, however, it must appear somewhat startling; at least, unless their notions on this and analogous subjects have undergone much modification since 1832, when Parent-Duchatelet asserted that "none of the supposed infectious diseases of animals exercised an unfavorable influence on the health of man."*

For our own parts, we place more confidence in the natural and clear symptomatic distinction of the cases into two classes; the primitively local, and those in which, from the first, the whole frame is affected, than in the absence of discernible breach of surface as indicating the reality of infection. Dr. Elliotson was of opinion that "abrasion of the surface is necessary to the production" of the malady, basing his belief on the notion then (1830) prevailing among the majority of veterinary surgeons, that "glanderous matter never excites the disease in the horse, even if applied to the pituitary membrane, while the surface is entire:" but this opinion is no longer general. In a recent and valuable paper, Dr. Eck, of Berlin, expresses himself thus: "And our medical police regulations assume as proved the contagious nature of glanders; a fact, indeed, of which experienced horse-dealers scarcely anywhere entertain a doubt. Now, though the glanderous nasal discharge is recognized as the chief vehicle of infection, and, on this account, the use of harness, mangers, drinking vessels, &c., which have been in contact with glandered horses, and are liable to be brought in contact with the pituitary membrane of sound animals, is especially to be feared; yet it can hardly be doubted that the contagion may exist in the other excretions; nay, that occasionally, as in damp stalls, infection may take place without any observable contact, probably through the medium of the respired air."†

Our author has collected nine examples of the disease resulting from infection. Of these the first alone is original, and supplied the "motive and the cue" for the compilation of the present bulky treatise. For the details of this case, which occupy twenty pages, we must refer the reader to the original. The chief points in the history may be briefly given thus: A groom, who slept in a stable near a mare affected with acute farcy glanders, is seized with typhoid symptoms, subcutaneous abscesses, pustular and gangrenous eruption of the skin, nasal fossæ, and larynx; ecchymoses and eschars below the ear, on the glans penis and feet; small abscesses in the legs, large subcutaneous and muscular depositions of pus, &c. The fluid from the pustules and phlyctenæ produced glanders in a horse by inoculation. The other cases originating, according to Rayer, in simple infection are those reported by Brown, M'Donnell, Elliotson, Remer, Wolff, Alexander, and Schilling. In two of these, mention is made of a slight scratch. Six cases of acute glanders, produced by inoculation, follow, conducting us to the author's summary on this form of the disease.

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* Gazette Médicale, Déc. 1832, t. iii. No. 118.

† Medicinische Zeitung, Mai 3, 1837. No. 18, s. 85.

In addition to the resemblance as to cause, course and duration, that subsists between acute glanders in man and affections produced by the absorption of pus or morbid poisons, it would appear that there occurs in this disease, as in eruptive fevers, a variable though limited period of incubation. Thus, after inoculation of glanderous matter, a period of from two to eight days, or more, elapses before the occurrence of any notable symptoms. At the end of this time the evidences of local irritation appear at the injured part; its extension follows, as is proved by the condition of the neighbouring lymphatic glands and vessels, and by general febrile phenomena. In some instances these local symptoms were slight, and easily relieved; the patients even appeared on the brink of recovery, when the essential and specific symptoms of glanders supervened. When the disease is contracted by *infection*, the invasion is marked by fever, attended with gastric symptoms, diarrhœa, or pains in the limbs.

Ushered in in this manner, the disease ordinarily presents the following phenomena in its course:—Articular or muscular pains, in some cases simulating rheumatism, followed by subcutaneous, circumscribed, painful swellings (the probable result of angeioléucitis), which either undergo superficial mortification or are converted into abscesses, containing laudable or sanious pus; a yellowish, viscous, nasal discharge, of limited quantity, issuing, in the great majority of cases, from *both* nares, and first observed from the fourth to the sixteenth day, accompanied in some instances by a similar excretion from the mouth or eyelids; occasional tumefaction of the nose and adjoining parts, followed by gangrene in one-eighth of the cases; in very rare examples, swelling of the submaxillary lymphatic glands, or depositions of pus therein; and pretty frequently inflammation of the throat and tonsils; a peculiar pustular eruption, differing from all varieties hitherto observed,—viz. gangrenous bullæ appearing, towards the twelfth day of infection, on the face, arms, thighs, and anterior surface of the trunk, and sometimes preceded or accompanied by profuse fetid sweats; rapid and full pulse at the outset, subsequently weak, depressible, and sometimes intermittent, and, as death approaches, extremely small and frequent; diarrhœa, with watery stools of cadaverous smell, and occasionally containing dark-coloured blood; dental sordes; dry, brown tongue; tympanitic abdomen, with hardly any abdominal tenderness; thirst in a few cases; difficult deglutition; occasional vomiting, especially towards the close; *no typhoid* *maculæ* on the surface; violent cerebral symptoms, terminating in delirium, coma, and death.

As respects the mortality of the disease, M. Rayer states:

> "Acute glanders in the human being has hitherto invariably terminated in death, (one doubtful case excepted,) and two-thirds of the patients perished before the seventeenth day. Two died from the twenty-first to the twenty-eighth day; one only survived to the fifty-ninth. In the latter case, the early symptoms were those of farcy; those of acute glanders supervening as the final phenomena, in the same manner as has sometimes been observed in horses." (p. 728.)

In analysing the results of the recorded examinations after death, M. Rayer found two obstacles in the way of accurate conclusions,—the imperfect manner in which the investigation was almost always conducted, and the use, by different writers, of the same word in different senses. Thus, the various significations which the term "tubercle" is made to assume in this country, according to the whim of authors, are productive of much confusion among ourselves; while to a foreigner, accustomed to limit its application to the anatomical character of phthisis and to a particular class of cutaneous affections, this inaccuracy of language renders much otherwise valuable matter completely unintelligible. Spite of these difficulties, the following results are valuable:—Externally traces of the pustular eruption are constantly observed, besides, in the majority of cases, gangrenous bullæ or cutaneous sloughs. The nares presented lesions eminently characteristic of the disease in the only four cases in which they were examined; these were ecchymoses and gangrene of the pituitary membrane, minute abscesses, ulcerations of the septum, granulations in the frontal sinuses, or thickening and infiltration of the pituitary membrane, which was studded with ulcerated "tubercles." The larynx, rarely examined, is however noted as affected with a peculiar eruption in the author's own case, with ulceration or inflammation in others. Of the condition of the trachea little is known. In the fourteen fatal cases, the lungs were not examined four times; in three instances, they were declared healthy. In the remainder, lobular pneumonia, pleuro-pneumonia, "vomica," pleural tubercles with abscess, dark-coloured sanguineous engorgement of the pulmonary tissue, existed in various degrees. The blood, occasionally buffed at the outset, was liquid and of slight consistence towards the close or after death. The heart and great vessels were healthy. The lesions described as occurring in the alimentary canal were slight in degree, and by no means constant in appearance; besides, the vague language used in their description renders it impossible to form any correct opinion of their nature. In the subcutaneous and intermuscular cellular membrane were found collections of white gelatinous pus, infiltrated serum, purulent depositions under the periosteum of the cranial and shoulder bones.

The differential diagnosis of the affection has evidently been studied with much care by M. Rayer; but the importance of his results does not justify the extreme diffuseness in which he indulges. They amount in the main to this: that acute glanders may be distinguished from the results of dissection-wounds, of absorption of pus after capital operations, from ordinary phlebitis, angeioleucitis, malignant pustule, carbuncular affections, putrid variola, &c., by the peculiar nasal discharge, pustular eruption, and the property the secreted pus possesses of producing the specific disease in sound animals. The nice points of distinction brought into notice by the author are, however, we admit, worthy of study.

An elaborate description of the disease as observed in the horse, and founded on M. Rayer's own researches, is next given, in proof of its identity with the human malady. We shall allow the curiosity of our readers to seek this in the original, and pass at once to the remarks on treatment, which, indeed, contain little worthy of extraction.

If, when first called in, the medical attendant find the lymphatic vessels and glands inflamed, the author recommends the immediate excision of the swollen glands; and, if the local inflammation following this operation, or resulting from the inoculation alone, were intense, "débridement" and mercurial frictions might, he presumes, be employed with advantage. He is decidedly hostile to general or topical blood-letting, notwithstanding the buffy condition of the blood in the early stage: if the former be employed, prostration and stupor rapidly supervene; if the latter, local gangrene. The use of bark, serpentaria, and other tonic antiseptics, is declared to be equally fruitless. The treatment to which he gives the preference consists in repeated purgation and the exhibition of large doses of acetate of ammonia; a favorite remedy with veterinary surgeons. Whatever be the number of abscesses formed, they should be incised, and the cutaneous pustules and bullæ opened and cauterized; while the patient's strength is supported by "tonic drinks and generous wine diluted with a gaseous water."

There seems, from these statements, to be but a sorry chance of devising, by the aid of experience hitherto acquired, any feasible plan of arresting the progress of this frightful disease. In the cases collected by M. Rayer, especially the German ones, every variety of rational treatment appears to have been employed with vigour and discernment, yet those in charge of the patients had not the satisfaction of indisputably checking for a moment the violence of the symptoms or the rapid course of the malady. If such be the truth, (and the cases themselves prove it to be so,) the fair inference therefrom seems to be, that the wisest plan, —at least as regards *general*, in contradistinction to *local* treatment,—would be to trust mainly to the powers of nature. The remarkable form of disease from which Professor Roux recovered under treatment almost purely expectant, (a few applications of leeches being the only active means employed,) bearing, as it did, a very strong analogy to glanders in many of its characters, seems to justify a trial of the mildest therapeutic measures in the latter disease, in preference to a repetition of the heroic remedies that have hitherto so signally failed.

In his account of *button farcy*, M. Rayer agrees with the majority of those who have written on the subject, in viewing that form of affection as produced by the same contagion as glanders, and differing solely from this in the situation of the affected parts: and, as he very justly remarks, some of the fifteen cases that follow, though quoted as examples of simple farcy, may, in reality, have been instances of farcy glanders; for, though no nasal discharge was noted during life, yet (inasmuch as the nasal fossæ were not examined after death) it cannot be affirmed that they were free from glanderous eruption. Exposure to contagion is pretty evidently made out in these cases. In four of them the disease was evidently inoculated; in the others, the mode in which the contagion acted was not ascertained.

When the affection results from inoculation, the first symptoms are those of angeioleucitis, attended by diffuse cellular inflammation. If the spot where the morbid matter was introduced suppurate, a true pustule is occasionally formed, succeeded by a foul ulcer. The inflammation of the superficial lymphatics is sometimes sufficiently great to produce real farcy cord. These local derangements are accompanied by fever, anorexia, nausea, shivering, &c. So far, however, the occurrence cannot be distinguished by its effects from an ordinary dissection-wound; their cause alone distinguishes them. If the glanderous poison enter no further into the system, recovery frequently takes place. If otherwise, the characteristic pustulo-gangrenous eruption and glandular abscesses supervene, announce a general infection of the blood, and are, with rare exceptions, followed by death. The specific eruption varies as to the time of its appearance, from the fourth day to the third or fourth week after inoculation. The eruption is preceded or accompanied by copious sweats, and occasionally by gangrene of the cheeks or other parts of the body. Add to this the appearance on the limbs, and elsewhere, of soft doughy tumours, small and slightly prominent, usually almost indolent, and very generally ending in suppuration or gangrene. These tumours, however, possess no character distinguishing them from those of ordinary angeioleucitis. Death, which almost always takes place if there be glanderous eruption, supervened from the thirteenth to the nineteenth day, preceded by prostration, delirium, involuntary fetid stools, &c.

But, as the reader will probably enquire, if the non-eruptive form of the disease be completely similar in its symptoms to the affection resulting from ordinary dissecting wounds, can it fairly be ascribed to a specific farcy infection? In the absence of the only information which would justify a positive conclusion,—namely, the results of inoculation of the horse or ass with the pus secreted by the human subject,—we confess ourselves little inclined for a discussion of the question. M. Rayer, in adopting the affirmative, appears to have lost sight of the prudent reserve that guides him in the majority of his conclusions. It may be, and we believe it is, true that "circumscribed angeioleucitis and successive formation of abscesses have been more frequently observed to follow anatomical wounds in veterinary than in medical practitioners;" but we should be sorry to found our opinions on evidence so indirect and unsatisfactory. If the non-eruptive disease be really a variety of farcy, it at least differs materially from the acknowledged form of the affection, as it does in the mortality it occasions. Of five cases without eruption, four terminated favorably, after one or more months' suffering, it is true; while nine of the eleven subjects in whom the eruption existed succumbed. Our author's paragraphs on the appearances after death are of necessity meager in detail, from the careless and incomplete manner in which the examinations were made: those on diagnosis are a repetition of facts and opinions already noticed: those on treatment exhibit nothing novel.

A lengthy chapter follows containing reports of "cases of doubtful nature." There are valuable materials in these for substantiating the diagnosis of analogous affections. M. Roux's account of the malady he contracted from a wound of the left index finger, while deeply inserted into the rectum of a patient on whom he was operating for fistula in ano, will, in spite of its egotistical pompousness, be read with interest. It clearly shows that there is nothing *specific* in the tumours following the inoculation of farcy; for nine similar tumours appeared during the progress of this disease. The symptoms form a suitable illustration of Amussat's correctness in attributing the fatal termination of the operation for imperforate anus, performed in the ordinary way, to absorption of fæces by the sides of the incision.

The *chronic* form of glanders next occupies our author's attention. Previously to reporting the cases published by Dr. Elliotson and Mr. Hardwicke, (the only examples of the affection in the human subject hitherto recorded,) he enters into a minute detail of the symptoms and lesions characteristic of the disease in the horse. This chapter will especially repay the trouble of perusal of those who take an interest in comparative pathology. It manifestly proves that chronic glanders is not, as Dupuy and others have maintained, a tuberculous affection. A certain degree of doubt unquestionably hangs over these cases, as respects the really glanderous nature of the disease. It is true the general character of the phenomena observed during life, as well as the ascertained appearances on dissection, argue in favour of its specific character; but, while ignorant of the anatomical state of the nares, we want the materials for coming to a positive conclusion. Again, of the seven cases drawn from French and German sources, and adduced as examples of *chronic farcy*, we confess ourselves unable to select one, the perusal of which carries with it conviction of its fair claim to such title. M. Rayer's argumentation in their behalf is acute and well sustained; but he at last admits the doubtfulness of their nature, and esteems them chiefly valuable (and herein we heartily concur with him,) as furnishing materials which may aid future research.

On the whole, we consider this essay a valuable production: it contains a clear, full, and impartial analysis of previous writings on the subject; furnishes us with a minute description of the structure of the bullæ and pustules; together with the results of microscopic examination of the various secreted fluids, and details of the only perfect autopsy hitherto published. The author argues with judgment on the question of transmission by inoculation, by contact, and by infection through the medium of the air; and gives us probably the best description that exists of the glanderous lesions in the horse. M. Rayer is the only writer on human glanders who appears to have fitted himself for its study by a diligent investigation of the disease in the solipeda; and, we think, he establishes the differential diagnosis of the disease, as it occurs in man, with greater accuracy than had hitherto been attained.

The treatise is illustrated by two well-executed quarto plates, containing sixteen coloured figures, representing the various morbid appearances produced by acute glanders in the human subject.

## ART. IX.

*Report of the Proceedings under a Brieve of Idiotry*, (Peter Duncan *against* David Yoolow,) *tried at Coupar-Angus*, 28-30 *January*, 1837; *with an Appendix of relative Documents, and Introduction*. By LUDOVIC COLQUHOUN, Esq., Advocate.—*Edinburgh*, 1837. 8vo. pp. 116.

THE subject of this essay is of such importance to medical jurists, and is so ably treated by the author, that we deem it right to lose no time in laying an account of it before our readers. It is by monographs of this description that more real good is conferred on the science of medical jurisprudence than by the publication of the most voluminous treatises. The author is evidently master of his subject; and he treats the evidence given on both sides of this interesting case with such candour and liberality that it is impossible for either party to take offence from his remarks: but there is something more than this to recommend his introductory observations to the notice of our readers. They form not merely an introduction to the case reported, but they furnish a clear and concise history of the medico-legal relations of insanity, and of the relative value of general and medical evidence on these occasions. The witness, liable to be summoned on a commission of lunacy, will here discover the true bounds within which he should confine himself in delivering evidence, and the circumstances which will give to it the moral and legal force that every conscientious practitioner must desire it to possess. The author remarks, with great truth, in speaking of medical evidence—

> "In many cases, its operation is most salutary; in some, though productive of loss of time and probably of patience, harmless; while in others its effect is positively mischievous; and instead of throwing light on the point to be ascertained, it envelops a clear case in doubt, and plunges a doubtful one into still deeper obscurity." (*Introduction*, p. xxxv.)

Most medical witnesses on these occasions are apt to consider that it rests with them to say whether a party against whom a brieve or commission is issued be insane or not. They thereby usurp the province of a jury; and, instead of expressing an *opinion* founded on facts, to be sifted and examined by the rules of common sense, they utter a *dictum*, which, in their estimation, ought to weigh down the evidence of ordinary witnesses, and even the observations made by the jury themselves on the mental condition of the supposed lunatic or idiot.

Let us, however, consider what kind of evidence is required of a medical witness examined on a commission. His evidence relates partly to matter of fact, and partly to matter of opinion: he states to the jury such facts relative to the question at issue as have fallen under his notice, or been elicited by the tests to which he has subjected the party. He states also the opinion which he deduces from these facts, and the reasons for that opinion. At the same time, the essential point for the consideration of the jury is not the opinion which the witness may have formed, but the facts from which it has been derived. But, although the verdict of a jury ought ultimately to be founded on their own inference from the facts proved in the case, the opinion of an intelligent and experienced medical witness is unquestionably well worthy their serious attention, as an aid to the formation of that inference; and therefore, in receiving the facts which he attests, the jury ought to consider the conclusion which he draws from them, and the reasons he assigns for having come to that conclusion. Should these reasons appear not to warrant his conclusion, while the jury reject the conclusion they still have the facts from which a more legitimate inference may be deduced by themselves. However eminent in his profession a witness may be, or however extensive his experience in relation to the subject of insanity, a jury ought in no case to adopt his opinion, unless the grounds of it are completely satisfactory to their own minds. Where these grounds are unsatisfactory, or where medical witnesses come to different conclusions on facts essentially the same, the jury must throw aside the opinions which they deem inconclusive, and form their own opinion from the facts actually established in evidence, as well as from their inspection and examination of the party.

With these prefatory remarks on the value of medical evidence, we shall extract from Mr. Colquhoun's Report a short account of the case of the individual who was the subject of this "brieve of idiotry." A brieve of fatuity or idiocy, according to the law of Scotland, relates to those persons only in whom there is a *total* incapacity of mind. The issue which the jury have to try is, whether the party be "*incompos mentis, fatuus et naturaliter idiota*," and not to be trusted regarding the alienation of his property. The issue must be considered and answered by them "*in terminis;*" and, therefore, unless satisfied that the evidence amounts to proof of actual idiocy, they are not entitled to cognosce, i.e. to find the party an idiot.

From the general evidence offered, it appears that David Yoolow was, at the time of the commission, about fifty-four years of age,—that, from the age of ten, he had been affected with paralysis, which had rendered him a cripple and confined him during the greater part of his life (forty years) to the house. From the death of his father, he had been chiefly attended to by a sister, who died about six months before the issuing of the brieve. During the lifetime of his father, who was a farmer, he had taken no part in the management of the farm, and his farming affairs were entirely superintended by his sister after his father's death. In consequence of the paralytic attack, he had been removed from school during his boyhood, and no further instruction was attempted to be given to him. His mind was, therefore, in a most uncultivated state. His father and sister appear to have regarded him as incapable of attending to matters of property; for, at their deaths, they vested the property in trustees for his benefit, and did not directly bequeath it to him. After the death of his sister, the next of kin, the pursuer in this case, Peter Duncan, suspecting that, from the weakness and imbecility of David Yoolow, he would be liable to be imposed on in the disposition of his property by designing persons, sued out this brieve, in order to have the actual state of his mind legally investigated. We need hardly remark further, that, if the party were cognosced, i.e. found to be an idiot, he would cease to have any person in law. He would be held to have been incapable of acting or of consenting to any act since the time at which his mental infirmity was declared to have commenced; and any act or deed done subsequently to that time, and before cognition, or which he might do after cognition, would be *ipso jure* null. The administration of his property and the care of his person would devolve thereafter upon a curator appointed by the Court; generally the next of kin. This curator or committee, as he is called in English law, is legally responsible for the conscientious exercise of the trust reposed in him.

The medical evidence as to the state of this person's mind was of a very conflicting nature. Several physicians appeared in support of the unsoundness of mind, among others Dr. Christison and Dr. Malcolm. Both of them argued that Yoolow was a man of unsound mind, unable to manage his affairs, and labouring under a great degree of imbecility. Five other medical witnesses deposed to the same effect, in language nearly similar. Not one, however, contended that there was absolute, but only partial, derangement of intellect; all were unanimous in considering him to be incapable of transacting business. The counsel for the claimant admitted, in his opening speech, that Yoolow was not absolutely bereft of reason; and, indeed, had not the admission been made, this would have been clearly established by the evidence adduced. The facts upon which these medical opinions were founded were closely investigated. Each witness was required to state the questions put by him to the party. For these and the answers we must refer to the work itself: we shall only observe, that many of the questions were answered with such shrewdness and sagacity as to leave the impression that the party answering them was scarcely a fit subject for the enquiry. Some of the questions were eminently absurd, and we may be well surprised how a sensible individual could have put them under the circumstances. Thus, Yoolow having stated that he had 1200*l.* in the bank, and that he received 20*l.* for interest, Mr. Symmons asked him how much that was the hundred (i. e. per cent.)? (p. 13); Yoolow said "he could not tell, and that he was no good hand at arithmetic." As his counsel, Mr. D. M'Neill afterwards remarked, (p. 80,) this "is a question not so easily answered even by some of those who by courtesy are termed learned." Indeed, the counsel actually put this question to one of the claimant's medical witnesses, in cross examination, (p. 24,) and he acknowledged that he was unable to answer it. What better or more sarcastic commentary could be offered on the absurdity of putting such a question to an alleged idiot!

Several non-medical witnesses were examined, and their evidence went to show that Yoolow was a person easily swayed or controlled by those about him; that he was of weak mind, guilty of many silly and unreasonable acts; that he had no shame or sense of propriety in regard to calls of nature before the servants, male or female, or before his sister, (p. 21;) that he required a servant to take care of him; and that he was accustomed to be humoured like a child.

On the other side, several medical witnesses stated that Yoolow was neither an idiot, fatuous, nor *non compos mentis:* they found nothing about him, from their observations, to indicate insanity or idiocy. All seemed to think that his attainments were limited, from want of experience and instruction only. Dr. Nimmo considered him to be a perfectly sane man,—a man of sound mind,—by no means an idiot or fatuous. (p. 35.) Mr. Lowe thought that he was of *sufficient* degree of soundness of mind to give directions to others to act for him; and that his being unable to act for himself was owing to decrepitude of body. (p. 42.) So far as we have examined the evidence, we cannot find that any of the witnesses on this side of the question, with the exception perhaps of Mr. Lowe, explicitly admitted that the defender was possessed of sufficient mental capacity to direct and manage his own affairs. They leave this to be inferred from their statements, that "he was not an *idiot*, but *quite the reverse*." (p. 30.) In a written opinion, signed by Dr. Nimmo and Mr. Bell, it is stated that Yoolow "possesses a strong though uncultivated mind; and is therefore perfectly competent to select proper persons to manage his affairs or to bequeath his property." (p. 33.)

A few respectable non-medical witnesses were examined; among others a clergyman, who found Yoolow well informed in the Scriptures, and able to return satisfactory answers when questioned on the principles of the Christian religion. So far from showing any trace of imbecility or weakness of intellect, this witness considered that the answers given to his questions "evidenced an average degree of information and intelligence." (p. 29.) Other witnesses deposed that, so far as their observation went, they thought him a person of sound mind: they saw nothing wrong about him. (p. 41.)

The jury having visited Mr. Yoolow, a number of questions were put to him in their presence, the answers to which were very correct and proper, (p. 109;) and the sheriff remarked, in summing up, that, from the ready and unembarrassed mode in which these answers were given, he felt satisfied at the time that the character of idiocy did not apply to him. After having stated the law on the subject, and represented to the jury that idiocy applies only to those who are entirely deprived of the faculty of reason, and in whom there is a *total* incapacity, he proceeds to observe that, if the incapacity proved be only *partial*, the brieve of idiotry would be inapplicable to this case, and another and different remedy, provided by the law, must be sought for. The medical witnesses, he observes, differ widely among themselves as to the general conclusions at which they had arrived on the state of Yoolow's powers of mind. "This is somewhat difficult to account for, and is very apt to lead us to believe that so great a difference of opinion cannot arise out of their evidence as *medical* men; on the contrary, that it is a case in which non-medical men, and all persons of common sense, may be equally capable of judging." (p. 112.) The jury have to consider, whether the whole of the medical evidence in favour of the brieve amounts to a proof of fatuity or idiocy against Yoolow, or only to a partial derangement of the intellect, to which the brieve of fatuity and idiocy does not apply. It is for them to judge whether some of the questions put by the medical witnesses were or were not applicable to the party's limited state of knowledge and acquirements, and to deficiency of education.

The jury unanimously returned a verdict against the brieve, finding the prisoner not idiotic.

In commenting on this case, we must first of all separate the idea of bodily infirmity from mental incapacity. All agreed as to the state of disease or helplessness of the defender; but because an individual is helpless or bed-ridden, this is no ground for finding him *incompos* and incapable of directing or managing his own affairs. The evidence of mental incapacity in Yoolow's case does not therefore rest upon the fact of his labouring under corporeal infirmity, although it is well known that, from a severe attack of paralysis, the mind, if already developed, is apt to suffer; or, if undeveloped, is apt to have its capacity for acquiring knowledge either enfeebled or destroyed. The questions which it appears to us this case involves are simply: What was the actual state of this individual's mind? and what did the law require to establish the brieve? We shall separate the medical conclusions from the premises on which they were founded; and we do not hesitate to declare our opinion, from the *whole* of the evidence adduced, that the defender was a person of weak mind, labouring, in the language of Dr. Christison, under a *great degree of imbecility;* and that he was unable to manage and direct his affairs with that ordinary discretion necessary to protect an individual from the designs of artful persons. We, of course, consider all the witnesses to be worthy of belief. On the affirmative side, it was shown that Yoolow had been confined to his house for a period of forty years; that he required persons to look after him; that his habits before servants, and even his sister, were indecent; that he was under the control of servants; that he had no settled pursuit or occupation, if we except the reading of the Scriptures; that, although possessed of a farm, he had not undertaken any bargain or sale, and had intrusted its management to one who appears to have treated him like a child. From the medical evidence, he seems to have been easily excited and rendered irritable by trivial causes; and, judging more from his general conduct than from the answers returned to the questions put at these examinations, we have no hesitation in saying that we think there are strong proofs of his being a weak-minded and imbecile man. That he was wholly unfit to manage or direct affairs of property may perhaps be better derived from the conduct of those who must have known him well from birth, than from the observations of others whose opinions were founded on occasional visits and on the answers returned to their questions. In his father's settlement of property, "he is described as a person of weak mind, and, on that account, provided for merely as a helpless child, without any right of management or interference. The leases of the father, accordingly, are left entirely past Yoolow, and made descendible to the daughter. This is the expressed opinion of the father in 1805 and 1810; and, in 1834, Agnes Yoolow, the sister, takes the same course, and leaves the leases to trustees, under the obligation of supporting her brother." (p. 50.)

We do not find any satisfactory answers to these points in the defence. The counsel, Mr. D. M'Neill, devotes the greater part of a most able speech to a critical analysis of the medical evidence for the claimant; but he says little on the evidence of the non-medical witnesses, passing over the points to which we have adverted as eccentric habits, and justifying and excusing them by a reference to eccentricities and peculiar pursuits adopted or taken up by certain individuals. It is quite possible that some parts of the defender's conduct may be ascribed to eccentricity; but it seems to us equally certain that other parts can only be set down to mental incapacity. It would, perhaps, require a nice judgment to draw the line between an act depending on eccentricity and one depending on mental incapacity, if the signification of the word is to be thus extended; but the true question really is, whether, what are thus called eccentric acts are to be tolerated in an individual, when the result may be injury to his own civil rights, or the rights of his family. Evidence, it is true, was adduced on the defender's side, which went to show that some persons "had known nothing wrong of him;" but this negative evidence amounts to very little when we consider that two of the witnesses spoke of the period of his boyhood, forty years before, and previous to the paralytic attack; while the other two, one of whom had certainly known him a long period, only met and conversed with him occasionally. It is not probable that such individuals were likely to observe those parts of his general conduct which could not escape the notice of others to whose care he was immediately intrusted. But the conduct of Yoolow's father and sister towards him appears to us a sufficient answer to evidence of this description. The defender's counsel refers this conduct to the *personal decrepitude* of Yoolow, and the *secluded life* he had led, (p. 72;) but, setting aside the expressed opinion of his father on the state of his son's *mind*, we cannot admit that these circumstances were sufficient to justify the total transference of property to trustees for his benefit, without allowing him the least right of management or interference. This, be it remembered, applies to a man at that time twenty-seven years of age; and, in 1834, the same course is followed by a sister towards a brother fifty-two years of age! We cannot consider that such a proceeding would have been adopted by these relatives towards one whose mind was, as described by some of the witnesses, active and *strong*, and who was capable of directing or managing his own affairs. We think this circumstance corroborates the views of those who appeared on the affirmative side of this question; and that it affords an answer to those who formed their notions of his having a directing and disposing mind from a few cursory examinations.

Having thus expressed our opinion as to the actual mental state of the individual, we have, lastly, to examine—What did the law require to establish the brieve, or what proof of mental incapacity or imbecility was demanded in order that this person should be deprived of the management of his property? Besides the issuing of a brieve of idiotry or insanity, there are other processes in the law of Scotland by which the property of a person, labouring under a *partial* incapacity may be protected. It is not our purpose to refer to these remedies: it may be sufficient to state that the interposition of the law is in them of a more lenient character, while the issuing of a brieve of idiotry or insanity is considered to be an extreme measure. Idiocy, it has been already remarked, implies, in the law of Scotland, a *total* defect of judgment, and an entire deprivation of the faculty of reason. As such, it could not be said that the subject of this brieve was an idiot. His mental incapacity was proved to be only *partial*, and he undoubtedly had, to a certain extent, the power of reason. Indeed, not one of the medical witnesses on the claimant's side asserted that this unfortunate being laboured under idiocy in its strict legal sense. Dr. Malcolm expressly stated that he was not an idiot. While, therefore, we allow, with Mr. Colquhoun, that Yoolow was not a fit subject for cognition under a brieve of idiotry, we perfectly agree with the witnesses for the claimant, that he was imbecile and a man of very weak intellect. Under these circumstances, we cannot be surprised at the verdict of the jury: their duty was of a very simple nature; they had merely to consider whether the mental incapacity of Yoolow was *partial* or *total*, and upon this point we think there cannot be a reasonable doubt.

We cannot take our leave of Mr. Colquhoun without again complimenting him on the manner in which he has executed a somewhat difficult task. We wish that his professional brethren would more frequently follow his example. There can be no want of materials; and, judging by the medico-legal knowledge displayed in this volume, we should say they would be thereby conferring a great benefit on the members of the two learned professions, and rendering an important service to society at large.

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## ART. X.

1. *Traité de Diagnostic et de Séméiologie.* Par P. A. PIORRY, M.D. &c. *Treatise on Diagnosis and Semeiology.* By P. A. PIORRY, M.D. &c.—*Paris*, 1837-8. Three Vols. 8vo. pp. 611, 548, 651.

2. *Grundriss der Speciellen Semiotik.* Nach den Quellen bearbeitet, von Dr. H. E. SUCKOW, Kreisphysicus in Jauer.—*Jena*, 1838. *Outlines of Special Semeiology.* By Dr. H. E. SUCKOW.—*Jena*, 1838. 4to. pp. 296.

THE first of the above works, the only one deserving special notice in this place, professes to be practical, and to be founded upon the physiological or physico-chemical laws of the economy. It is appropriately dedicated to MM. Bally and Orfila: to the former, for having devoted the experience acquired at the bedside to the improvement of therapeutics, as well as because a great number of the facts collected were observed in his service at the Hôtel Dieu; and to the latter, for his application of physical and physiological laws to the history of the effects of poisons and medicines: and, consistently with such a dedication, the author claims for his work no other merit than that it consists of a collection of facts verified by himself.

The commendation, however, that he disclaims for himself, he loses no opportunity of invoking upon his *organo-pathological nomenclature*, to which he devotes an introduction of twenty-seven pages. The following table of the new terms, anglicised, will give our readers a better idea of this nomenclature than any lengthened description of it.

| Names of certain Organs. | | | | | |
|---|---|---|---|---|---|
| English. | Greek. | Changed in Composition. | Termination. | Prefix. | Epithets. |
| | | | -pathic, *affection* | | |
| | | | -hemia, *sanguineous congestion* | hyper-, *increased degree* | Tubular |
| Stomach | Gaster | Gastro- | -itis, *inflammation* | hypo-, *deficiency* | Cancerous |
| Intestine | Enteron | Entero- | -trophy, *volume* | poly-, *multitude* | Hydatic |
| Lung | Pneumon | Pneumo- | -algia, *pain* | a-, *want or absence* | Epidemic |
| Womb | Metron | Metro- | -nervy, *nervous action* | dys-, *difficult* | Endemic |
| Brain | Encephalon | Encephalo- | -rhagia, *flow of blood* | hydro-, *watery* | Intermittent |
| Bone | Osteon | Osteo- | -rhœa, *colourless discharge* | aero-, *gaseous* | Typhoid |
| Joint | Arthron | Arthro- | -artia, *contraction* | toxico-, *poisonous* | Choleric |
| | | | -extasia, *distension* | pyo-, *purulent* | Saturnine |

Thus, an apoplexy is a *Hemo-encephalorrhagia;* a congestion of the brain from plethora is an *Encephalohemia-polyhyperhemica;* and an emphysema is an *Aero-dermo-ectasia!*

Now, before we use our privilege as critics with regard to this nomenclature, let us see on what grounds the author recommends it. The reasons adduced are, first, that "in medicine a nomenclature is indispensable;" though no cause is alleged why it is so, except we take the following, "that it is absurd to be continually employing terms that have no correspondence with the things implied by them." But, granting for a moment the sufficiency of these reasons, are the terms used altogether free from ambiguity? Certainly not; for M. Piorry himself confesses that most of them are arbitrary, and that some have been perverted from their primary and general signification to a secondary and restricted sense: thus, -rhagia and -rhœa, in the original Greek, express different degrees of the same thing, (viz. an impetuous or gentle current,) yet one is used to describe a flow of blood, and the other of white or colourless discharges. Besides making these admissions, M. P., like certain lawyers who have a weak cause to defend, brings forward objections for the purpose of showing how easily he can overthrow them. The first is, the length of the terms. The example, Hyper-spleno-trophy; to which he replies, It is much longer to say, an increase of volume of the spleen. Hence, it conduces to rapidity and conciseness of style to substitute a word for an expression. We will reserve what we have to say with regard to the above example, and pass on to the next objection.

2d. The terms are harsh and inharmonious. Example, Cardiarctia. The reply is, first, These words have been admitted into science; and it is better to admit them, though they are inharmonious, yet are understood, than to coin others more agreeable, but whose meaning nobody can find out without hunting in a dictionary. In this reply, M. P. has supplied his opponents with a most powerful argument against his nomenclature; for we are quite certain that no person, however adorned with classical acquirements, can make out such terms, even with the aid of all the Scapulas and Hedericks in the Bodleian, unless he is acquainted with the organo-pathological nomenclature, from the author's works themselves; particularly when one-half of a term may have a Greek and the other a Latin derivation, as in the example before us. It is, however, clear that M. P. rather prides himself upon this hybrid creation of his organo-pathological imagination; for he asserts that Cardiarctia, in the difficulty of its pronunciation, expresses the obstacle to the flow of blood through the contracted heart! We think that he might have gone even further in his onomatopœa; for we cannot utter the word without eliciting a *bruit de scie* from our lips.

3d. It is assuming the province of scientific bodies. Reply, Great bodies move slowly, and the nomenclature is not the original work of one person, but a collection of terms, most of which have already been employed.

4. It creates new words. Reply, No: it is a combination of terms already in use.

5th. What is the use of changing the terms? Why not retain the adopted language, when everybody understands its meaning? Reply, The adopted terms are not understood: for an apoplexy with one is a congestion, with another a hemorrhage. Fever is an entity that everybody speaks of, but nobody understands. The only way to extricate one's self from the labyrinth of skin-diseases is to name each organically; i. e. from the tissue affected. The new nomenclature remedies these abuses: it does not give a definite name to the disease, but it clearly expresses the state of the organs, in the opinion of the person who employs it; but, if another be of a different opinion, he is enabled by it to express himself in adequate terms. Thus, if there be a question respecting a gastric pain, one may call it a Gastritis, another a Gastralgia, and a third a Gastrohemia; and, if the case be obscure, they may all concur in terming it a Gastropathy.

Here, again, we think M. Piorry unfortunate in his choice of arguments; for, if the affection be thus obscure that it cannot be decided whether it be an inflammation, a neuralgia, or a congestion, it appears to us that the original expression, "gastric pain," is quite as clear as gastropathy: and, with regard to diseases of the skin, as we shall see hereafter, the application of the new nomenclature is a signal failure; for it not only admits of the old arrangements into papular, pustular, &c., but, by classing them all as Dermites, or inflammations of the skin, considered as a whole, it imposes a character upon them that is far from correct in all instances; for it is evident that the same tissues cannot be affected in a pustular or papular disease as in the squamous or the exanthemata.

As to the ambiguity of the term apoplexy, no one, we conceive, in using it, does more than apply a name highly expressive of the character of a set of symptoms, that are better understood by this appellation than if he were to refer them to their organic cause, whether congestion, rupture of a blood-vessel, or a softening of the brain.

The great fear of our author seems to be, lest, by giving determinate names to diseases, medical men should fall into the error of expecting to discover specific modes of treatment for them, and thus neglect the consideration of the pathological states on which they depend. On this objection to the old terms, with the present thirst for knowledge and spirit of enquiry that distinguishes medical men, we do not think it necessary to bestow any consideration.

We shall not dwell upon two other rather weak objections to the nomenclature,—viz. that it is difficult to acquire, and is not likely to become popular; because, if it be worth anything, it is worth learning; and, though it may not become familiar, men of science will take pains to understand it. But there are still two more, and in our eyes very formidable, objections, which M. P. has failed in overcoming, and which, in reading recent French medical works, are a great source of annoyance; viz. that every scribbler, now-a-days, takes the liberty of enveloping his crude conceptions in a shroud of long-winded terms, without, in some cases, giving any explanation of what he intends by them; pretty much in the same barbarous style that a corncutter terms himself a Chiropodist and Corn-exantlator; and that another Worthy of the same stamp, terms a washball by the sonorous name of Rypophagon. The other objection is still more serious; namely, that in many cases the new nomenclature imperfectly describes the disease, or cannot describe it at all in the present state of our knowledge, or rather ignorance, of the subject. To this M. P. replies, that we may either use the old terms, or may each of us apply such a name as we think most closely coincides with our views on this subject. In the former alternative we fully agree with the author, but the latter is open to all the objections expressed above.

After stating our objections to M. Piorry's nomenclature at such length, our conclusion may appear strange, but we give it with perfect good faith; viz. that we consider it, with one or two exceptions, etymologically well-formed for its purpose, and in many cases very expressive: moreover, we shall not hesitate to avail ourselves of it whenever we find that the old and adopted terms are inadequate to express our meaning so fully and so concisely. But, to repeat our objections briefly, we protest against the use of definite names for undetermined diseases, and we dislike that established and well-understood terms should be changed merely because they are not of themselves a definition of the thing they represent. As an instance of the former impropriety, we cite Hypersplenotrophy, as indicative of the state of the spleen in ague; which is not an hypertrophy, but only an enlargement or distension: and of the latter, the disuse of the word rheumatism, because it implies an erroneous view of the nature of the disease.

After the introductory chapter upon his Organopathological Nomenclature, our author enters upon his work with two short sections upon Diagnosis and Semeiology in general, and upon the mode of examining a patient; from which we shall only extract one most important yet rather neglected hint,—viz. that a judicious method of examination gives the patient the highest confidence in his physician: to which we would beg leave to add, that, if the patient do not object to them, the physician cannot be too minute in his enquiries, if he wishes to attain the same end. He then enters fully into the causes, symptoms, and physical signs of disease, in reference to the diagnosis, which he further considers specially and comparatively. He next points out the necessity of keeping in view the effects of treatment as a source of correct diagnosis; defends the moderns from the charge of neglect of prognosis, which he asserts they draw more justly from the exact state of the affected organ than from physiological or pathological symptoms, as the ancients did; and concludes this part of his work with a table of the questions to be put, and of the routine to be followed, in examining a patient. With respect to the imputation of neglecting prognosis, we must say that we think our Gallic brethren are rather open to it; and we found our opinion not only on our own observation in French hospitals, but upon the numerous cases published in the different journals, of patients bled, day after day, till within a few hours of their death; nay, even up to the last moment.

The first volume is devoted to the diseases of the organs of Circulation and Respiration; the second, to those of the Digestive and Generative Organs; and the third, to those of the Skin, the Nervous System, the Muscles, and the Bones. But, before entering upon the special diagnosis of the diseases of any organ, M. Piorry gives an account, not only of every mode by which information of the state of the organ may be acquired, but also of the diagnostic value of the result obtained; so that the special diagnosis is little more than a mere enumeration of certain symptoms and signs, whose relation to the disease has been already proved and established. This plan has the advantage of placing before the student at once every thing that can elucidate the different diseases of each organ; but, if he has already formed his diagnosis, it very much increases the difficulty and trouble of ascertaining its correctness, particularly if many of the symptoms be of a sympathetic character; for these are described with the affections of the organ they implicate, not in connexion with that part with which they sympathise: thus, for the state of the eye in epilepsy, the reader is referred to the section on the eye, to that on the nerves, and to the special diagnosis of the disease itself.

M. Piorry devotes 120 pages to the exploration of the heart and to the special diagnosis of its diseases, in which we have found many points of interest, particularly in the results of examination by percussion.

He attaches but little importance to the motions of the epigastrium, as indicative of adhesion of the pericardium, or of fluid in its cavity; and he omits entirely the rubbing sensation occasionally imparted to the hand under the former circumstance. He claims for percussion not only the power of determining the limits of the heart, but of indicating the position and size of the right and left cavities, and, "approximatively," the thickness of their walls.

M. P. has proved by experiment that the volume of the heart may be greatly increased by congestion of its substance when its cavities are perfectly empty; and it is to the removal of this state by bleeding that we must refer the great diminution in its size frequently consequent upon that operation. In the normal state, the heart is in contact with the ribs over a space of twenty to twenty-four lines in extent; and it usually reaches, covered by the lung, an inch and a half or two inches further to the left, as may be detected by powerful percussion: its normal transverse diameter is therefore from three and a half to four inches. From an examination of 177 cases of various diseases, it appears that those attended with disordered respiration were more or less conducive to an augmentation of the heart's size. The vertical dimension of the heart is generally a little less than the transverse; and the distance of the heart below the clavicle is usually from three to three and a half inches, but this space is often diminished one or two inches by hypertrophy or by pressure of the diaphragm. In the above 177 cases, the dimensions, during life, occasionally exceeded those found after death from the cause above stated (the congestion of the heart), as well as from its distance from the parietes and the convexity of the chest; all of which have the same tendency to increase the extent of the dull sound. At the right of the normal dull space, there is dulness without resistance to a slight extent, corresponding to the extension of the right cavities beyond the left on that side; while to the left we have both dulness and resistance combined, arising from the greater thickness of the left ventricle. In hydropericardium with much fluid, there is a dull sound over a pyramidal space, extending from the upper part of the sternum to the region of the heart. When the fluid is in a less quantity, it is detectible by dulness along the right or left side of the upper part of the sternum, according to the side upon which the patient is lying. It is distinguished from dilatation of the right cavities by extending round the great vessels to either side the sternum; whereas, in dilatation, the dulness is chiefly to the sight of the sternum, or under the right side of that bone.

Although the rules laid down by Laennec and succeeding authors are generally correct with regard to the loudness of the sounds being proportionate to the thinness of the walls of the heart, yet M. P. has frequently found the parietes thickened when the sound was clear, and *vice versâ;* and the tone will frequently vary in the same persons, if subject to palpitations. He attaches but little importance, therefore, to the differences in clearness of tone and to the extent over which the sounds of the heart are audible, the latter being modified by so many circumstances; but he attributes the former to the quantity of blood contained, the sound being clearest when the heart is most empty, yet is contracting with energy, as in palpitations: these points, however, he thinks, require further investigation. The impulse offers the same uncertainties; and is rather an indication of the force of the blow than of the thickness of the walls. In the aged females at Salpetrière very little impulse has been observed, though the organ was often much thickened; and in nervous subjects with small hearts the impulse is frequently very strong. A good illustration is taken of the state of action of the heart in such cases, from the struggles of an hysterical female, in whom there is most powerful muscular energy under excitement combined with general debility. He does not attempt any explanation of the double, and even triple impulse described by M. Bouillaud.

In some cases he has observed only twenty-eight or thirty pulsations in a minute, and in one instance only seventeen. He quotes M. Bouillaud's observation of double and triple sounds being confined to persons whose hearts have their orifices contracted; and states that the distance of the sounds of the heart is no proof of hydropericardium, as it is produced by the overlapping of the lung also, and is noticed in cases of extreme hypertrophy.

Upon the value of the bellows-sounds as a sign of diseased valves, M. Piorry is quite at issue with M. Bouillaud, having frequently heard it when no disease existed, and *vice versâ.* He quotes a letter from M. Dechambre respecting eighty-four patients at Salpetrière: of fifty-eight without disease, the bellows-sound was heard in three; of twenty-six cases of indurations or cartilaginous ossifications of the valves or orifices, the sound was heard occasionally or permanently in only seven; and in some of the cases, where there was no sound, the contraction was greater than where it was heard. Of thirteen other cases of contracted orifices, (seven extensively so, six in a less degree,) the bellows-sound was heard in six of the former and only two of the latter. Various other observations of his own and of M. Louis are cited in support of this fact, which our own experience enables us to confirm. The account of the other abnormal sounds presents no novelties to those who have attended to the writings of Corrigan, Spittal, Stokes, &c.; to whom it is pleasing to see that our author, though a Frenchman, will allow some little merit.

In summing up the diagnostic value of the sounds, he gives a preference to the results of percussion, and offers a judicious caution not to insist too much upon the stethoscopic indications, to the exclusion of the more practical physiological symptoms, by which at last our treatment must be guided.[*]

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[*] M. P. says, with great naïveté, that, when we have established the position and dimensions of the heart by percussion, we must be very careful to mark them out on the body of the patient with lunar caustic!!

From the special diagnosis of diseases of the heart we have extracted the following particulars:

*Congestion of the Heart:* Disturbed circulation and respiration, with extensive dulness on percussion, arising suddenly, and quickly removed by bleeding.

*Carditis:* When a sudden acute pain, particularly if rheumatism be present, is felt exactly where the heart strikes the ribs, and nowhere else, we may suspect carditis. Abscess, softening, &c. cannot be ascertained during life.

*Pericarditis:* In moderate effusion, the dulness is found to be vertical rather than transverse over the heart.

*Atrophy:* No symptoms are given, but they are the reverse of hypertrophy. To avoid error, it is necessary to percuss with force, and to recollect that, where the liver does not advance to the left side, the heart sinks low in the epigastrium.

*Dilatation* of the right auricle produces a dull sound at the upper part of the sternum, and is distinguished from hydropericardium by not extending to the left side.

In the exploration of the aorta, M. Piorry places great reliance upon percussion, which, in case of an aneurism, will not only give a dull sound over the part where the tumour is in contact with the walls of the chest, but also, if pretty forcible, will indicate the whole circumference of the aneurism, whether it be of the ascending or descending aorta: only, in the latter case, percussion must be applied on each side of the spine. This examination requires the greatest caution, lest too great violence should rupture the tumour; and hence, in these cases, the pleximeter is indispensable. In proof of the great accuracy of this mode of exploration, M. P. was enabled, by the difference of the sounds elicited, to define the extent of an aneurism, and of the right auricle in contact with it. In an appendix he states that, by pressing his fingers down beneath the sternum, while the neck was bent to relax the mastoids, he was enabled to feel a dilatation of the aorta, in a case in which he had suspected its existence from the dulness observed over a circular circumscribed portion of the sternum.

After some minute but useful directions respecting the method of feeling the pulse, M. Piorry proceeds to the indications derivable from its various modifications. He does not attempt (perhaps judiciously) to account physiologically for all the variations in its frequency, but contents himself with an enumeration of their occasional causes. We must not omit, however, that he adheres firmly to Broussais's opinion that the quickness of the pulse in fever is produced by the communication of irritation to the ganglionic system, and so to the heart, from the inflamed bowels. Upon the whole, he places no reliance upon the rapidity of the pulse indicating any organic affection, irrespective of other morbid phenomena; but he considers a state of fever to exist when the pulse constantly exceeds its normal number by eight or ten beats. His observations upon the other characters of the pulse are exceedingly just, and well worthy the attention of the student; but he has omitted to mark sufficiently strongly the hemorrhagic pulse, perhaps the most difficult of all to appreciate, and yet the most important to distinguish in practice. In order to prove how little the pulse is to be relied on in forming a diagnosis, he gives a table of 100 cases, taken at random, in which the pulse was accurately noted, and shows that almost every variety of pulse was present under similar circumstances of disease: still, however, a general character was observable in the majority of cases of the same affection. He also makes some most useful remarks upon the effects of change of position of the limbs upon the pulse, and on their diagnostic value; and he questions the accuracy of Dr. Corrigan's conclusion that, when an increase of the force of the pulse at the wrist is induced by elevating the arms, it is a pathognomonic sign of insufficiency of the aortic valves.

M. Piorry has never observed a sense of pain and heat along the vessel in arteritis.

The necessity of attending to the state of the veins in certain obscure affections of the abdominal viscera is inculcated, (§ 656,) particularly where obliteration of the cava is suspected; but we do not find any hint given of a phenomenon we have occasionally noticed in obstruction of the internal veins,—viz. that the blood will pursue a retrograde course in the cutaneous vessels, and thus restore the circulation.

The first of the following observations (§ 711) is highly important; the other (§ 712) is likely to mislead. It is a mistake (says M. P.) to suppose that a lymphatic temperament is one in which the lymphatic vessels are more particularly active: on the contrary, they are weaker than they ought to be, like varicose veins, and, like them, their distension depends in great measure upon debility of the heart and circulating organs. It does not follow, he adds, that, because a child has swelled glands, it is of a scrofulous habit; we often find an obvious cause in a carious tooth or the irritation of teething: nevertheless, the engorgement, once established, is not only continued, but even extends to other parts of the system, in virtue of the law of extension laid down by Bichât. Here we differ from M. P.; for we do not believe that the glandular affection will extend itself and become general, except in persons of a scrofulous constitution.

The following is interesting, as bearing upon the origin of *Tubercle.* In fourteen cases M. P. has observed "greyish granulations" in the buffy coat of the blood. Their colour was well marked at their centre, but it passed insensibly at the edges into that of the surrounding mass; their size varied from that of a poppy-seed to a hemp-seed; and in many cases they could be separated by the point of the scalpel from their matrix, when they were found to be of a dark-grey colour, and soft: hard enough, however, to admit of division by the scalpel. They had no true pus at their centre. Where they did not admit of separation from the buff, they were easily rendered visible at its surface, by tearing it across, perticularly if held up against the light. In one mass of buff seen by Andral, there were above fifteen granulations; and, when examined by M. Donné by a microscope, they were found to be minute clots of solid matter contained in the cells of the buffy coat. In all these cases there were decided chronic inflammations, more particularly of the lungs. In one instance, where the patient was seventy-one, there was purulent urine, followed by pneumonia, obliteration of the brachial arteries, and softening of the brain. These fourteen were all the cases out of 10,000 where the blood was observed with care, and in all the fourteen pus was detected either in the sputa or urine, during life or after death, in some of the organs; and hence we must consider this circumstance as a sign of pus in the blood.

In this section upon the Blood there is a great want of proper arrangement of the subjects; there are frequent repetitions; and the relations of the chemical states of the blood to disease are far too lightly touched upon.

We set our faces most strongly against the doctrine (§ 779, &c.) that the state of the blood indicated by the buffy coat is a constant attendant upon inflammation, or that it is always indicative of its presence. We need not refer to the appéarance of the blood in pregnancy and bronchitis, in illustration of this error; for, in another paragraph (§ 780), he admits that, of seventy-six venesections performed in twenty-nine cases of inflammatory disease (not bronchitis), the blood of sixty only exhibited the buffy coat. To this state of the blood M. P. has applied (improperly, in our opinion, and inconsistently with his own nomenclature,) the name of Hemite, or Hemitis: and we object to this term, because, as he and other pathologists have adopted the termination *-itis* as expressing inflammation of the organ or tissue to which it is attached, others, as well as ourselves, may be led into the error of supposing that Hemitis means that the blood itself is inflamed, which is not the case.

Our readers may form some opinion of the minute attention that M. Piorry pays to his subject, from his devoting twenty pages to the exploration of the nasal fossæ; from the whole of which we are sorry that we can find nothing to transcribe that is worth their attention. In the thirty-eight pages taken up by the larynx and trachea, also, there is little to detain the reviewer or man of experience, though the student will find some useful hints, drawn from various sources, upon the examination of these organs.

Under the head *Palpitation in the Exploration of Bronchial Disease,* M. Piorry remarks upon the burning heat of the skin in certain cases of bronchitis; so that it would appear that this symptom, recently enlarged upon by Dr. Addison, is not confined to inflammation of the substance of the lung. Indeed, although M. P. asserts that the temperature of the body in diseases of the lungs is remarkably disturbed, and that, in almost all cases where the heat is very great there is an increase of action, either general or partial, of the lungs, yet he nowhere gives the slightest hint of its being a pathognomonic sign of pneumonia; which, were it so, such an accurate observer would hardly have failed to notice. The voice, cough, and rhonchus or wheezing, afford considerable assistance in forming a diagnosis of bronchial and pulmonary disease; and, although we cannot altogether agree with M. P. in ascribing a necessary mutual dependence of these sounds, yet we think there is a sufficiently intimate relation between them, particularly when modified by disease of the bronchi or lungs, to deserve more attention than it has hitherto met with, either from writers or lecturers on these subjects.

M. Piorry is very unwilling to refer the absence of the respiratory murmur to any other than organic causes; and hence he thinks the existence of such a disease as nervous asthma very doubtful; and ascribes the physical signs of emphysema of the lung, as given by Laennec, to distension, not rupture, of the vesicles, caused by the obstruction to the return of the air along the minuter ramifications of the bronchi, in consequence of the mucous secretion always present in chronic bronchitis, of which emphysema is a result. In another place, in reference to this subject, he claims precedence of M. Reynaud for the discovery, due in fact to Reisseisen, that, from the want of communication between the bronchi, the function of that portion of the lung to which an obliterated or obstructed bronchus leads must be lost to the economy.

The propositions laid down in the article upon *Percussion*, in the section devoted to the *Exploration of the Lungs*, are highly valuable; and, to use our author's words, "inasmuch as they establish the presence of a solid, liquid, or gaseous body in the lungs, admit of great exactitude;" and we perfectly agree with him, that, to practise percussion as it ought to be done, an accurate acquaintance is required with the anatomy of the thoracic organs, and of their condition and mutual relations both in health and disease. We cannot, however, discuss this subject without remarking that, though in a work on diagnosis it is essential that the student should be put into possession of every fact that may be useful to him in the investigation of disease, yet we think it doubtful whether such extreme nicety of touch is attainable by the majority as is said to be necessary, in some instances quoted, for the ascertaining the exact state of the organ examined. We allude to the assertion in § 1286, that the want of elasticity, as well as the dulness of sound, on percussion, will establish accurately the fact of a *deeply-seated* portion of the lung being indurated. In the correctness of the latter sign we agree, because the sound emitted is derived from all the organs to which the shock of percussion is communicated; but the elasticity depends almost entirely upon the resilience of the ribs and their covering, aided no doubt by the lungs beneath, which, we conceive, would afford an equally elastic substratum, whether their centres were indurated or not. Indeed, in his 18th Proposition, he states that circumscribed indurations of the lung, when small, do not give a different sound from the healthy lung: *a fortiori*, then, a deeply-seated one would be still less detectible by its want of elasticity. Another proof of the extreme delicacy of discrimination said to be attainable by means of percussion is M. P.'s assertion, that Laennec and Andral are both wrong in supposing that crepitation is audible in pneumonia before the resonance of the chest is affected; whereas, he maintains that the resonance is first diminished, which is certainly more conformable to what is observed in external inflammations,—viz. that congestion and engorgement of the tissue, pulmonary or other, precedes effusion, whether into the vesicles and minute bronchi, or into the cellular tissue of external parts. He attributes the presumed error of these excellent pathologists and auscultators to their using their fingers instead of the pleximeter.

The section upon *Auscultation of the Lungs* is meager, and proves that he places far more reliance upon the results of percussion than of auscultation. Great attention is paid to the consideration of the sputa; but the test for pus, proposed by Dr. Young, by refracted light, is omitted.

Upon the whole, after all we have constantly heard of the skill of the French physicians in diagnosis, we must say that we have risen rather disappointed from the perusal of this first part of M. Piorry's work.

Except in the application of percussion,—in which we are bound to acknowledge him to be *facilè princeps*, and which we regret to observe is much neglected in this country, though far more easily learnt to a certain degree, and therefore in one respect more useful than auscultation,—we remark an almost entire unacquaintance with the important recent discoveries in the diagnosis of chest disease that have been published in this country, of all which Dr. Stokes has so carefully availed himself, and to which he has himself so largely contributed in his excellent work on Diseases of the Chest.

In the forty-seven pages devoted to the *Exploration of the Mouth*, there are one or two subjects considered that are deserving attention, although we do not receive our author's conclusions as by any means established. One is, the inflammation of the parotid observed during the course and convalescence of fever, which M. P. says is not a critical phenomenon, but arises from the retention of the saliva in the duct in consequence of the obstruction of its orifice by the mucus of the mouth, which, dried by the passage of the breath, not only plugs up the mouth of the canal, but produces its ulceration, and the consequent inflammation of its whole extent to the gland. In proof, he alleges that, when the orifice is set free, there is first a discharge of pure pus, followed by pus and saliva mixed, and then by a return of the natural secretion, and by the subsidence of the glandular enlargement. We are aware that this fact admits of a different explanation, but we think it a point worth attention on practical grounds. The other subject is the condition of the tongue in various diseases. In 100 cases of typhus with enteritis, the state of the tongue was very variable, although the gastro-enteritic symptoms continued throughout the course of the disease. Bloodletting produced paleness of the organ, and the redness of the point was caused solely by the compression of the sides when the tongue was protruded. It was not red in sixty cases of organic lesions of the stomach and bowels. In 100 cases of pneumonia, or general hyperhæmic states not complicated with gastric symptoms, there was vivid redness of the tongue, followed by paleness after bleeding. From these observations the conclusion is, that the redness of the tongue indicates the state of hæmatosis and of the circulation, rather than the condition of the alimentary canal.

The dryness of the tongue is referred, in all cases, to the evaporation of its secretion by the air respired; and this opinion is attempted to be supported by many arguments, which, however, have failed in convincing us of its truth.* The fur on the tongue and teeth is regarded as merely dried saliva; and all the varieties of colour presented by the secretions of the mouth may, it is asserted, be produced by drying this fluid, or the tartar of the teeth, at a temperature of 100°, more or less rapidly, after it has been for some time exposed to the air. The loss of speech, in severe cases of typhus, is said to be owing merely to the dryness and stiffness of the tongue, and may be instantly relieved by moistening the mouth of the patient. The observations upon the acidity of the saliva do not altogether confirm those of M. Donné: "In the present state of our knowledge, (says M. P.) we cannot ascertain the condition of the stomach or other organs from the acid, neutral, or alkaline state of the saliva; and further researches into this subject are necessary before any decided opinion can be formed." (§ 1484.) The mercurial fœtor, according to M. Piorry, is nothing more than the odour arising from the fur and tartar that the increased secretion of saliva deposits upon the lining membrane of the mouth. This is our own opinion, except that we do not refer the secretion to the saliva alone, but think that it is formed also by the mucous membrane.*

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* We think our author's salivary organs must be peculiarly active, if they have never failed in their duty under the desiccating influence of a hot day, salt food, a slight excess in wine, an extra cigar, or even of a strong moral impression.

We were not aware of the fact stated by M. P., that redness of the fauces is an ordinary symptom of plethora, especially in women at the menstrual period, when it assumes the form of congestion of the tonsils, but is immediately relieved by venesection. This symptom, if a true one, may be useful as a guide to treatment in other affections arising at such periods, when we are often in doubt as to the propriety of depleting measures.

The whole article upon the *Exploration of the Stomach, and the special Diagnosis of its Diseases*, though containing little or nothing but what an attentive consideration would suggest, is exceedingly useful, as bringing together, in a well-arranged form, all the various affections of the organ, and the different symptoms and physical signs that, if not infallible guides, are sufficiently certain to obviate any very egregious errors in the diagnosis.

The following account of the diagnostic powers of percussion, when applied to this organ, however, rather savours of the marvellous.—M. Fabre ascertained by this means that the stomach was contained in a scrotal hernia.—Percussion will determine whether the stomach be full or empty, and consequently its size and extent; the period during which the food remains in it; its contents if liquid, and the extent of tumours developed in it or in connexion with it. M. Piorry even looks forward to percussion being the guide to the operation of gastrotomy, if ever it be performed.

We think the following remark upon *Hemorrhage from the Bowels* (Enterorrhagy) worth noticing:

> "When, in a case of Enterorrhagy, we find that the iliac regions give a dull and fluctuating sound on percussion, we are not to conclude that the blood may not proceed from the rectum. In more than ten cases I have discovered, in piles, or in ulcerations of the rectum above the sphincter, the source of hemorrhagies that have been followed by accumulations of blood in the colon and cæcum. In such cases, therefore, we must examine the rectum most carefully." (§ 1757.)

Taking a review of the *Diseases of the Alimentary Canal*, we cannot forbear to notice that, while describing a set of organs that have the most intimate connexion with, and are acknowledged by all physiologists to be most particularly under the influence of, the nervous system, M. P. should have scarcely hinted at any other than organic and local causes for all the host of symptoms enumerated.

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* We are presented, in § 1491, with a lamentable picture (and we fear it is by no means a solitary instance) of the evils resulting from trusting to theoretic notions instead of observation of disease. It seems that certain persons (disciples of Broussais, we imagine,) attach great importance to a fætor of the breath, termed "gastric," as indicating disease of the stomach; and a lady, who presented this symptom from a quantity of carious teeth, was actually nearly starved to death, in consequence, by her physician, who kept her without food for thirty days!

Under *Exploration of the Liver*, we are presented with an interesting account of the dimensions of that organ in health and disease, as indicated by percussion, and confirmed in many cases by examination after death: but we should have been much better pleased if M. Piorry could have given us some directions for detecting the various chronic diseases, unattended with much change of volume, to which the liver is so liable, and which are far more interesting to the practical physician than the temporary increase of bulk induced by congestion or inflammation, that admits of detection also by other means besides percussion. Not the slightest reference is made to the valuable researches of Mr. Kiernan, throughout the whole chapter; but, as we conceive it will be more interesting to our readers that we should cull M. P.'s excellencies than expose his defects, we will proceed to give the results of his mensuration of the liver in health and disease.

| No. of Cases. | | Towards Axilla. | Towards the Nipple. | In the Epigastrium. | To left of Median Line. |
|---|---|---|---|---|---|
| | *In Health* . | 4 inches | 3 inches | 2½ inches | 2 inches |
| | *In Disease*— | | | | |
| 55 | Typhus . . | 5½ | 4½ | 3 — | 2 |
| 19 | Bronchitis . | 5 7/12 | 5 | 3 — | 1⅓ |
| 24 | Phthisis . . | 5⅓ | 4⅓ | 2⅔ | 1½ |
| 8 | Rheumatism . | 5 7/12 + | 4 7/12 + | 3 | 2½ |
| 24 | Hepatitis . | 7 — | 5½ | 4⅙ | 3⅓ |
| 82 | Ague . . . | 5⅓ — | 4 + | 2¾ + | 11 lines |
| 22 | Pneumonia . | 5¼ | 4⅓ | 3 | 1⅓ |
| 9 | Heart Disease | 6⅔ | 5⅓ | 3⅙ | 1⅓ |

From the above table we may infer, 1st, that, in hepatitis, the vertical diameter of the liver is two inches above the normal dimension, and that the transverse diameter is also increased. 2d. That, in disease of the heart, attended with dyspnœa, the liver is augmented in size. 3d. In bronchitis, frequently attended with obliteration of the bronchi and severe dyspnœa, the dimensions are less than in the former diseases. 4th. In pneumonia, the left side is larger than in bronchitis. 5th. That there is hepatic hypertrophy in rheumatism also. 6th. That, in ague, the liver is less enlarged than in rheumatism, bronchitis, or heart disease.

The presence of an hydatid cyst in the liver is detectible by percussion, to which it returns a sound that is compared to that of a repeater-watch, or a metal-spring cushion when struck with the finger, together with a sensation of striking upon a gelatinous matter. Of thirty-eight cases of hepatic disease, only four presented the symptom of pain in the right shoulder.

The spleen can only be examined by the touch when it is so much enlarged as to extend beyond the edge of the ribs; so that, of 500 cases in which it was hypertrophied, in only one-fifth was M. P. able to detect its extension into the hypochondriac region. Neither is the absolute size of the organ to be ascertained by this means; for, in some subjects, it forms a considerable projection beyond the ribs when only slightly enlarged, in consequence of not rising high under the diaphragm in the normal state. In other cases, on the contrary, it hardly advances beyond the bounds of the chest when its diameter vertically is 6¼ inches. In this uncertainty of the normal extent of the spleen, percussion offers itself as the only mode of examination that admits of accurate results; and M, P. somewhat exultingly points to his discoveries respecting the state of this organ in ague, as a touchstone of the superiority of percussion by the pleximeter over that by the fingers, which fails in giving such accurate results. As his discoveries on this point of pathology are quite original, we think the following sketch of his mode of proceeding to detect the state of the spleen will be acceptable to our readers.

First, the extent of the left lung is traced downwards in a direct line from the axilla, till powerful percussion indicates, by a dull sound in place of the clear pulmonary resonance, the presence of the spleen deeply seated beneath the ribs; next, by the same means, is found the point where the spleen is in contact with the abdominal parietes; lastly, these two points being determined, and also the limits of the heart, lung, liver, and kidney, it becomes easy to circumscribe the extent of the spleen in the other directions, except backwards towards the spine: but the difficulty of tracing the organ in that direction may be considerably lessened if the distension of the stomach and colon by solid, fluid, or gaseous matter is removed previous to the examination.

The healthy proportions of the spleen are as follows: In its vertical diameter it is from 3⅓ inches to 3¾ inches, and, in the transverse, 3 inches. It is situated some inches to the left of the median line, and rarely ever in health projects beyond the edge of the ribs. Its increase of size in disease is usually proportionate in all its dimensions: hence, in the subjoined account, its vertical diameter alone is given. In fifteen cases of pneumonia, it was 4 inches; in thirty-eight of phthisis, 3⅓ inches; in thirty-three of gastro-enteritis, 4⅓ inches: several of these cases were attended with rigors, and the spleen was contracted by the use of quinine. In twenty-three cases of hepatitis, it was 3¾ inches; in 130 agues, 5¾ inches. In most of these cases, its breadth and thickness were equal to the height. These results, and the observation of above 500 cases of ague, have convinced M. Piorry that the spleen is invariably enlarged (hypertrophied) or painful in ague; but that, in other diseases, a great increase in its size is only observed where periodic febrile attacks have occurred; and this has been confirmed by various observers both in France and Algiers.

The correct admeasurement of the spleen has established the following facts with regard to the use of quinine in agues: 1st. It reduces the volume of the spleen not only when it is greatly, but even when moderately, enlarged. 2d. The diminution bears some proportion to the quantity of the medicine taken; and this effect may be produced in half an hour; the urine also becoming very bitter in a very short time. We have ourselves seen, in the wards of M. Bally, a case in which the spleen, projecting two inches beyond the ribs, became hardly perceptible the next day after a large dose of quinine had been taken. The effect produced by quinine upon tertians and quartans is proportionate to the reduction of the spleen; so that the disease is cured simultaneously with restoration of this viscus to its healthy dimensions. On the other hand, though the symptoms be arrested, they will be liable to recur as long as the spleen exceeds its proper size. As the spleen attains its greatest enlargement at an early period of the disease, it proves that it is not the paroxysms that produce the hypertrophy, but rather the enlarged organ that maintains the disease. In the opinion of the author and M. Bally, no other remedy is so certain or energetic in agues as quinine; and its powers are equally remarkable in the ascites that results from long-continued disease of the spleen.

We had marked many passages for extraction connected with this novel and most interesting subject; but, as we find that we should exceed our limits were we to give them all in full, and we have already quoted sufficient to point out the author's views, we beg to refer our readers to the chapter itself upon the Spleen, which will amply repay them for its perusal.

M. Piorry has but little to add to the observations of Drs. Bright, Osborne, &c. upon *Albuminous Urine*: he has however observed a puriform deposit in the urine in several cases where the buffy coat of the blood was granulated, and where pus existed in the substance of the lungs. Another case of pneumonia rapidly recovered after a similar deposition had occurred in the urine, that continued some days. In phthisical subjects, also, who have exhibited no other sign of renal affection, albuminous urine has been noticed. From these and other facts, he concludes that a transient appearance of albumen in the urine does not alone warrant the admission of organic disease of the kidneys.

The third volume opens with the *Exploration and Diagnosis of Diseases of the Skin*. This account extends to 150 pages, of which ninety-five are occupied by the former subject, comprising the indications to be derived from the inspection and examination of the skin, with regard to its colour, temperature, consistence, &c., as well as from the consideration of its physiological relations, and the influence it exerts and receives by its sympathy with other organs in a state of disease. With his usual industry, M. Piorry has examined the temperature of above ninety persons in health and disease, and the following is a summary of the results: Of twenty-one healthy individuals, from seventeen to eighty years of age, the temperature of the axilla and groin (and it was always alike in both) varied from 97°.25 to 106.°25 F.; the difference between the eldest and youngest varied from 2°.25 to 4°.50 F. In six cases of fever (typhoid), the temperature was from 108°.50 to 117°.50 F.; in one of these the blood was 113° F. In fifteen cases of phthisis, from 97°.25 to 114°.12, the hands being always hotter than in health. In eight of pneumonia, from 99°.5 to 113°: in one instance, the hand was 115°.25, and the blood only 113°, (unless this is a misprint.) In fourteen cases of inflammation of the uterus and vagina, the axilla was from 101°.75 to 110°.75, but the vagina from 104° to 117°.50.

In the above abstract we have been careful in converting the degrees of Reaumur, given in the original, into the equivalents of Fahrenheit, as we were struck with the remarkable difference of several of them from the statements of preceding authorities. Thus, in the observations of Dr. Currie, the temperature of fever is stated, at most, at 107°; we ourselves have seen it at 108°; it is stated by Donné at between 103° and 104°.* The only observations with which we are acquainted, that at all approach the high standard recorded by M. Piorry, are given by Dr. Granville, in a paper of Sir E. Home's, in the Philosophical Transactions for 1825; in which the temperature of the uterus, during and after parturition, is mentioned as being, in different cases, 100°, 105°, 108°, 110°, 118°, 120°. We call the attention of our readers to this subject, as we confess we have great doubts of the accuracy of the observations which give so elevated a degree of temperature. If incorrect, the inaccuracy may be readily explained by the well-known differences that exist among thermometers; a difference which is inevitable, unless every thermometer is individually regulated by a standard, which we fear is far from being the general practice of thermometer-makers.

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* British and Foreign Med. Review, Vol. I. p. 247.

Among the various morbid sensations of the skin, symptomatic of other diseases, he mentions one that has been generally overlooked, except in cases of neuralgic disorder; viz. an excessive tenderness of the skin, increased by the slightest touch, together with headach and muscular pains throughout the side opposite to the affected part of the head. This, in many cases, is indicative of a commencing softening of the brain; and the correctness of the diagnosis has been verified by dissection.

In speaking of the relation of the skin and the primæ viæ, M. P. states that his experience is contrary to the prevailing opinion (in France) of the dependence of erysipelas upon disorder of the digestive organs. Of 100 cases, only three were traceable to such an origin, and almost all the rest originated in some external cause.

M. Piorry opens the subject of the *Special Diagnosis of Skin Diseases* by finding fault with all the classifications hitherto used for their divisions and subdivisions, the neglect of the anatomy and physiology of the skin, and disregard of the coexistent states of the viscera, &c. of the system generally; for all which evils he proposes his own arrangement and nomenclature as a panacea eminently practical. We have attentively perused this section, and we consider it the feeblest part of his whole work; for he has not only added nothing to our knowledge, but, by the introduction of his nomenclature, he has left confusion worse confounded. Indeed, it is evident to us that his acquaintance with cutaneous disease is very limited, and derived rather from books than practice. We therefore pass on to the subjects of the *Eye* and the *Ear*, under which we find little to detain us, except a mode of ascertaining the soundness or imperfection of the drum of the ear, that we think worth mentioning: it consists merely in propelling the air along the Eustachian tube into the drum, by forcible expiration while the nostrils and lips are closed, when, if the membrana tympani be imperfect, the air will pass through it with a rushing or squeaking sound, audible to a bystander, and very perceptible by the patient.

About ninety pages (a space commensurate with the importance of the subject) are occupied with the *Exploration of the Brain and its Membranes*, which M. P. has treated in a very systematic manner, avoiding the fault that we have noticed in other parts of his work, of entering fully into the description of the diseases in order to explain the origin of certain of their symptoms. Almost the whole of this section merits close attention from the student, though there is little that is novel in the observations. Under *Mensuration of the Skull*, he remarks the occasional concurrence of atrophy of a portion of the brain with a want of symmetry of the two sides of the cranium.

M. Piorry's definition of *Mania*, or *Delirium*, is the old one of "false judgment from true premises," or "correct conclusion from false premises:" as to its seat, he observes, "it may be symptomatic of a great number of conditions of the brain; but no lesion has been discovered invariably in connexion with it, and, in very many cases, nothing has been found in the dead body to account for the phenomena observed during life." Upon *symptomatic delirium* he is more diffuse in his comments, and attributes typhoid delirium either to the disturbance of the respiration and circulation by the pressure of the tympanitic bowels upon the diaphragm and great vessels; or to the absorption and circulation through the brain of altered and diseased fluids. Here, again, he cannot admit the operation of causes of disease that are inappreciable by the senses: but, not to enter upon a discussion on this *vexata quæstio*, we dismiss the subject with this single remark, that we cannot believe that a symptom dependent upon a lesion of the brain, as some assert, or an alteration of the fluids, can be removed by so transient a cause as a dose of opium, with or without antimony, a glass of wine, or the return of day.

In regard to the coincidence of heart-disease and apoplexy, it is stated that, of twenty cases of the latter, taken at random, eight had hypertrophy, four had asthmatic affections, and several of the others were plethoric or had concretions and ossifications of the arteries.

There are some interesting experiments, with remarks, upon the effects of keeping the head of an animal, exhausted by bleeding, raised or depressed, with some highly useful practical deductions upon their bearing both on the diagnosis and treatment of cerebral affections. The opposite states of coma from congestion, and syncope from anæmia, of the brain, were induced in the animals, according to the position in which the head was maintained.

We think the following *Special Diagnosis of Encephalitis* and *Softening of the Brain* will be acceptable to our readers, as the opinion of an author who appears to have both read and observed carefully everything connected with cerebral diseases, for the express purpose of clearing up their diagnosis; though we admit that it exhibits the little advancement hitherto attained in this branch of enquiry. The experience of every one will immediately suggest how numerous are the blanks to be filled up in order to complete an accurate picture of the varied phases, not only of these two affections, but also of meningitis; of which M. P. acknowledges it is most difficult to trace the characters, from its being inseparable from disease, if not of the whole brain, at least of the parts in contiguity with the membranes. For the sake of comparison, therefore, we give an outline of its symptoms, removed from their proper place in this chapter into juxta-position with those of the two diseases more particularly under consideration.

"Meningitis, or inflammation of the membranes of the brain, is attended with vomiting, headach, and delirium at the outset; soon succeeded by convulsions, and closed with coma, paralysis, and death. But when a patient, before the age when ossifications of the arteries are usually met with, exhibiting no symptoms of disease of the heart or arteries, is seized with a dull and deep-seated pain in one side of the head, followed by sudden jerking convulsions, like those of epilepsy; when the sensibility to pain is most exquisite, which pain is excited by the slightest touch or movement of the opposite side of the body to that side of the head that is painful; when these parts become stiff and contracted, and the pain is increased by extension of the arm; when the mind is rational but obtuse,—we must dread an inflammatory softening of the brain. Again, when an old person, or one worn out or wasted by disease, especially of the organs of respiration and circulation, whose veins are varicose, whose arteries are ossified, and scarcely beat; when to a pain in the head succeeds stiffness of the muscles, with rigidity and contraction; when the convulsions are less strong, distinct, or jerking, the sensibility of the opposite side being preserved, and intelligence still perfect, then there is probably softening of the brain from obliteration of the arteries, or from retarded circulation. This conclusion will be more certain if there be no appreciable physical cause for the symptoms, nor any fever; and will be confirmed if there be any obstacle to the venous circulation in the lungs, heart, or great vessels."

Such are the symptoms of the most striking and best-marked cases, and they present but little difficulty to forming a correct diagnosis; but M. Piorry freely confesses that there are others of an intermediate kind, where it is impossible to decide to which of the two diseases they are to be referred. This exactitude of diagnosis is, however, practically of little moment; for, the softening from want of circulation being necessarily fatal, the treatment should be such as is applicable to the more curable state, the Encephalitis.

With regard to fixing the seat of the various affections of the brain, M. Piorry has nothing new to adduce; but his own experience tends rather to confirm the opinions of Gall, Serres, Foville, &c., that the grey substance is affected in delirium; the corpora striata when the lower limbs, and the optic thalami when the upper are convulsed or paralysed; that, when there is loss of speech, we may expect disease of the anterior lobes, and of the cerebellum when there is much excitement of the generative functions: but he confesses that, on all these points, and various others respecting the localization of cerebral disease, we have nothing approaching to certainty to guide us.

Great stress is laid upon the employment of quinine, in large doses, in nervous and cerebral diseases of an intermitting character, particularly in the remittent fevers of children, to whom it may be given in clysters. Even mania, when paroxysmal, will yield to it, and also the delirium of fever, when there is enlargement of the spleen. In epilepsy, when the attacks are periodic, great success has been obtained by him from the use of quinine.

There are two very instructive examples given of accurate diagnosis,—one of obliteration of the arteries of the brain, the other of cancer of the brain with paralysis of the 7th pair; but our limits will not allow us to transcribe them. The latter disease, and tubercles of the brain or its membranes, present no characteristic symptoms, and are only to be recognized by an attentive consideration of other points connected with the age and constitution of the patient.

This whole article upon the exploration and diagnosis of the brain, and its diseases, is fully as interesting as any in the work; and, though M. P. has added but little to the amount of our knowledge on these subjects, and has made a great omission in merely naming delirium tremens in conjunction with mercurial trembling, yet he has conferred a benefit on science in pointing out the extent of our ignorance, which appears to be deplorably great, both in the physiology and pathology of the cerebral organ.

There are some interesting remarks (but too long for extraction) upon *Spinal Irritation*, and the various symptomatic and sympathetic pains to which it gives rise. We are glad to see that this subject, till lately neglected, is attracting a good deal of attention in France; and we anticipate that, with their extensive opportunities and ardour for research, the French physicians will be able to add considerable information to the little we yet possess upon these minor affections of the spinal cord. At present M. P. terms the motions excited by irritation or inflammation in paralysed limbs automatic and involuntary, and refers them to the power inherent in the spinal cord of transmitting movements, howsoever excited, to the parts supplied with nerves from itself; and adds, that these motions are not more surprising than those excited by strychnine and brucine in limbs similarly affected; which, however, he does not attempt to explain.

With regard to the *Special Diagnosis of the Inflammatory Diseases of the Spinal Cord*, M. P. states his opinion that, even with the closest attention, it is very rare that we can ascertain the nature of the lesion, or even the portion of the spine that it occupies.

M. P. divides *Neuralgias*, or *Disorders of the Nerves*, into two kinds, which he calls ascending and descending, according as they have their source in the extremities or origins of the nerves. The former class he separates into those whose influence ceases on their arrival at the nervous centres, as sciatica and other forms of nervous and muscular pains; and those in which the influence extends to the corresponding sentient portion of the brain or spinal cord, and from thence is continued to the motory part, so as to produce convulsions and spasmodic motions, that vary in respect to the part of the brain affected or to the character of the neuralgia. Under this head are included epilepsy, hysteria, tetanus, and hydrophobia. In other cases a plexus forms the nervous centre, and then the affection is transmitted along the nerves that pass through it in various directions, as occurs in the pain of angina pectoris, which is felt in the arms, and that round the pelvis and down the thighs when the uterus is diseased. Descending neuralgias are instanced by the cramps, pains, and convulsions attending diseases of the brain, or when a tumour forms upon the nerve of the part affected.

M. P. describes angina pectoris under the name of Brachiothoracic Neuralgia, which he localizes in the brachial plexus and its branches, attributing the pain and palpitation of the heart to its striking against the exquisitely tender side; but he does not attempt to explain its being confined to the left side, nor why an attack should be excited by such causes as accelerate the motion of the heart. Respiration, he says, is troubled as soon as the neuralgia extends to the respiratory nerves. He has not discovered, on dissection, any ossification of the coronary arteries, nor any other organic affection of the heart, to which the disease could constantly be referred.

In his theory of *Lumbago*, as in other cases, we think M. P. has fallen into an error rather common to his countrymen, of generalizing from a few facts. In this disease, having observed two instances of lumbar pains in which ecchymosis and separation of the muscular fibres were found after death, and having also noticed that the first attacks of pain have come on after a violent blow or strain, he forthwith concludes that lumbago, pleurodynia, and almost every other muscular pain, arises from the same cause. He even adduces this theory to account for psoas abscess, which he terms an inflammation of the psoas muscle, brought on by rupture of its fibres, and extending to the surrounding cellular tissue, where it forms an abscess that finally affects the bone with caries. We do not deny that this disease may originate occasionally in this way; but we think its marked connexion with a strumous constitution ought not to have been passed over in silence.

M. P. divides *Rheumatism* into two forms,—Hæmoarthritis and Arthrohæmitis, according as the local disease precedes or follows the appearance of the general fever or inflammation of the blood; but, although he states this division to be highly practical, he neglects entirely to point out its bearing upon the treatment. This account of rheumatism, as well as of gout, we think, is improperly arranged with the diseases of the bones and joints, as though these parts constituted its primary seat. It would have been better included among the diseases of the blood, and then it would have been almost in juxta-position with diseases of the heart and pericardium, that constitute its most important and formidable complications.

Having expressed our opinions pretty fully on different parts of this work, during the course of the analytical review we have attempted to give of it, we have but little to add in conclusion. It certainly possesses very considerable merit, and testifies to the talent, ingenuity, and unwearied zeal of M. Piorry, in the investigation and application of almost every fact that can throw light upon the diagnosis of disease. Its faults are, its great size, arising from its diffuseness and the prominence given to unimportant particulars, and the desultory mode in which the subjects are treated; though the evil is remedied, in some degree, by a copious index: but it contains so much that is valuable, not only to the student, but to many who have not the advantage of attending hospital practice, and yet wish to keep on a level with the progress of medical science, that we shall be very glad to see it again, in a condensed form, and in an English dress.

The work of Dr. Suckow is one of the most striking instances we have ever met with of misapplied industry. The labour expended in its composition must have been enormous; and, although it contains an infinity of facts, we believe it to be utterly useless. Such a work, perhaps, could only be written by a German: assuredly, it will never be read by any one but a German. Its plan is, to take up separately every part and function of the human body; to enumerate, in order, every possible quality, appearance, and alteration that every such part or function can present; and then to arrange, under each head, every disease, organic or functional, of which such alteration is a sign, or in which it does or may by possibility occur! It may be conceived what a farrago of morbid affections are thus crowded together in most admired disorder, and without a shadow of scientific interest or practical benefit. The same sign is set down as indicating diseases of the most various and often directly opposite kinds, heaped pell-mell one upon another, without the slightest attempt at classification; and this, paragraph after paragraph, through 300 close-printed quarto pages! The following two paragraphs, the first from the section "*on Signs from the Position of the Head,*" and the second from one of the numerous sections "*on the Tongue,*" may be taken as very mitigated instances of the general character of the book.

> "*The head hanging down, or sinking inwards*—indicates inflammation, swelling, induration of the internal parts of the neck, of the external coverings, or of the vertebræ; wounds or cicatrices on the anterior part of the neck; weakness of the muscles; depressed powers; giddiness; stupefaction; sleepiness; idiocy; pressure on the brain; dazzling; cataract; bashfulness; sadness; meditation; melancholy; incipient hydrocephalus; cerebral or spinal paralysis; apoplexy; long-continued epilepsy." (p. 34.)

> "*White tongue*—indicates catarrh; aphthæ; *soor* of the mouth; tobacco-smoking; ptyalism; gastricism; scrofula; rickets; defective nutrition; long confinement; insanity; induration of the abdominal viscera; diseases of the heart; peritonitis; hepatitis; splenitis; ulcerated intestine; induration of the lungs; gastric, rheumatic, catarrhal, nervous, mucous fever; hæmorrhoids; hypochondriasis; phrenitis; meningitis; inflammation of the chest; ague with gastric complication; dyspeptic phthisis." (p. 86.)

That ever a man who had studied medicine or seen diseases should imagine that a Babylonish congregation of names of this sort could by possibility be "useful and agreeable in practice," (as the author hopes in his preface,) is really most amazing to us. But we have done our duty towards our many great and respected friends in Germany, by exposing the absurdity of the attempt.

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## ART. XI.

*Inaugural Dissertation on the Influence of Climate on the Health and Mortality of the different Regions of the Globe.* By A. S. THOMSON, Candidate for the Degree of Doctor in Medicine.—*Edinburgh*, 1837. 8vo. pp. 102.

THIS essay is a prize thesis in English. It appears that the writers of the greater part of the theses at the late graduations in Edinburgh have substituted the English for the Latin language: this, as we remarked on a former occasion, is a result to be expected from the regulation which renders the choice of the language optional with the candidate. We repeat, that we are not without fears that the new method may have the effect of lessening the amount of classical knowledge among the candidates for the doctorate. Still we are ready to admit that the old system was attended with very serious inconveniences; and, if the great superiority of the theses presented on the new plan is to be regarded as a proof of its superiority, it is impossible to resist the argument. To judge from such of the theses as have been printed, the improvement in the subjects, the matter, and the style has been very great. If we demurred to the propriety of having the thesis in English, we had no doubt of the necessity of having the principal part of the examinations in the vernacular tongue. Making Latin the language of the examination tended very much to render the whole a mere matter of form; and, as it was an easy step from procuring a translation to purchasing a thesis, we fear this step was often taken. These circumstances contributed not a little to the partial discredit into which inaugural dissertations have fallen as a test of mental capacity; yet this discredit is, in our opinion, unmerited. An oral or written examination may show the candidate's range of acquirements, the tenacity of his memory, and the readiness with which he can call up his knowledge, and bring it to bear upon any given question; but it does not fully display the original powers of his mind. An essay, where he has access to books, and time for observation, experiment, meditation, calculation, on the other hand, effectually answers this purpose. A practical examinator can form an accurate opinion, from the thesis, of a man's industry and acuteness in collecting facts, and arranging them in their natural relations: nor would it be so difficult as some imagine to identify the thesis as the candidate's own production, or to discern the common-place essays of venal writers, if proper precautions were taken. In directing attention to a special point of enquiry, the thesis also often lays the train of important subsequent results: several of our best works are known to have sprung from this source. Perhaps the candidate should not be put to the expense of printing against his wish; although the publication of a good thesis would be the best justification imaginable of the examining body, and of the title it conferred. The thesis, in our opinion, cannot be safely superseded: and we hope its presentation, at least, if not publication, will be among the regulations adopted by the senate of the new London university.

Dr. Thomson has dedicated his thesis to Henry Marshall, Esq., "who (he gratefully adds) suggested the subject, and from whose statistical researches a great part of the materials have been obtained." This, while it does not detract from Dr. Thomson's merit, raises the value of the work; as the character of Mr. Marshall's valuable labours in the medical statistics of the army is well established.

The author has not examined climate under the same point of view as Sir James Clark, in his classical work on that subject; he has treated its elements less as therapeutic agents than as causes of sickness and death, with reference rather to the prevention of diseases than their causes: but, as regard should always be had to the influence climate has in producing particular diseases, and in increasing or diminishing the mortality of natives and foreigners, no one can undertake to deal with climate, or to apply it in practice, not well versed in these particulars. Few will deny the necessity of this preliminary information, when they see patients sent in search of health to Jamaica, and other places notoriously unhealthy.

After a rapid sketch of the temperature, rain, and winds of the torrid zone, and of the various races of inhabitants, Dr. Thomson proceeds to examine the ratio of mortality among the indigenous population. It has been often stated that the mortality is greater among the inhabitants of the torrid zone than in temperate climates; but no positive observations have been produced to prove that the mean duration of life is really shorter between the tropics than elsewhere. The facts on which the statement set forth by M. Moreau de Jonnès rests have never been published; and, under these circumstances, do not deserve to be cited. The following observations appear to show that the mortality of the black troops on the west coast of Africa is above the average, but below the mortality of the black troops in the West Indies.

| Period of observation. | Mean number living. | Annual deaths per cent. |
|---|---|---|
| 14 years. | 469. | 3.6. |

In the island of Bourbon, Mr. Thomas states that the annual mortality of the slave population was 3 per cent.; among the 5069 free blacks, 1.6 per cent. The latter could not have included children. The annual rate of mortality in New Spain (tropical America) is 3.3 per cent. (*Humboldt.*) Lieutenant Tulloch has constructed an interesting table of the mortality among the slave population of the West-India islands, from which it appears that 2.8 per cent. die annually in twelve colonies taken together; and that the mortality in Tobago was 4.15 per cent., and in Antigua 2.4 per cent. The observations generally extended over fourteen years (1817-30), and embraced 249,037 deaths, 8,954,513 years of life. The mortality of the African troops appears to be higher than the mortality of the African slaves. In the nineteen years, 1810-28, 2,938 deaths took place out of 51,734 living one year: the mortality was 5.6 per cent., although the rules of the service protect the soldier from harsh treatment. Lieutenant Tulloch concludes, therefore, that the mortality and decrease of the slave-population in the West-Indies is due rather to the climate than to the miseries of slavery. It should be recollected that the mortality of soldiers is always higher than the mortality of the general population at the same age.

Of the black troops in the West Indies, (1817-31,) 30 per 1000 died annually in Jamaica; and, of these, 8 from fever, 10 from diseases of the lungs, 0.5 from diseases of the liver, 2.5 from diseases of the bowels. In the Windward and Leeward Islands, 40 per 1000 died annually; 4.4 from fever, 16.6 from diseases of the lungs, 1 from disease of the liver, 7.3 from diseases of the bowels. The negroes are, therefore, not exempt from fever, but it is much less fatal to them than to Europeans. Pectoral affections are by far the most destructive disorders among the African population in the West Indies; and this is to a considerable extent induced by the climate, if much confidence can be placed in an extravagant statement advanced by Dr. Winterbottom. Speaking of consumption among the native Africans in the vicinity of Sierra Leone, he says, "As an idiopathic disease, I do not recollect to have seen a single case!" However this may be, tubercular consumption is exceedingly fatal among the negro troops, not only of the West Indies, but of Ceylon and the Mauritius. (pp. 22-3.) Colonel Sykes stated, at the British Association, that the mortality in the Deccan (population, 3,000,000,) was 2.7 per cent. annually. In 1827-30, the annual deaths among 74,850 native troops amounted to 1010, or to 1.35 per cent. In the presidency of Bengal, the native troops, in 1833, amounted to 90,075, the deaths to 955; to 1.06 per cent.: in the presidency, the mortality was 5.8 per cent.; in Cawnpore, 0.7 per cent. In 1827-30, the deaths in the native troops of the Madras presidency amounted to 4,041; the deaths per cent. from fevers to 0.25, pectoral complaints 0.06, diseases of the liver 0.01, diseases of the bowels 0.11, cholera morbus 0.33; all diseases 1.35. The mortality from pectoral diseases is low: among the troops in Scotland it is 0.46 per cent., nearly eight times higher than among the native troops of

India. The proportion *constantly sick* is a very important point of enquiry: the only light thrown on this is derived from returns of the sick among the native troops.

*Sickness among the Native Troops of Bengal and Agra*, 1833.*

| Total strength. | Mean number constantly sick. | Sick per cent. | Deaths per cent. |
|---|---|---|---|
| 90,075 | 4,238 | 4.7 | 1.06. |

Dr. Thomson then investigates the number of cases of sickness and the influence of the seasons, which concludes the first chapter on the climate of the Torrid Zone.

The second chapter is devoted to the *Temperate Zones*, where the several branches are examined in the same order, but at greater length, as the materials are more abundant. The third chapter treats of *the Influence of a Temperate Climate on the indigenous Inhabitants of the Torrid Zone;* the fourth of the *Influence of an Intertropical Climate on the indigenous Inhabitants of the Temperate Zones.* Our limits do not allow us to make any further analysis of Dr. Thomson's work. The following facts, however, our readers will do well to compare with others previously cited.

*British Troops in Bengal and Agra*, 1833.†

| Strength. | Died. | Mean Sick. | Died per Cent. | Sick per cent. |
|---|---|---|---|---|
| 11,999 | 632 | 1,551 | 5.3 | 12.9. |

According to Dr. Burke, (1826-32,) the deaths, out of 60,904 living one year, amounted to 3,486, or 5.7 per cent.; namely, 1.54 by fevers, 0.25 by pectoral diseases, 0.42 by diseases of the liver, 1.84 by diseases of the bowels, 1.15 by cholera morbus. In the West Indies, (1817-34,) diseases prevailed among the British troops in very different proportions. In the *Windward and Leeward Islands*, the annual mortality was 8.0 per cent.; 3.86 from fevers, 1.05 from diseases of the lungs, 0.19 from diseases of the liver, 1.9 from cholera morbus. In *Jamaica*, the mortality was 13.1 per cent.; viz. 11.26 from fevers! 0.74 from diseases of the lungs, 0.8 from diseases of the liver, 0.45 from diseases of the bowels, 0.1 from cholera.

Dr. Thomson has not succeeded in showing the influence of a change northward on the inhabitants of tropical climates; a defect we shall endeavour to supply from a very interesting series of observations, published by Sir James Macgregor, in his Medical Sketches (1804.)

When the French were in possession of Egypt, an English army was assembled in the Mediterranean, to drive them from that country; and at the same time an Anglo-Indian army moved from the shores of the English possessions in the East. This army was 6,800 strong. The regiments disembarked at Cosseir Bay, in 1801, and arrived only in time to see the French capitulate, without having any share in the campaign. They remained nearly one year and a quarter in Egypt. There were 3,759 British troops; 3,042 Sepoys (Hindoos): the departments, 419, and public and private followers, 666, we exclude at present from calculation.

As the regiments landed in different months, it was necessary to reduce the time to one year; which was done in the following manner: The 10th English regiment landed "28th November, 1800, and left Egypt 1st May, 1802:" it therefore remained in Egypt one year and five months, = 1.42 years. The strength, 984, multiplied by 1.42, would give 1,397 years of life: in other words, 984 men in Egypt one year and five months may be considered equivalent to 1,397 living there one year. During their residence in Egypt, 98 died, and 49 were invalided; half of these (73) should be deducted from 984 as a correction; but, as the deaths and invaliding occurred in greater proportion towards the latter end of the period, we have subtracted them from the years of life, 1,397, thus reduced to 1,324. The ten different English corps and the nine Indian corps were thus treated separately, and the following were the results:

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* Report of Mr. Hutchinson on the Native Jails. † Ib.

| | BRITISH. | HINDOOS. | ANN. PROPORTIONS. | |
|---|---|---|---|---|
| | | | British. | Hindoos. |
| Strength on landing | 3759 | 3042 | | |
| Years of life | 4355 | 3551 | 100 | 100 |
| Invalided | 117 | 41 | 2.7 | 1.2 |
| Total deaths | 315 | 326 | 7.2 | 9.2 |
| Deaths by Plague | 38 | 112 | .9 | 3.1 |
| Fever | 18 | 85 | .4 | 2.4 |
| Liver Complaints | 64 | 10 | 1.5 | 0.3 |
| Dysentery | 148 | 42 | 3.4 | 1.2 |
| Diseases of Lungs | 4 | 4 | .1 | .1 |
| Stroke of the Sun | 2 | 0 | .05 | 0 |
| Casualties and other Diseases | 41 | 73 | .9 | 2.1 |

It will be recollected that the mortality of the Hindoo troops in India is 1.35 per cent.; and that the mortality of the British troops in India is about 5.5 per cent. In Egypt, "on the bleak shores of the Mediterranean," (Macgregor,) where the thermometer raged in December from 49° to 70°, the mortality of the Indians was 9.2 per cent. two per cent. higher than the mortality of the British engaged in active duty. The number invalided from the British troops was higher than the number invalided from the Hindoos; the casualties among the latter were more numerous; and two Bombay regiments of Sepoys were particularly exposed to the causes of fever and plague. It may, therefore, be admitted that, *cæteris paribus*, the mortality would be nearly the same in the troops of both races. But what a remarkable and characteristic difference in the disease by which they were attacked and decimated! The Indians fell chiefly by plague and fever: the Europeans by dysentery and liver diseases. One of the English regiments came from the Cape of Good Hope; the others from Calcutta, Bombay, &c.; but the difference of mortality on diseases in these cases was not considerable. Among the Indians, the mortality bore principally on the 1st and 7th Bombay regiments; the 7th, a corps of 708 men, lost 69 by plague, 42 by fever; in all, 160 by death, 10 by invaliding, in 1¼ year. This regiment was harshly used. On the voyage, it had suffered severely from fever; the transports having been probably crowded. While the rest of the army proceeded to salubrious quarters at Alexandria, the 7th Bombay regiment was left at Rosetta, where the plague raged violently, till January 8th: it was then marched to Aboukir Bay, to remain isolated with a pest establishment. The Indians gave themselves up to despair; and, when conveyed to the Pest-House, would take neither food nor physic. The mortality in the departments, consisting of 419 natives, was low: not more than 25 deaths (10 of plague) out of 578 years of life, equal to an annual mortality of 4.3 per cent. The deaths (forty) among the public, and private followers (666), were equally few; showing that half the mortality was due to the peculiar exposure and hardships of the common soldiers on active duty.

The march of the Anglo-Indian army over the desert of Thebes, and the descent of the Nile, excited a great sensation at the time; and we are not aware that any other statistical observations exist showing the influence of climate on the Hindoo race. To make this notice complete, it may therefore be well to recall a few of the circumstances recorded by Sir J. Macgregor, in his Journal of the Expedition.

To carry the men from India, large, lofty, and roomy ships were fitted up, and the greatest care was taken to embark *only such as were in perfect health.* Excellent provisions and wine for the sick, were provided. The army landed at Cosseir Bay, and penetrated Egypt through the desert of Thebes. In *June*, 1801, there was little sickness; in *July*, the army at Ghenne suffered more; on *August* 12th, great part of the army, after a navigation of the Nile of nearly 400 miles, arrived at Ghiza. The troops were uncommonly healthy on landing: in a week, most of the corps sent eight to ten per cent. of their strength to hospital. In three weeks, there were 1000 sick, chiefly of fever. The cases of ophthalmia were also numerous. Ghiza was found to be swampy. At the end of August, the army embarked for Rosetta: sick 1200. Rosetta is surrounded on two sides by swamps. The streets are narrow, dirty lanes, with high houses, overhanging each other, and the passage frequently interrupted by houses in ruins. Here, during *September*, the sickness gained ground rapidly; particularly ophthalmia, dysentery, and hepatitis. The first cases of plague appeared; and the cases of ophthalmia amounted to 600. In *October*, the army removed 1-5 miles distant from Rosetta; *November* 1st, sick 1350, or more than 25 per cent.; 170 cases of intermittent fever occurred. Sick in the weekly return, November 20-26, Europeans, 632, Sepoys, 380, = 1012. *December*, nearly all the army marched to Alexandria. Several cases of plague and continued fever occurred; and, towards the end of the month, many cases of catarrh, pneumonia, rheumatism, and ophthalmia. December 3d, 312 cases of ophthalmia; December 31st, 98. January, 1802, the mortality increased; the number of sick was smaller: 92 cases of plague occurred. *February*, 1802, fever, hepatitis, dysentery decreased; 21 cases of plague occurred. February 1st, sick 705. February 28th, 312. *March*, small-pox broke out among the sepoys: total sick of the army, 300. The Indian army was now ordered to return. It marched by night over the desert of Suez, encamping at sunrise. It suffered several times from the dry suffocating wind of the desert; which, at Moses's Wells, caused the thermometer to rise from 60 to 98 degrees: ophthalmia became more frequent. On June 2d, 1802, the embarkation commenced. Most of the corps were in a healthy state, except the 7th regiment (Indian), which was ordered to remain two months. No cases of plague had occurred after the army reached India, so late as November, 1802.

Besides the diseases in the table, the Indian army suffered severely from ophthalmia; it sent home fifty invalids from blindness. Few officers were attacked. In the 88th, (strength 466,) where forty men did not escape, only two out of thirty officers were attacked: one lost an eye. The Guinea-worm was little seen in Egypt: on the voyage 199 cases occurred among 360 men of the 88th.

The deaths in the French army, from May 30th, 1798, to September 30th, 1800, amounted to 8542;* 3561 happening in battle, 852 as the consequences of wounds; 281 died of accidents; 1429 of plague; 2419 of ordinary diseases. From other facts, which we cannot discuss here, it appears that the strength of the French army was 25,729. The annual mortality, therefore, was 14.2 per cent.: 5.9 per cent. were killed in battle; 1.4 died of wounds; 0.5 of accidents; 4.0 of common diseases; 2.4 of plague, = 14.2. Exclusive of the deaths from fighting, the annual rate of mortality was 6.9 per cent.: very nearly the mortality observed in the British troops. The annual mortality of the French army at home, including officers, is 1.94† per cent.; it was, therefore, increased more than sevenfold by the Egyptian campaigns, the climate, and the epidemic plague.

These facts authorize us to conclude that the mortality of inhabitants of the torrid and of the temperate zones is nearly equal, when transported to an intermediate climate; but that the former suffer principally from epidemics, an inference supported by some statements in Dr. Thomson's Essay.

In concluding this article, we must speak favorably of Dr. Thomson's Prize Thesis. It is a judicious summary of the facts best established in this department of medical science; and is no less creditable to himself than to the school whence it originates.

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## Art. XII.

*Institutes of Surgery: arranged in the Order of the Lectures delivered in the University of Edinburgh.* By Sir Charles Bell, K.G.H. F.R.S. L. & E., &c.—*Edinburgh*, 1838. Two Vols. 8vo. pp. 353, 380.

Placed at the head of a large class of surgical students; enjoying a high reputation; with a mind matured by the active practice of his profession during a period of nearly forty years; it appears natural that the author of the work before us should desire to give his pupils the benefit of his experience in a digested form: and they had a right to expect that the book in which that experience was recorded should contain at least a fair summary of what it was essential they should know of the theory and practice of surgery. It was, we apprehend, incumbent upon him so to frame his guide-book that the students, who attended his class, might fairly lean upon it for support in fitting themselves for the active practice of their profession;—that, though only a summary of what was known, it should be a fair and sufficient summary; and that, in those cases where it left much to be sought for elsewhere, it should have carefully indicated the best sources where it might be found. It is our business to enquire how far and how well these objects have been attained.

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* Desgenettes, Histoire Médicale de l'Armée de l'Orient.

† Benoiston de Châteauneuf: Annales d'Hygiene, t. x., p. 2, 1833.

The work is doubtless an outline of the lectures which the author has delivered, and many of the blanks which exist in the "Institutes" may have been filled up in the communications made to the student; but the existence of those blanks, and they are numerous, is a serious drawback on its general usefulness, and must materially lessen its value to surgical students at large. There is also a painful want of unity or plan in the work. A large portion of the subjects are merely shadowed out, whilst a considerable number are tiresome from their minuteness of detail: thus, to Mortification, including its nature and treatment, two short pages are allotted; to Phlebitis, one; to Caries, that most important disease to the surgeon, twelve lines are devoted; while Scarifying the Gums boasts of two pages, and Pivoting Teeth of one. Again, at the end of the second volume we find an "Appendix" of 121 pages of small type, containing more than a third of the matter in the whole work; this space is occupied with clinical lectures (delivered at the Middlesex Hospital, and long since published in the Medical Gazette,) and a large number of cases, which, if they appear at all, should have been placed in the chapters which treat of the diseases to which they refer.

We are most sincerely grieved to notice these, and, we regret to say, yet greater blemishes that are to be found in this work; because the unanimous voice of the present age and of posterity must hail Sir Charles Bell as one of the lights of medical science. His labours and great discoveries in Physiology will render his name immortal, as they now render it revered in every country of Europe. Why, then, does he force upon our attention, in many parts of these volumes, a narrowness of mind and illiberality of feeling so unworthy of his intellect and his fame? The following, is an example. "In noticing a few authorities," he says, "I have preferred *English* authors, principally because they stand distinguished by truth of narration." (*Introductory Chapter*, p. 23.) We deprecate, in the strongest terms, this insinuation as to the good faith of foreigners. No matter what their character or country, they are involved in one sweeping condemnation as persons whose statements are unworthy of credit. What right have we to arrogate to ourselves exclusive purity? Is truth never violated in our own land? No doubt there are in all countries persons whose veracity may be impeached; in our own, we regret to admit, that many such may be found; but would this circumstance palliate the injustice of that person who should stigmatize us as a nation of liars?

In the notice which we propose to take of this work, we shall so far follow the sketchy and desultory manner of the author as to put down, without much regard to connexion or continuity of narration, such remarks as occur to us in its perusal. We shall have occasion to condemn as well as to commend: and here, as in all other cases, we shall be influenced more by what is due to science and our readers than by any personal regard we have for the author.

At page 7 is a passage which astonished us; the author says "it will not be imagined that, after the life the author has led, he can desire to throw discredit on morbid anatomy; but, by pottering in the dead house we shall not discover the sources of disease." It certainly is difficult to imagine that any man holding the office of professor of surgery, and charged with the direction of the studies of a large number of young men, should have deliberately penned such a sentence. What should be its natural effect, but to impress the student with an opinion that it was unnecessary to devote much time to the prosecution of this branch of medical knowledge? What can be more contemptuous than this expression, coming, too, from a person who has profited so largely from morbid anatomy?

The third chapter is on *Adhesion*. In speaking of the means of procuring adhesion, the following judicious remarks occur. "All that regards dressing should be studied in the hospital. But from the bad taste (to give it the mildest term) which prevails, and which tempts surgeons to contend against time, and therefore to have the patient soon off the table, the dressings are huddled on or deferred. Surgeons of public institutions owe therefore a public duty,—to serve the poor, but also to be an example to their younger brethren, that right methods may be taught. The dressing of a patient after an operation is very often the most important part of the office of a surgeon, and anything like hurry the most dangerous lesson." (Vol. I. p. 18.)

On the subject of *Whitlow*, we differ from the author in his opinion that it originates in an inflammation of the vascular body which secretes the nail: in the majority of cases it may be so, but in a considerable number of instances it is entirely unconnected with this organ. With respect to the treatment of this complaint, he observes:

> "When the matter is deep, says an old surgeon, there is no alternative; place the patient's hand on a mass of tow, hold him firm, and with the scalpel cut to the bone! All I have to say is, that it may be done effectually, and with less pain, by passing inward a fine sharp bistoury, and cutting outward. Cut away the nail; touch the ulcer, and especially the very sensible fungus, with the blue-stone; and, according with the old advice, dress with the balsam copaibæ and tinctura thebaica." (p. 27.)

In treating *Chronic Abscess*, the following few plain and useful suggestions on the mode of opening them occur:

> "When you puncture an abscess, lay the flat head of your probe on the side of the lancet, and hold them firm. When you have penetrated to the matter, by a motion of your thumb introduce the probe further, and withdraw the lancet; or you may use the directory in the same manner; the groove of that instrument serving the purpose of a canula. A grooved needle is employed for exploring a tumour in which the surgeon is uncertain whether there be matter contained or not. The use of the probe or directory is to permit the matter to flow, and to prevent the necessity of much poking in the wound, which makes the union of the puncture uncertain." (p. 29.)

In the treatment of abscesses, to prevent their falling into obstinate sinuses, the following reasons for the necessity of rest are given.

> "I have found an abscess under the latissimus dorsi and under the trapezius, kept open by permitting the motion of the arm. Mr. J. Bell was wont to narrate a case of fistula under the pectoral muscle, which had not yielded to injections, setons, compresses, &c. cured by the necessary bandaging on the patient's breaking his arm. When there are abscesses in the temple, it is necessary to keep the temporal muscle

at rest by binding up the jaw. Sinuses after bubo in the groin are in like manner kept up by the motion of the part, and cured by strict rest, compress, and bandage." (p. 31.)

We have already remarked on the insufficient space given to *Mortification*. The chapter commences with the following observation:

> "Looking, as we do here, directly to practice, and eschewing theoretical discussion, we say, mortification is one of the terminations of inflammation; but it is essential to observe that when phlegmon terminates in the death of the part, there is an essential omission in authors; it is the effusion and infiltration into the cellular membrane which checks and hinders the action of vessels." (p. 38.)

In Dr. Carswell's "Illustrations of Pathological Anatomy," Fasciculus vii., published in 1835, (and similar statements are made by several other authors,) is the following observation on the subject of mortification: "these appearances would seem to prove that the rapidly destructive effects of this form of inflammation depend, in great measure, on the mechanical influence exercised by the effused fluids on the capillary circulation. These fluids must compress the neighbouring veins to a degree that will prevent the return of the blood poured into the capillaries." So much for the "essential omission in authors."

In the following page it is stated, that the cause of *gangrena senilis* "is ossification of the arteries of the leg;" that "it commences in a small black spot on the side of the little toe." Here, again, we refer to Dr. Carswell, and we find "that, in every case of gangrena senilis which I have examined after death, the arteries of the limb were obliterated to such an extent as to interrupt the circulation of the blood. The obstructing cause consisted, in five or six cases, of a fibrous tissue formed either in the walls or cavities of the arteries, and which had converted these vessels into nearly solid cords of ligamentous consistence." There is no doubt that, in old persons, who are the subjects of this disease, ossification of arteries would be found, but not often, in the leg itself. MM. Dupuytren and Londe incline to the opinion that it is always a consequence of arteritis; but this is not proved. With respect to the point at which the spot first appears, it is a question of fact to be decided by the observation of every medical man. In Sir C. Bell's experience it may have been the little toe; in the experience of other persons, this preference for the little toe has not been always observed.

In the next section of the same chapter, *Anthrax* is treated of very satisfactorily: were the other portions of the work as well done, our present task would be one of unmixed pleasure.

Chapter vii., *of Wounds*, gives also a good practical consideration of the subject. There is one point in it upon which we shall reserve our opinion until we come to the chapter on Amputation—the use of needles and ligatures.

Next comes the important subject of *Hemorrhage*, and this is treated with much ability: in fact, this is one of the author's best chapters, and is evidently written *con amore*. At page 52 he states, that "many years ago he showed that, if an artery was bleeding during an operation, and you took your forceps and pinched it, it would soon cease bleeding. The intelligent reader will see how this touches on the subject of the *torsion* of arteries, which is nothing more than a mode of injuring the coats." Does the author mean, by this passage, to insinuate that the idea of *torsion* came from him? Long before Sir Charles Bell pinched arteries with a forceps, it had been done by others: but this *pinching* will not restrain hemorrhage, except from very small arteries, and not at all in those of a fifth or sixth rate caliber; and, in principle, it is very unlike that upon which *torsion* is dependent. In the latter, the vessel is twisted upon itself so completely that a fair section of the internal coat is made, and a physical obstacle to the passage of the blood is formed, even in the axillary artery; and this so powerful, that we cannot succeed in establishing the canal, even with the assistance of the propelling power of a large syringe. It is probable that the ancient surgeon who attended to the following advice of Galen often *injured* the coats of the artery while *pinching* them: "Si vas undè profluitur alte sit demissum, certius ipsius tum positum intelligat, tum etiam magnitudinem; præterea vena ne sit, an arteriola. Post hæc injecto unuo attollat." (*De Loc. Aff.* lib. i. cap. i.) In Peyrilhe, also, (*Histoire de la Chir.*, tome ii. p. 638,) we find the following sentence: "Si le vaisseau qui fournit le sang est profond, le chirurgien tâche d'en reconnaître la position et la grandeur, de distinguer s'il est veineux ou artériel; il le soulève ensuite avec un crochet, et le tord un peu." Even for a century back (perhaps for many centuries) it has been the custom of sow-gelders, in the practice of their art, to restrain hemorrhage by twisting the vessel five or six times. Most persons are aware that M. Maunoir, of Geneva, published a series of experiments on the subject, in the year 1820. At page 53, Sir C. Bell says "a man of genius values what he has discovered, and respects the thing in others. Dr. Jones was wrought upon to be unjust: his external and internal coagulum were matters better explained by Mr. John Bell, who also showed the danger of trusting to this state of the vessel." It does not appear to us that it was necessary for Dr. Jones to have referred to Mr. John Bell. Our own opinion is, that Dr. Jones's observations were much the better of the two: besides, as far as discovery is concerned, it does not rest with either of these gentlemen. In 1733, J. L. Petit communicated to the Académie des Sciences the results of certain experiments which he had made upon the obliteration of arteries. According to him, there is formed, at the divided extremities of the vessel, two clots; one of which surrounds and presses upon the extremity of it, the other penetrates as a central plug. In 1736, Morand modified this theory, by stating that the artery suffered a retraction, accompanied by a circular femoral contraction, which served to "imprison" and fix the clot. Variously modified and expressed, this explanation has descended to the present day: to whom, therefore belongs priority? If Dr. Jones referred it to any one, it clearly should not be to Mr. J. Bell.

The following observations are accurate and judicious:

> "The ligature puts a mechanical stop to the hæmorrhage; it also stimulates to the reaction of the living vessels, and the adhesion and final obliteration. In this double office it should be considered, to arrive at just principles. At the time when writers on surgery ran away with the idea that the action of a ligature was to bring the inner coats of an artery into the state of an 'incised wound,' I foresaw what follies would be committed; and at that time I had a large class of intelligent students, I made the experiment to show that it was the presence of the ligature as a foreign body that excited the coats of the artery. I placed a circular ligature around the artery, letting it lie in contact with the proper coat of the artery, without drawing at all, and showed them the artery stopped by a clot." (p. 56.)

> "Mr. Lawrence, in the Medico-Chirurgical Transactions, vol. vi. page 156, treats of some important subjects, but the chief part of the communication is on the ligature of arteries. He is the advocate for small, hard, single thread ligatures, which he cuts off short by the knot. This was a practice of John Bell's, and which I did not approve of then, nor do I now. The object is to procure immediate adhesion, which, it is alleged, is prevented by the common manner of retaining the ends of ligatures. I have seen patients, under the hands of good surgeons, shrieking and complaining of their toes, when ligatures were put on the arteries on the face of the stump. In matter of fact, the ligature often includes the nerve. Conceive, then, the misfortune of having a fine, hard thread firmly tied on a nerve, and the ends cut off; conceive the distress, the pain, the tic, thence arising! and you will not follow this fashion, for it is but a fashion. Long after the stump should have been well, ulcerations have been occasioned by the ligatures being cut short, and the noose left." (p. 58.)

> "The conviction that my brother's conversation and practice left upon me was, that the artery should be firmly tied with a moderate-sized ligature; that the surrounding parts should be disturbed in the least possible degree; and care taken that, whilst the thread was within the sheath, and in contact with the proper coats of the artery, the vasa vasorum were to be preserved entire." (p. 62.)

Chapter ix, which treats of *Gun-shot Wounds*, deserves unqualified commendation. In speaking of the occasional difficulty of discovering a musket-ball, he says,

> "The nerves will sometimes indicate the course or position of a ball. I might have said, in the last case, that, by the numbness in the course of the radial nerve, on pressing against the ball, I had informed myself that, although it lay apparently superficially, the nerve ran over it. So, when a ball has taken its course through the pelvis, or across the shoulder, the defect of feeling in the extremity, being studied anatomically, will inform you of its course; that it has cut or is pressing on a certain trunk of nerve." (p. 78.)

His statement that he "never saw a living man with his thigh carried off," must not convey an idea that the case does not occur; for there are many such on record, and on indisputable authority, in which the patient recovered.

The chapter on *Tetanus* (x.) is as meager a sketch as could well be conceived. His description is as follows:

> "One of the most alarming consequences of wounds is tetanus. This consists of violent and permanent contractions of the voluntary muscles, and consequent tension and rigidity." . . . "As the cases have presented themselves to me, my attention has not been called to premonitory symptoms, though such might have been observed: dejection, and an anxious countenance, mark the commencement of the attack; the patient feels a stiffness and rigidity of the muscles of the jaw; the tongue is affected." (How?) "This rigidity extends to the abdominal muscles, with a pain striking inwards, as if the diaphragm were affected. The spasms become more universal, the more powerful muscles constraining the less." (pp. 84-5.)

The treatment is still more vague, and the references are to an essay by Dr. Maclagan and Sir George Ballingall; no notice being taken of Trnka, Larrey, Dupuytren, or the recent clever compilation of Mr. Curling.

From the chapter on *Ulcers*, which is a very good one, we extract the following graphic description:

> "An ulcer is attended with an absorption of the adipose membrane beneath and around it; its edge is elevated by deposit of coagulable lymph, and these together make the sore appear deeper than it is actually. Its surface is covered with a film of coagulable lymph, and in this the changes are wrought which we have to notice: in

this lymph the granulations are formed. The granulations are those small, red, convex bodies which appear on the surface of the ulcer: they secrete, and cover themselves with, pus." [Or rather, we should say, the pyogenic membrane which covers them secretes the pus.] "You may see the granulations forming on the inflamed surface of a bone. You perceive, on one day, a matter on the surface like pale jelly, which you might wipe off: on the morrow it is fixed, and, when you touch it, it bleeds." . . . "It is of importance to notice the *welt* around the ulcer; for, while it continues, the ulcer will not heal. It has been described as a circumvallation, which the ulcer throws up for defence: the idea is absurd, but it leads to a useful inference that you must cause the absorption of this hard circle, or it will be in vain to attempt healing the sore." (p. 87.)

In Chapter xii., *on Diseases of Bone*, Sir C. Bell says (p. 97), "I do not object to the term *periostitis:* but I believe it to be a mistake to describe the periosteum as the seat of a disease distinct from the affection of the bone itself." We apprehend that the mistake rests with the author. In many cases of periostitis, the bone is not affected, in those cases more especially where the disease has been induced by the vicinity of chronic ulcerations. We have lately known three cases in which the bone was untouched: in one, the periostitis was developed spontaneously on the inner border of the tibia, without any change of colour in the skin; the second and third cases were in the same bone, lower down: there was no purulent matter present in either, and the membrane adhered to the bone in the ordinary way. We are aware that the opinion of our author has been maintained by others. Hunter believed that the bone is primitively affected. We are not commonly called upon to examine such cases until the bone has partaken of the disease; but, if we attend to the seat of pain, and to the organic changes which are brought about in the periosteum, we cannot refuse to acknowledge that the disease first attacks the periosteum.

That most important disease, *Caries*, is disposed of by the following paragraph!

> "Much has been written to little purpose on caries; because they do not define what is meant. A surgeon puts his probe into an ulcer, and he finds that he can force it into the bone, and he says 'The bone is carious.' What is this? A bone softened by inflammation, where the earth has been in part absorbed. Or, again, in probing, he feels the bone rough: 'The bone is carious.' In this latter case the bone is dead; deprived, consequently, of its coverings and periosteum; it is exfoliating. Or suppose that there is an ulcer of the leg which extends to the bone: 'the bone is carious.' Thus is caries anything or nothing: the bone may participate in any disease modified by the peculiarity of its composition, in possessing the hardening material, earth or bone." (p. 98.)

Is this such a summary as we have a right to expect in a work written by a professor of surgery, as a guide or code of instruction for students?

Again, in *Necrosis*, (p. 100,) the subject is just as loosely treated. Sir C. Bell says, "There is no reasoning on the subject of necrosis, unless we go to the elements of anatomy, in which very few are properly initiated." But, surely, because the author entertains this opinion, he is not therefore to be exonerated from entering upon the subject; nor the student to be sent to practise his profession in perfect ignorance of this frequently-occurring and formidable disease?

Again, in p. 102, all the description given of *Mollities Ossium* is the following: "In that case there would be pains universally in the bones, with earthy deposit from the urine. The disease proceeds, I am afraid, unchecked by remedies. The disease is very slow in its progress, which therefore makes the observation of the effect of remedies difficult." Now, what idea of the nature of this disease can the student derive from such a description?

*Fragility of Bone* does, we apprehend, exist under other circumstances than "from want of use and old age;" but we have no room here to enter upon that subject. The cancerous and scorbutic diatheses appear to predispose to it. The experiments and observations of Troja show that it does not depend upon a superabundance of the calcareous phosphate or a diminution of gelatine.

Chapters xiii. and xiv. relate to *Fractures;* and, with considerable defects, contain much valuable practical matter; but, as we shall shortly notice the whole subject, in a separate article, we pass it over for the present.

The portion of the work which treats of *Diseases and Injuries of the Spine* is entitled to much praise; but even here, good as we deem it, we have to regret that the author seems to be unaware of everything done by younger men than himself. We could have wished that he had expressed an opinion upon the mode of treating diseases of the spine by laying the patient on his belly upon an inclined plane. By this plan certain important objects are attained: the patient being kept there night and day, you secure quiet; the feet and the arms hanging down, exert some straightening influence; no pressure is made upon the curvature; and applications of any kind may be made with great facility. We have recently seen this plan in operation, the children being very cheerful and satisfied, feeling no restraint; and the functions of the abdominal viscera much improved by the pressure made upon them.

Among the cases of fractured spine which he gives, it is singular that Sir C. B. did not allude to that communicated by Mr. B. Phillips to the Medico-Chirurgical Society, and published in the Society's Transactions. In this wonderful case, the man fell from a hayrick upon the back of his head; he was stunned by the fall, but soon recovered; he lived eleven months, and the only discomfort which succeeded the injury was an inability to rotate the head. After death, it was found that the violence had fractured the atlas transversely; the anterior part of the ring was driven down between the pharynx and the spinal column, until it came on the same plane with the axis, to which it became attached by perfect bony union; the transverse ligament which attaches the processus dentatus was strong enough to carry that process with it: and yet all this violence occurred without occasioning any important or distressing symptom!

Sir C. Bell is incorrect in stating that authors have considered dislocation of the spine in no other light than as producing pressure on the spinal marrow. It is true they set it forward as the most prominent evil, and it is so; for pus is not always formed as a consequence of such injuries; for instance, it was not even in Mr. Phillips's case: neither does inflammation of the spinal marrow always occur. He will also admit, and should have done so already, (as it appears that the singular vertebræ in Mr. Phillips's case, as well as its history, is in his possession,) that, "when the fracture is above the principal origin of the phrenic nerve, the act of respiration is [not] always stopt; and that death from suffocation does [not] always suddenly follow." We are free to admit that the cases in which an attempt has been made to elevate a depressed portion of the spine have not been very successful. We know of only four cases; and, of these, one was performed successfully, as we are informed, only a few months ago, by a surgeon of the name of Edwards, living at Caerphilly, in South Wales. There were present the usual symptoms of compression,—paralysis of the organs of locomotion, the rectum, and the bladder: the situation, as far as the operation was concerned, was unfavorable,—the lumbar region; the posterior arch of the bone was raised, the symptoms of compression relieved, and the patient did well. As far as other circumstances were concerned, the situation was indeed favorable; for it may have been at a point where the medulla spinalis no longer retains its cord-like form, and where pressure is less suddenly fatal in its effects. How long Cline's case survived the operation, we do not know. Tyrrel's died of peritonitis, at the end of three weeks. Barton's patient lived only four days; but during that time there was a momentary return of sensibility. The operation therefore is not, as Sir Charles states, "inevitably fatal." We are unable to comprehend the grounds upon which the author assumes "that the membranes of the spinal marrow are the most susceptible of inflammation and suppuration of the whole frame." (p. 157.) In our ignorance, we should have said, without hesitation, that pleuritis was more commonly seen than spinal arachnitis.

*Injuries of the Head* occupy the sixteenth chapter; and with it neither ourselves nor the student have cause to be dissatisfied. On the subject of compression, to which the author has given much attention, there are some ingenious remarks, which, although we do not entirely adopt, we may refer our readers to. The author states "that there is a hiatus in all writers on wounds of the head; they take cognizance of no other cause of torpor than compression." He should have stated that he supposed this must be so, but that he had not ascertained that the statement was correct. It happens that most writers on the subject distinctly point out that torpor may exist without compression: for instance, it may accompany the incubation of inflammation which has been excited by simple concussion.

The section on *Hernia Cerebri* is most unsatisfactory: why not either have referred to or condensed the able memoir on the subject by Louis?

In chapter xviii., where he speaks of *Injuries to the Hip-joint*, he should have stated that the changes produced in the head and neck of the femur by "inflammation of the hip-joint" are also produced by external violence, by a fall on the great trochanter, and that it is described satisfactorily by Mr. B. Bell, in an essay on Interstitial Absorption. All that is said under the head of *Compound Dislocation of the Knee* is, "This is a case for amputation." In the section on *Dislocated Ankle*, he has omitted two cases which may be found in the fourteenth volume of the Medical Gazette, and which are quite unique. By jumping against a bank in one case, and falling forwards, the dorsum of the foot was forcibly brought towards the tibia, and the astragalus alone was driven backwards, until it got clear of the articulation, and, without breaking the skin, caused the tendo-Achilles to describe an angle of sixty-five degrees.

In *Dislocations of the Humerus* at the shoulder, the author very properly alludes to simple modes of reduction. The following is one in which a person, unassisted, may almost always succeed in reducing it:—Let the patient sit sideways in a chair, and let the injured arm be brought over the back of the chair, which has been previously padded for the axilla: take a round towel; make a noose by passing the end of the towel through, and fix it upon the arm just above the elbow: you then place yourself at the back of the chair; of the depending part of the towel you make a stirrup, into which you put your foot, and make as much pressure as may be necessary: you have thus both hands free to manipulate the joint.

In *Dislocation of the Thumb,* Sir C. B. seems to be desirous of impressing the student with the conviction that he had better not use violence in reducing it, because the second joint "has been torn off in reducing the first." We agree with him that such violence is improper; but it is still very desirable that the dislocation should be reduced, and he should have been prepared with some substitute. Mr. Liston states that he had succeeded by cutting the lateral ligament.

In speaking of *Club-foot,* he says, "on dissection, I have found the defect less than could have been imagined." Our own impression is different: it is true that a considerable deformity may exist without occasioning anything more than a change of direction, in the phalanges of the toes, the metatarsal, and even the three cuneiform bones; without, in fact, any change of configuration. But, when we come to the scaphoid, the cuboid, the astragalus, and the calcaneum, the case is different. Scarpa held that the scaphoid was most deformed, but this opinion is certainly incorrect: it suffers little or no change in configuration, but much in direction; the greatest change which happens in the scaphoid is a loss of bulk, or atrophy, produced probably by the pressure consequent upon its unnatural position; the cuboid is similarly affected. The astragalus suffers the greatest deformity: there is here not only change of direction, but also of conformation; it is usually diminished in volume; it is always deformed. The calcareum follows the astragalus in its displacement, only its external border rests on the ground. The plantar fascia, the ligaments, especially the internal lateral, and the muscles, also suffer considerable changes. How this state is produced, it is not easy to determine. "Mechanical means" will not always cure this deformity; and it is singular that the author has not pointed out another mode of treatment, at this moment much employed, and certainly with great success in numerous cases. This operation consists in the section of one or other of the tendons of the muscles of the leg, which allows of a ready reduction to the natural state. A large number of cases of this kind are now on record, many of them of very old standing; and, although some of them may have been curable by well-directed mechanical means, yet there are others which would not have been so cured; and in all cases the time occupied is much less. It would be desirable, but difficult, to determine the circumstances under which we may cure club-foot without section of the tendon. By the hand alone, in infants, we can change the direction of the parts, and the section is unnecessary. And, in this case, they may be readily enough maintained in their new position by plaster of Paris. In after-life, we may also dispense with the section wherever the muscles will give way upon the force which may be applied by the hand; but, when this cannot be effected, the section of the tendon will enable the practitioner very soon to restore the foot to its natural direction. Another reason why this method should be employed is, that the operation is not nearly so painful as the extension procured by brute force, and it seems to be unattended by danger.

The second division of the volume commences with the *Diseases of the Natural Passages*. In speaking of *Ranula*, he says, "I have taken the patients to old and experienced practitioners, to ask what they called the complaint? They have answered, Ranula! I have then passed the Anel's probe into the sublingual duct, showing it to be quite pervious: therefore, I do not give the definition 'a tumour under the tongue, from an obstruction of the duct.'" (p. 218.) No one knows better than Sir C. Bell that a calculus may pass along the urethra, that it may be detained at a certain point, that it may obstruct the passage, that a pouch may be formed for it, that it may glide into it, and that a bougie may then be easily passed along the urethra: why may not a similar circumstance occur to the sublingual duct,—for cretaceous particles are often found in ranula? But we look at it under another aspect: we apprehend that ranula is usually understood to be a tumour which results from an accumulation of saliva in the excretory ducts of the *submaxillary* glands; rarely in those of the sublingual. However, we are free to confess that, whether these tumours be serous cysts in contact with the parietes of these ducts, or in direct communication with them, is a fact yet undetermined. The disease is an obstinate one, and difficulties are often experienced in curing it. Besides the methods of treatment alluded to by our author, there is one which was invented and successfully employed by M. Dupuytren. The instrument he used was a little hollow cylinder, terminating at each end by expansions something like two buttons joined together by a smaller central portion; a small snip was made in the tumour, so as to admit with difficulty one end of this instrument, which was introduced and left there. It was afterwards modified by making it solid, it being found that the saliva escaped around its circumference.

The section on *Stricture of the Œsophagus* is important.

> "I have been in the use of passing the bougie at regular intervals, so as to preserve the passage free. There is another method, effectual and safe; by using a kind of probang, with a conical piece of sponge. This, dipped in a weak solution of nitrate of silver, and passed repeatedly through the stricture, dilates it, clears away the mucus, and removes the disposition to spasm. The patient swallows with comfort for a long time after this process. But to the question we must come at last, what is to be done when a wretched creature is actually starving, in consequence of stricture in the œsophagus? I look in vain for authority and advice. I have attempted, by incision on the side of the neck, to divide the stricture and use a tube, but failed." (p. 243.)

With respect to dilatation by bougies, it is true a certain quantity of relief is thus afforded, but we fear this is all. There are some cases on record in which a cure has been accomplished. Paletta mentions one case, Home another, Earle a third. Mr. Fletcher has used a three-branched dilator, but it is an unscientific instrument. Costallat's plan has also been recommended. Four times, in a solid form, nitrate of silver was applied "successfully" by Home; three times unsuccessfully; three times also by Andrews, and in one alone did it succeed. We cannot feel satisfied that these were genuine cases of stricture. If the stricture be a consequence of chronic inflammation, of induration of the mucous or submucous tissue, cauterization may be attempted; but how can we be sure that it is not cancerous, fungous, &c.? and, in that case, what would be the use of such an application?

On the subject of *Phthisis Laryngea* he says, "I long since, in hospital reports, recommended the solution of nitrate of silver to be squeezed into the glottis, by means of a piece of sponge attached to a catheter wire. The late Mr. Vance adopted the practice." (p. 250.) This passage would seem to convey the idea that Sir Charles was the originator of the practice. This may have been the case; but it is well known that Mr. Vance employed it long before the publication of "Hospital Reports." But, for a fuller consideration of this subject, we refer the reader to the article in our last Number.

At the commencement of the section on *Diseases of the Prostate Gland*, the following judicious advice is given to the student:

> "The student of surgery has his most important lesson to get from the dead body. He should examine the outside and neck of the bladder, the place of the entrance of the ureter, the relation of the vesiculæ seminales and prostate gland, the distance of the prostate from the anus. He may consider how these parts should feel with his finger in ano in the living body. He should open the bladder, and pass his probe along the mucous membrane, thinking of the possibility of natural obstruction to instruments, and the place of diseased obstruction." (p. 285.)

This section is, however, on the whole, unsatisfactory and incomplete.

The section on *Irritable Bladder* contains some good practical remarks. In the case of children who pass water when asleep, Sir C. B. repeats a recommendation made by him long since.

> "The position of the child in bed should be attended to. When we lie on the back, the urine gravitates to that sensible part of the bladder on which the action of the muscles depend. The sensible spot on the interior of the bladder, just below and posterior to the opening into the urethra, commands the muscles at the neck of the bladder as infallibly as the sensibility of the glottis controls the muscles of respiration. When that spot is irritated, you are forced to make water. If you order that the nurse should lay the child on its cheek, inclining it to lie on the stomach or the side, there will be no call to urine, no wetting of the bed. On giving this rule to older patients, they have found it infallible; so much so, that they would start up the moment they were sensible that they had turned on their back, knowing the consequence." (p. 293.)

*Stricture of the Urethra* is noticed somewhat in detail, as its importance deserves. "The permanent cure of stricture, (says Sir C. B.) is to be accomplished by dilatation." If, from this sentence, the student should infer that stricture is, or may be, permanently removed by this means, he would be in error. A certain, but not a large, number of obstructions in the urethra may be removed by the judicious employment of dilating bodies; but, if a stricture have existed long, and if it be dependent upon a mass of induration occupying the submucous tissue, the only *cure* which will ordinarily be obtained by such means is a *temporary* dilatation of the canal. We say temporary, because the tendency to contraction remains, and will be soon developed, unless the use of the dilating instrument from time to time (that is, with intervals of perhaps a fortnight,) be persisted in. The statement, therefore, "that the membrane has no disposition to contract," is, we believe, incorrect. We do not think that the presence of the dilating body for ten minutes is enough, unless the urethra be irritable: many men express no inconvenience at the end of half an hour; and, when this is the case, it would be better to retain it so long. We agree with the author that extensive applications of caustic are rarely necessary, and that they must be followed by a tendency to contract, in consequence of the generation of fibrous tissue; but we do think that there are modes of applying it very preferable to the armed bougie. Allusion should have been made to the modes of using it recommended by Ducamp, Lallemand, and Phillips. We apprehend that the stimulus given by a slight application of caustic,—merely brushing over, in fact,—should be soon followed by the dilating body; as, by this combination, the absorbents are more efficiently excited to the removal of the indurated matter than by simple dilatation.

Sir C. Bell has lived long enough to modify an opinion once expressed, now many years ago, that no stricture could exist at or near the prostatic portion of the urethra: in the present work, it is so put that the truth of his position depends on a definition. We are not, however, disposed to quarrel with any man for abandoning a position which is no longer tenable; we are desirous only that opinions should never be too positively maintained. In the works of M. Lallemand and Mr. Phillips are several cases in which the obstruction existed between seven and nine inches from the orifice. Some of the obstacles no doubt occupied the prostatic portion of the canal; but we are quite aware that we may be met by the statement that the obstruction was caused by Cowper's glands, the sinus pocularis, or, in fact, by anything but by a stricture in or near the prostate. M. Lallemand, however, gives the post-mortem examination of a person who died during treatment for urethral disease. "Within a few lines of the prostate, (he says,) we remarked projections, hard to the touch, about five lines long, and of the thickness of the nail; they appeared to be evidently the debris of the principal stricture."

That "there are men whose hourly business is poking into this passage with bougies, who know nothing of its structure or diseases," is unfortunately too true, but, in every period of civilization, pretenders will exist. It is still more to be regretted that many men who do know its structure are accustomed "to force" strictures, and rupture the membranes. This is a reproach to which our author is not open; we have often admired the great delicacy with which he used instruments in this canal. This tact, however, was not more than is absolutely necessary: very few cases can arise to justify force, or in which gentle and patient treatment will not accomplish all which can be done by force, and without its evil consequences.

The best chapter in the work is unquestionably that *on Amputation*. The principal points are here more carefully considered, the omissions fewer. No student could read it without improvement; no practitioner without satisfaction. As reviewers, however, it is still our duty to consider the propriety of certain of the instructions which are laid down, and to point out the reasons which induce us to withhold our entire concurrence in the positions which are taken.

"An amputation," says Sir C. B., "is well performed when these objects are attained:—1. The skin so cut that it shall cover the muscles. 2. The muscles covering and concealing the end of the bone. 3. The bone sunk deep with all its periosteum about it. But, besides these, there are other very important circumstances which innovators forget—the ligature of the arteries, and the condition of the nerves." (p. 323.)

Why that fling against innovators? it is unnecessary, and not generally applicable; besides, in many situations in the present volume, he prides himself on his innovations; and why should he not? to innovate successfully is great merit.

We make the following extract simply because the subject is very important, and because anecdotes, as illustrations, have often more force than simple directions or suggestions.

"You look around, as you ought in all the principal operations, to see that every thing is prepared. Will you believe that the late Mr. Lynn, of the Westminster Hospital, on putting out his hand for the saw found there was none! and they had to send to the joiner. That, on another occasion, in tapping a woman, the foolish assistant gave him the stilette, and kept twisting the canula between his fingers, which was not discovered until the surgeon had plunged the instrument into the woman's side! Recollect what befel a good man, that on operating for the stone, and having made his incision, there were no forceps—no, nor within twenty miles of the place. From that time the gentleman resigned his profession, and all men pitied him." (p. 324.)

He insists, as he has formerly done, (and a large number of the profession have at last acknowledged the justice of the opinion,) upon the necessity of a particular position of the limb when the bone is sawed through.

"The pupil who has sat holding the leg now rises, and raises the limb so that the thigh-bone is perpendicular. You touch with your knife as the muscles withdraw themselves from the bone, especially on the back part, and at the linea aspera. If you at this juncture take a general view of the parts cut, you ought to see, towards the leg, the skin retracted off the fascia, and the muscles projecting, and about two inches of the bone bare." (p. 326.)

The bone is then sawed through close to the integuments. "When the amputation is thus performed, and when you bring down the stump, the bone has disappeared entirely among the muscles; you see it no more, unless you again raise the stump to the perpendicular. When you bring the integuments over the muscles, they fall together neatly, their edges exactly corresponding, and without the slightest irregularity." (p. 326.)

The author's estimate of the comparative merits of the circular and flap operations is very good, and is singularly contrasted with Mr. Liston's consideration of the subject, who does not seem aware of the existence of any other method of amputation than the flap operation.

"Let us consider the different conditions of the face of the stump. The muscles being cut obliquely, and in some measure drawn out by the knife, the integuments (which has a thin edge) is too short to cover them. You will see this exemplified in the flap operation below the knee, where the parts being cut from within, the gastrocnemius hangs out beyond the skin. The muscles being cut irregularly and obliquely, the arteries are not easily found, and their mouths are of the shape of a pen; whereas, in the other mode of operating, their mouths present directly and are easily found in the flat muscular substance. But all objections are of no consideration compared with the state of the nerves. They give less resistance, are drawn out before the edge of the knife, and hang from the face of the stump. I have seen it necessary to cut off a full inch and a half of the popliteal nerve from the face of the stump. I have been brought to reflect on this subject, because I have never seen so many cases of affection of nerves in the stump as in the last years of my attendance on the Middlesex Hospital, which I attributed to the mode of operating with the flap. I had long made it a particular injunction on my pupils, that when they amputated in the common way, they should on retracting the integuments, be careful to cut short the cutaneous nerves which run upon the exterior of the fascia, and not to leave them with the integuments; and this I did under the conviction, from experience, that those sensible cutaneous nerves being engaged in the cicatrix, gave rise to great distress. You will perceive that, in the flap operation, these cutaneous nerves are left long, projecting to the very edge of the integuments. In whatever way you amputate, these dissections" speaking of those contained in a paper of Mr. Langstaff's in the 16th vol. of Med. Chir. Trans., "as well as the sufferings we witness in patients, evince the necessity of burying the nerves deep in the stump. I have often referred to this subject at clinical lecture; for, unless the surgeon attend to this matter (and they do seem to think very little about it) the patient who has suffered amputation becomes an invalid,—a sort of barometer exhibiting the change of weather. After these observations, you will perceive how I consider the question to hinge as to the mode of operating. A clever operator will make a good stump either way." (pp. 330-331.)

Now, with respect to dressing, which, as is said, is of far more consequence than the mode of operating, Sir C. Bell advocates the old system "much dressing and careful rolling." And no doubt in discreet hands this will do well enough; though we confess, we like the simplicity of that employed by Mr. Liston.

In speaking of *Amputation of the Stump*, he says,

> "M. Maunoir, of Geneva, published a memoir in favour of the English mode of operating in amputation. In that essay he contrasts the method of which he is the advocate with that practised by Dupuytren, who, having divided the integuments and muscles together, took the farther means of ensuring a bad result by raising up the stump to the perpendicular; and this he did for a reason somewhat too ingenious,—that the blood might be impeded by the sudden angle of the artery at the groin and the ligature protected." (p. 336.)

We are not aware of the existence of any such reasons in the published lectures of Dupuytren. In justice to that great surgeon, we will give the reasons which influenced him in cutting "through the muscles and integuments together." In speaking of various modes of operating, he says, "of all these modes, some attain only very imperfectly the end proposed; others are difficult to execute; and, by modifications newly adopted, the sufferings of the patients are uselessly prolonged. In dividing at first the skin, in afterwards dissecting it, then in successively cutting the skin, the superficial, the propound muscles, and, lastly, the muscular fibres adherent to the bone; it is evident that the instrument is applied three or four times to the part, and the operation comparatively long and painful." This method, in the hands of Dupuytren, at the Hôtel Dieu, was certainly very successful. By a single incision, he cuts at once perpendicularly upon the bone. Sometimes, however, the incision was directed obliquely after the mode of Alanson; the retraction made by the assistant, and the contraction of the muscular fibres, give instantly to the stump the form of a projecting cone. It is to the base of this cone,—that is to say, at the level of the retracted muscles and integuments, that the cutting instrument is applied, and the whole of the projecting part is removed. In thus withdrawing the flesh as it is divided, and cutting the projecting mass, "the bone may be denuded to the extent of six inches." "By this method the operation is performed with an astonishing rapidity: the operator preserves as large a cushion as he wishes for the bone, and he spares the patient the acute suffering occasioned by the successive dissection of tiguments and muscles." Whether this method be preferable or not, it is only common justice to the dead to state fairly the reasons which induced him to propose this mode of operating. We have read through the article "Amputation" in the *Leçons Orales* of M. Dupuytren; we have been unable to find any directions for raising up the thigh to the perpendicular; for the "ingenious reasons" quoted, or indeed any other.

The revival of the *High Operation for Stone*, Sir C. Bell tells us, "was on the alleged necessity arising from the difficulty of extracting large stones from below." The operation was invented by Franco, it is true, under such a necessity, which occurred in cutting a child of two years, from whom he was unable to extract the stone through the perineum; but this was not the reason for its revival: it was believed that the bladder was more accessible, and that no important organs intervened; and, certainly, in the hands of Souberbielle, the operation has had great success. Neither, we think, is our author correct in assigning the reasons which are used to obviate the objections to this operation. "The incision," he says, "is made first in the perineum, in the usual manner, then a staff is introduced from below, and the fundus of the bladder pushed up above the pubis: here it is cut upon, and the stone extracted." Frère Cosme, no doubt, made such a proposal, but it was not long followed; it was energetically opposed by Scarpa; and is, so far as we know, completely abandoned.

Our author's opinion on the subject of *Aneurism* is similar to that of Scarpa, Hodgson, and others: nevertheless, we believe that we are justified in maintaining that the time is come for some modification of the opinion of Scarpa. There are now on record many well-observed cases, in which there has existed a dilatation or pouch occupying *only a portion of the circumference of the tube*, where the point of communication with the vessel is *somewhat constricted in the form of a neck*, and where *a large coagulum filled this pouch*, whilst the most perfect continuity of the internal tunic was demonstrated. Had Scarpa been aware that such cases were occasionally observed, his definition of aneurism would have been different.

We were hardly prepared to expect that, in the perusal of these volumes, we should have discovered a remedy for *tic;* yet so it would seem: our author states that, since he has exhibited a compound of one drop of croton oil and one drachm of compound colocynth pill, divided into twelve pills, one of which, with ten grains of galbanum pill, are to be taken at bedtime, "every true case of tic which has been presented to him has been cured." Sir Charles, we presume, does not mean that the curability of the disease by this medicine is to be the test of true tic: if so, we fear that the definition will not be very currently received. What is *true tic?* Is it a nervous pain dependent on torpor or accumulation in the bowels? Usually, we believe, it is not so; yet we apprehend this is our author's true tic. We have ourselves tried this remedy exactly as prescribed by Sir C. B., in what was certainly *tic*, but apparently not *true tic*, since the results obtained by us by no means corresponded with those obtained by him.

The author's opinion upon the nervous system has most deservedly great weight; yet we cannot quite accord with the opinion stated in the following sentence:

> "When a nerve is divided across, it cannot be perfectly restored; it unites, but a thousand chances are against its union—filament to filament. Sensibility is in part restored, but it is neither a perfect nor a natural sensibility which returns; a somewhat painful vibration is conveyed instead of the perfect sense of touch." (vol. ii. p. 107.)

It is very certain that union takes place upon the same principle and by the same means, in nervous as in other structures, and that the new tissue almost always, at the end of a period of time, varying with the tissue, acquires similar physical characters, and performs similar functions to the original structures. Arneman does not acknowledge that any regeneration takes place; and regards the substance which unites the nervous extremities as absolutely incapable of transmitting sensations; and this is equally denied by Breschet, by Richerand, and by Delpech. We apprehend they are in error. Fontana, after some time, examined the pneumogastric nerve of a rabbit, of which he had made a section, and found that the nervous filaments had no interruption. Michaelis examined nerves from which he had removed nine to twelve lines; and, by the aid of a microscope, could readily distinguish nervous filaments. Similar observations were made by Mayer; and he tested the truth by immersing the part in nitric acid, which produced no change. The observations of Cruickshank, Haighton, Prevost, and Swan are equally conclusive as to the recovery of structure and function. Tiedemann's researches into the recovery of function are equally satisfactory. Pring and Descot, Balfour, Peacock, Braid, Bailey, Houlton, and others, have given us conclusive illustrations of similar recovery of function in the human subject. Hunter, Fauchard, and Mouton, have left us instances of transplanted teeth acquiring sensibility. A large number of cases of tic might be cited, where a portion of nerve has been removed and the pain has ceased, but in a few weeks has again acquired all its former intensity. We do not, however, for one moment seek to maintain that the two portions unite filament to filament: in no tissue does this occur; but in all cases, we believe, the intervening or connecting structure acquires the character of those it connects, whether osseous, vascular, or nervous.

In the section on *Wounds of the Thorax*, it is said (vol. ii. p. 141,) "as to wounds of the heart and great vessels, I need not fill my pages with narratives; they are fatal wounds." Now this is not exactly the fact; and such an impression as is here conveyed should not be disseminated. The dictum of Hippocrates (*Aphor*. Sect. 6, No. 18,) is, that, without exception, wounds of the heart are necessarily mortal; it is also that of Celsus and of Paulus Egineta: yet a large number of cases are opposed to it. Wounds of the heart present a remarkable difference, dependent upon their situation, as well as upon the depth to which they have penetrated: if the cavities escape, the wound may excite only an inflammation, which may be subdued. Among many other examples furnished by comparative pathology is that related by Weber, where a musket-ball was found in the parietes of the heart of a stag; also one, contained in the Ed. Med. and Surg. Journal, (vol. v. 417,) in which a fat and vigorous doe was killed, in whose heart was found encysted a ball weighing 292 grains. In the human subject, among others, are the cases of Chabrol, Fourly, Richerand, and Latour: in one case, six years after the receipt of a gun-shot wound in the chest, the man died, and the ball was found in the right ventricle. We might mention many other cases, were it necessary.

In chap. viii., *Of Wounds in the Neck and Throat*, a curious anecdote is told, and is very properly held up to the student as affording a useful hint.

> "My predecessor in the Middlesex Hospital being under the hands of the barber, they began to talk of an attempt at suicide in the neighbourhood; on which the hospital surgeon called the wretched man a fool, and told the barber how he should have done it. The unfortunate barber retired into the back area, and cut his throat: there was no saving him!" (p. 149.)

An observation respecting the introduction of air into the blood-vessels would suggest some remarks; but we intend noticing this subject in another place.

We pass over chap. ix., *On Diseases of the Eye and the Operations performed on the Eye*, for the same reason.

Sir C. Bell seems to think that no satisfactory explanation can be given of the mode of formation of *loose cartilages in the knee-joint.* "It is sufficient to our present purpose, (he says,) to observe that they can be traced to a remote inflammation in the joint." Now, we think that the mode of their formation is satisfactorily made out, and that very commonly they cannot be traced to a remote inflammation. An opinion formerly entertained was, that they were masses of exfoliated articular cartilages. Loose cartilages, so formed, unquestionably have existed, but very unfrequently. Hunter believed them to be produced from extravasated blood which became organized and transformed. Russell thought they resulted from the condensation of a certain portion of synovia. There cannot, however, be a question that ordinarily they are differently formed: they are developed in the cellular tissue external to the synovial tunic; in the progress of development, they push before them that tunic and become pediculated; their pedicle becomes thinned, ruptured, and the body is at length free in the articulation. In Cruveilheir's *Anat. Path.* (Livraison 9, pl. 6,) there are many of these bodies represented in successive stages of development.

The last chapter of the work is on *Syphilis;* and opens with the following striking paragraph.

> "Is there any experienced senior of the profession, who, having a son of eighteen or twenty, and that son having a chancre, that would treat him without mercury? No! there is not such an unnatural person. This is our text, for to this the practical question should be brought." (p. 228.)

We have lately discussed the subject of the different modes of treating syphilis so fully (See Br. and For. Med. Rev., Vol. VI. Art. I.), that it is unnecessary to enter upon it here; but we cannot help remarking that here, as in many other parts of his work, Sir Charles Bell is disposed to be too dogmatical or too exclusive. In speaking of the cure of chancre, he says, "It is a matter proved that the primary sore will, in certain circumstances, and by treatment without mercury, heal; but it is as certain that, so treated, a sore-throat will follow." (p. 242.) Now, we think that the statements in the article referred to prove that this opinion, and this assertion, are too lightly advanced. There cannot, we apprehend, be a question in the present day that all the primary, and many secondary, symptoms of syphilis may be cured, and that, too, in a manner as complete as durable, without having recourse to any specific means, mercurial or other. It also has been proved that the secondary or constitutional symptoms occur, little if at all, more frequently than after the treatment by mercury; and it is doubtful whether these symptoms are ever so aggravated and obstinate. Indeed, the mode of treatment recommended by many of the most enlightened surgeons of the present day, is, first, to employ an antiphlogistic treatment for a reasonable time, and, if the sore remains obstinate, then to use mercury moderately.

But it is time to bring this article to a conclusion; and, in doing so, we must express our sincere regret that our task has been so much that of the critic. Our feelings towards the illustrious author would have led us to pass over defects and enumerate only excellencies; but the sterner duties of our office did not allow of this. Many things which we have criticised in him, we would have left unnoticed in the work of an inferior or unknown author. Sir Charles Bell's authority is potent for good or for evil; and it is our duty to use the humble means in our power to enhance the former and mitigate the latter. Without reference to former writings which have created the authority of which we speak, it is sufficiently manifest, from the work before us, that he who could write several of its chapters could have written all well, had he seen fit to use the exertion necessary for the purpose. It is a matter, not only of deep regret, but a probable source of serious evil, that this exertion has not been made.

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## ART. XIII.

*Experiments and Observations on the Gastric Juice and the Physiology of Digestion.* By WILLIAM BEAUMONT, M.D. Surgeon in the United States' Army. *Reprinted from the Plattsburgh Edition, with Notes, by* ANDREW COMBE, M.D. &c.—*Edinburgh*, 1838. 8vo. pp. 319.

We are very happy to welcome to this country a work which we cannot but regard as one of the most important practical contributions to the science of physiology which the growing attention to its pursuit has of late years elicited; important, not only in the bearing of the facts detailed in it upon the theoretical explanation of one of the most universal but least understood functions of the animal system, but also in the immediate applicability of the inferences directly deducible from them to the preservation of the human body in health, and its relief from some of the most distressing maladies to which it is subject. Although many of the most valuable of these inferences were embodied in Dr. Combe's excellent work on Digestion and Dietetics, yet we were still desirous that Dr. Beaumont's own experiments and observations should be laid before the English public; and, though we regret the delay which has occurred, we are not sorry that it has been the cause of their being introduced by an editor so judicious as Dr. Combe has shown himself to be.

Many of our readers are probably aware of the nature of the case which afforded the opportunity for the enquiries, the results of which we shall briefly sketch. Alexis St. Martin was a Canadian, of French descent, of good constitution, robust and healthy; and was in the service of the American Fur Company, as a *voyageur*. On the 6th of June, 1822, being then about eighteen years of age, he was accidentally wounded by the discharge of a musket; the contents of which, consisting of powder and duck-shot, he received in his left side, being at a distance of not more than a yard from the muzzle of the gun. The charge entered posteriorly, and in an oblique direction, forward and inward, literally blowing off integuments and muscles of the size of a man's hand, fracturing and carrying away the anterior half of the sixth rib, fracturing the fifth, lacerating the lower portion of the left lobe of the lungs, the diaphragm, and perforating the stomach. The whole mass of materials forced from the musket, together with fragments of clothing and pieces of fractured ribs were driven into the muscles and cavity of the chest. When first seen by Dr. Beaumont, about half an hour after the accident, a portion of the lung, as large as a turkey's egg, was found protruding through the external wound, lacerated and burnt; and immediately below this, another protrusion, which, on further examination, proved to be a portion of the stomach, lacerated through all its coats, and pouring out the food he had taken for his breakfast, through an orifice large enough to admit the fore-finger.

That the sufferer should ultimately recover from an injury of such severe character is not the least extraordinary part of the affair. Extensive sloughing of the integuments took place within a few days, very much enlarging the original wound; and a considerable portion of the lung was also thrown off. Large portions of the ribs also exfoliated; and after some months the cartilages of the five false ribs, and the ensiform cartilage of the sternum, came away in the matter of an abscess. In about a year from the time of the accident, the injured parts were all sound and firmly cicatriced, with the exception of an aperture in the stomach and side; this was about two inches and a half in circumference, and the food and drinks constantly exuded, unless prevented by a tent, compress, and bandage. St. Martin had so far recovered as to be able to walk about and do light work, enjoying his usual good appetite and digestion, and was rapidly regaining his health and strength. Some months afterwards, a small fold or doubling of the coats of the stomach appeared forming at the superior margin of the orifice, slightly protruding, and increasing till it filled the aperture, so as to supersede the necessity for the compress and bandage for retaining the contents of the stomach. This valvular formation adapted itself to the accidental orifice, so as completely to prevent the efflux of the gastric contents when the stomach was full, but was easily depressed with the finger. The only pain experienced by St. Martin arises from a deficiency of cuticle on a space about a line wide where the skin and mucous membrane approach each other; here the cutis and nervous papillæ are unprotected, and as sensible and irritable as a blistered surface abraded of the cuticle. He has subsequently married, and become the father of a family; he is active athletic, and vigorous, taking food, drink, and exercise like other people, and enjoying almost uninterrupted good health.

It is in these latter respects that the subject of the present case differs from others in whom similar openings into the stomach have existed. Thus, M. Hallé mentions an instance in which a malady existed which permitted the interior of the stomach to be seen; and the only observation that we find respecting it is, that at each entrance of the food into the cavity, the inner membranes of the œsophagus were partially everted, so as to form a prominent circular fold at the cardia.* Other cases have occurred in which the cavity of the stomach was laid open by external wounds; but in all of them death took place in a longer or shorter time, without affording any opportunity of observing the phenomena of really *healthy* digestion. It is fortunate for the interests of science, that the remarkable opportunities presented by St. Martin's case were offered to one so able and willing to avail himself of them as Dr. Beaumont has shown himself. For nearly eleven years after his accident, St. M. was kept under Dr. B.'s observation; during a large part of the time residing in his house as a servant, for the express purpose of being experimented on. Having at two intervals returned to Canada, he was again brought by Dr. B. under his cognizance; being on one of these occasions transported, with his wife and two children, a distance of nearly two thousand miles.† For the following judicious observations on the general character of Dr. Beaumont's investigations and conclusions, we gladly acknowledge ourselves indebted to Dr. Combe's preface.

> "That Dr. Beaumont eagerly and zealously availed himself of his unusual advantages, the following pages furnish ample evidence; and it would, I think, be difficult to point out any observer who excels him in devotion to truth, and freedom from the trammels of theory or prejudice. Among the disciplined physiologists of Europe, a more systematic experimenter might certainly have been found, but in Dr. Beaumont's instance, the absence of systematized enquiry—made too generally in support of a preconceived theory, and therefore apt to mislead as well as to instruct—is more than compensated by the implicit reliance which one feels can be placed on the accuracy and candour of his statements. Having no theory to support, and no favorite point to establish, Dr. Beaumont tells plainly what he saw, and leaves every one to draw his own inferences, or where he lays down conclusions, he does so with a degree of modesty and fairness of which few perhaps in his circumstances would have been capable. But, it may be said, singularly favorable as Dr. Beaumont's opportunities were, he has made no original discovery in the physiology of digestion. To a certain extent this is true; for, in the proper sense of the word, he has not made and does not claim to have made any discovery, but he has done what is at least equally essential for practical purposes. By separating the truth clearly and unequivocally from the numerous errors of fact and opinion with which it was mixed up, and thus converting into certainties points of doctrine in regard to which positive proofs were previously inaccessible, he has given to what was doubtful or imperfectly known, a fixed and positive value, which it never had before, and which, being once obtained, goes far to furnish us with a clear, connected, and consistent view of the general process and laws of diges-

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* Mayo's Physiology, 4th Ed. p. 115.

† "In proof of Dr. Beaumont's disinterestedness in conducting the enquiry, (says Dr. Combe,) I may mention that I have learned from private sources that the expenses attending the various series of experiments exceeded in amount 700*l.* sterling; the whole of which was defrayed by himself, and for repayment of which he was advised to apply to Congress, on the ground of the public being interested in the promotion of scientific discovery; but, although the American Treasury was at the time literally overflowing, the application was refused."

tion. Other physiologists have attempted to effect the same end by experiments performed upon the lower animals, but these are open to so many forcible objections, that we cannot always adopt their conclusions, even where they seem to be most clearly deduced. Not to mention the cruelty inseparable from the performance of such experiments, the pain which the animal suffers necessarily disturbs the regularity of the function under examination, and in a greater or less degree vitiates the results. And even if this were not the case, the difference between the digestive organs in man and in the lower animals is so great, that it would often be unsafe to assume conclusions as applicable to the former which have been verified only in the latter." (pp. vi.-viii.)

The first portion of Dr. Beaumont's work, entitled Preliminary Observations, contains a general view of the function of Digestion, in which are embodied several of the most interesting of the observations made by the author, with the practical and theoretical inferences to which they have led him. In the second division are found the Experiments and Observations themselves, which are unfortunately too deficient in systematized arrangement to enable us to offer to our readers their general results in a condensed form. We quite agree with Dr. Combe, however, in thinking that, "though this defect diminishes the facility of access to the results, it by no means detracts from their intrinsic value. On the contrary, the very absence of systematized arrangement leaves a character of even greater trustworthiness attached to the individual observations, than if the latter had been made under the influence of some prominent guiding principle, which might have given a bias to the mind." We must refer our readers, therefore, to the work itself, the perusal of which will amply repay them, for the details; and content ourselves with laying before them some of the observations and inferences which we deem most scientifically or practically important.

The following is the account given by Dr. B. of the appearance of the villous coat of the stomach in the living state, and of its functional changes:

"The inner coat of the stomach, in its natural and healthy state, is of a light or pale pink colour, varying in its hues, according to its full or empty state. It is of a soft or velvet-like appearance, and is constantly covered with a very thin, transparent, viscid mucus, lining the whole interior of the organ. Immediately beneath the mucous coat, and apparently incorporated with the villous membrane, appear small, spheroidal, or oval-shaped, glandular bodies, from which the mucous fluid appears to be secreted.

"By applying aliment, or other irritants, to the internal coat of the stomach, and observing the effect through a magnifying glass, innumerable minute lucid points, and very fine nervous or vascular papillæ, can be seen arising from the villous membrane, and protruding through the mucous coat, from which distils a pure, limpid, colourless, slightly viscid fluid. This *fluid*, thus excited, is invariably distinctly acid. The *mucus* of the stomach is less fluid, more viscid or albuminous, semi-opaque, sometimes a little saltish, and does not possess the slightest character of acidity. On applying the tongue to the mucous coat of the stomach, in its empty, unirritated state, no acid taste can be perceived. When food or other irritants have been applied to the villous membrane, and the gastric papillæ excited, the acid taste is immediately perceptible. These papillæ, I am convinced from observation, form a part of what are called by authors the villi of the stomach. Other vessels, perhaps absorbing as well as secretory, compose the remainder. That some portion of the villi forms the excretory ducts of the vessels, or glands, I have not the least doubt, from innumerable ocular examinations of the process of secretion of gastric juice. The invariable effect of applying aliment to the internal, but exposed, part of the gastric membrane, when in a healthy condition, has been the exudation of the solvent fluid from the above-mentioned papillæ.—Though the *apertures* of these vessels could not be seen, even with the assistance of the best microscopes that could be obtained, yet the points from which the fluid issued were clearly indicated by the gradual appearance of innumerable very fine lucid specks, rising through the transparent mucous coat, and seeming to burst, and discharge themselves upon the very points of the papillæ, diffusing a limpid, thin fluid over the whole interior gastric surface: this appearance is conspicuous only during alimentation or chymification. These lucid points, I have no doubt, are the termination of the excretory ducts of the gastric vessels or glands, though the closest and most accurate observation may never be able to discern their distinct apertures. . . .

"The gastric juice never appears to be accumulated in the cavity of the stomach while fasting; and is seldom, if ever, discharged from its proper secerning vessels, except when excited by the natural stimulus of aliment, mechanical irritation of tubes, or other excitants. When aliment is received, the juice is given out in exact proportion to its requirements for solution, except when more food has been taken than is necessary for the wants of the system." (pp. 94-96.)

The interesting character of the following observations induces us to quote them in full, notwithstanding their length.

"In disease, or partial derangement of the healthy function, this membrane presents various and essentially different appearances. In febrile diathesis, or predisposition, from whatever cause,—obstructed perspiration, undue excitement by stimulating liquors, overloading the stomach with food, fear, anger, or whatever depresses or disturbs the nervous system,—the villous coat becomes sometimes red and dry, at other times pale and moist, and loses its smooth and healthy appearance; the secretions become vitiated, greatly diminished, or entirely suppressed; the mucous coat scarcely perceptible; the follicles flat and flaccid, with secretions insufficient to protect the vascular and nervous papillæ from irritation.

"There are sometime found, on the internal coat of the stomach, eruptions or deep red pimples, not numerous, but distributed here and there upon the villous membrane, rising above the surface of the mucous coat. These are at first sharp-pointed and red, but frequently become filled with white purulent matter. At other times, irregular, circumscribed red patches, varying in size or extent from half an inch to an inch and a half in circumference, are found on the internal coat. These appear to be the effect of congestion in the minute blood-vessels of the stomach. There are also seen at times small aphthous crusts in connexion with these red patches. Abrasion of the lining membrane, like the rolling up of the mucous coat into small shreds or strings, leaving the papillæ bare for an indefinite space, is not an uncommon appearance. These diseased appearances, when very slight, do not always effect essentially the gastric apparatus. When considerable, and particularly when there are corresponding symptoms of disease,—as dryness of the mouth, thirst, accelerated pulse, &c.—*no gastric juice can be extracted, not even on the application of alimentary stimulus.* Drinks received are immediately absorbed or otherwise disposed of, none remaining in the stomach ten minutes after being swallowed. Food taken in this condition of the stomach remains undigested for twenty-four or forty-eight hours, or more, increasing the derangement of the whole alimentary canal, and aggravating the general symptoms of disease. After excessive eating or drinking, chymification is retarded; and, although the appetite be not always impaired at first, the fluids become acrid and sharp, excoriating the edges of the aperture, and almost invariably produce aphthous patches, and the other indications of a diseased state of the internal membrane mentioned above. Vitiated bile is also found in the stomach under these circumstances, and flocculi of mucus are much more abundant than in health. Whenever this morbid condition of the stomach occurs, with the usual accompanying symptoms of disease, there is generally a corresponding appearance of the tongue. When a healthy state of the stomach is restored, the tongue invariably becomes clean." (pp. 98-100.)

The dietetic principles founded upon these observations, and applicable both to the maintenance of health and the treatment of disease, are too obvious to require elucidation; but Dr. Combe's commentary is so apposite that we cannot refrain from adding it.

> "Many persons, who obviously live too freely, protest against the fact, because they feel no immediate inconvenience, either from the quantity of food or the stimulants in which they habitually indulge; or, in other words, because they experience no pain, sickness, or headach,—nothing, perhaps, except slight fulness and oppression, which soon go off. Observation, extended over a sufficient length of time, shows, however, that the conclusion drawn is entirely fallacious, and that the real amount of injury is not felt at the moment, merely because, for a wise purpose, nature has deprived us of any consciousness of either the existence or the state of the stomach during health. In accordance with this, Dr. Beaumont's experiments [observations] prove that extensive erythematic inflammation of the mucous coat of the stomach was of frequent occurrence in St. Martin after excesses in eating, and especially in drinking, even when no marked general symptom was present to indicate its existence. Occasionally febrile heat, nausea, headach, and thirst were complained of, but not always. Had St. Martin's stomach, and its inflamed patches, not been visible to the eye, he too might have pleaded that his temporary excesses did him no harm; but, when they presented themselves in such legible characters that Dr. Beaumont could not miss seeing them, argument and supposition were at an end, and the broad fact could not be denied." (p. 317.)

The examination of the characters of pure gastric juice was, of course, not omitted. This fluid was obtained by the introduction of an elastic tube into the stomach; the partial contact of which with its parietes excited the secretion from the points it stimulated. As these points bear but a small relation to the whole internal surface of the stomach, Dr. B. was never able to obtain in this manner more than one and a half or two ounces of the fluid at one time; and ten, fifteen, or more minutes were necessary to collect even this small quantity. Its extraction was generally attended by that peculiar sensation at the pit of the stomach termed sinking, with some degree of faintness, which rendered it necessary to stop the operation. The principal active ingredient contained in it appears to be muriatic acid, which exists free in sufficient quantity to produce a distinctly sour taste, and to throw down a large quantity of chloride of silver from a solution of the nitrate. From the experiments made with the fluid thus obtained upon various edible substances, Dr. Beaumont arrives at the conclusion, which appears to us unimpregnable, that chymification is essentially a chemical process, as maintained by Spallanzani; and that the solvent action of the gastric juice, aided by the *motions* of the stomach and the natural *warmth* of the system, is the essential agent in the operation. We need scarcely point out how completely this inference has been confirmed by the late experiments of Müller and Schwann upon artificial digestion; which was effected by means of a *manufactured* gastric juice.* We must say that *à priori* considerations alone would lead us to reject any other doctrine, unless supported by very strong evidence; for, as we are plainly to regard the stomach and alimentary canal but as an inversion of the external surface, contrived for adapting the function of absorption to the conditions of animal existence, there is no reason why substances contained in this cavity should become more *vitalized* than others applied to the skin, or than the air which is introduced into the lungs. Aliment is not really introduced into the system until the process of absorption commences; and with that process, we believe, the organization of the constituents of the fluid, and their endowment with vital properties, to commence. Dr. Beaumont found that the mixture of bile and pancreatic juices with chyme separated from it a fluid which he terms *crude chyle*, and which he regards as that taken up by the absorbents. He does not inform us, however, whether it contains globules or possesses the power of coagulating; both which are characteristics of true chyle.

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* See British and Foreign Med. Review, Vol. IV., p. 201; and Müller's Physiology, p. 545, &c.

The quantity of gastric juice secreted by the stomach for the solution of its contents does not depend upon the quantity of food introduced into the cavity, but upon the general requirements of the system. This is a principle of the highest importance in a practical view, and cannot be too constantly borne in mind. A definite proportion of aliment only can be perfectly digested in a given quantity of the fluid, the action of which, like other chemical operations, ceases after having been exercised on a fixed and definite amount of matter.

> "When the juice becomes saturated, it refuses to dissolve more; and, if an excess of food have been taken, the residue remains in the stomach, or passes into the bowels in a crude state, and frequently becomes a source of nervous irritation, pain, and disease for a long time; or until the *vis medicatrix naturæ*[*] restores the vessels of this viscus to their natural and healthy actions, either with or without the aid of medicine." . . . "Derangement of the digestive organs, slight febrile excitement, fright, or any sudden affection of the passions, causes material alterations in its appearance. Overburthening the stomach produces acidity and rancidity in this organ, and retards the solvent action of the gastric juice. General febrile irritation seems entirely to suspend its secretion into the gastric cavity, and renders the villous coat dry, red, and irritable. Under such circumstances it will not respond to the call of alimentary stimulus. Fear and anger check its secretion also; the latter causes an influx of bile into the stomach, which impairs its solvent properties." (p. 77.)

It would seem, then, that the connexion between the angry passions and the flow of bile, recognized by the term *choler*, is not so fabulous as some have imagined.

There cannot be a stronger argument as to the necessity of a careful regulation of the ingesta for the preservation of health than that derived from the relation between the quantity of gastric juice secreted and the requirements of the system, above alluded to. If a temporary increase in the quantity of this fluid were all the disorder occasioned by an excess in the quantity of food, the mischief would pass entirely unnoticed until the diminished power of the secreting organs prevented them from obeying the call. But we find the fact really to be, that, if a more than ordinary quantity of food be taken, and there is no demand for it in the system, part of it is left undissolved in the stomach, and thus indigestion is produced.

> "But if the ingestion of a large quantity be in proportion to the calls of nature, which sometimes happens after an unusual abstinence, it is probable that more than the usual supply of gastric juice is furnished; in which case the apparent excess is in exact ratio to the requirements of the economy, and never fails to produce a sense of quiescent gratification and healthful enjoyment. A great deal depends upon habit in

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[*] We must hint to Dr. Beaumont that this phrase is not very dissimilar to one which he quotes with disapprobation from another writer, "the foresight of the vital principle," as meaning everything or nothing.—REV.

this respect. Our western Indians, who frequently undergo long abstinences from food, eat enormous quantities, when they can procure it, with impunity."

"There appears to be a sense of perfect intelligence conveyed from the stomach to the encephalic centre, which, in health, invariably dictates what quantity of aliment (responding to the sense of hunger, and its due satisfaction,) is naturally required for the purposes of life; and which, if noticed and properly attended to, would prove the most salutary monitor of health, and effectual preventive of, and restorative from, disease. It is not the sense of satiety; for this is beyond the point of healthful indulgence, and is nature's earliest indication of an abuse and overburthen of her powers to replenish the system. It occurs immediately previous to this, and may be known by the pleasurable sensation of perfect satisfaction, ease, and quiescence of body and mind." (p. 55.)

We have not space to enter into the interesting description given by Dr. Beaumont of the motions of the stomach: we may mention, however, that there appears to be a complete revolution of its contents in periods of from one to three minutes, during the process of digestion, and that the whole contents of the stomach are thus mixed together, without any line of separation between the new and old food, as Dr. W. Philip supposed. As to the effect of these movements, Dr. B. remarks:

"The vermicular motions, being excited by mechanical irritation, not only carry the ingestæ into all parts of the stomach, and diffuse its mechanical influence throughout the whole inner surface of this organ, but by this means they uniformly mix the aliment with the gastric juice, which is constantly being secreted in proportion to the quantity of food received into the stomach, (unless that be too much for the wants of the economy,) until chymification be completed. Some stimulus seems to be necessary to continue the motions of the stomach after chymification is accomplished, in order to effect its complete discharge into the lower bowels: and it appears highly probable that the compound fluid of gastric juice and aliment, or chyme, by its acquired acid properties, affords this stimulus, and propagates the contractile motions of this organ, even after the mechanical irritation of the crude food ceases. This fluid acquires new chemical properties, becomes more acid and stimulating, as chymification advances, until it is completed. When it is all transferred to the duodenum, the motions of the stomach cease." (p. 82.)

With a few words respecting Dr. Beaumont's theory of hunger, we must close our account of the first part of his volume. The experiments and observations contained in the second have each an independent interest, and bear upon a number of minor points, to enumerate which alone would be tedious: we must therefore refer our readers to the work itself, with the assurance that they will find much in it to instruct and interest them. After explaining his grounds for withholding assent to any of the common opinions regarding the cause of the sensation of hunger, Dr. Beaumont says,

"My impression is, that the sensation of hunger is produced by a *distension* of the gastric vessels, or that apparatus, whether vascular or glandular, which secretes the gastric juice, and is believed to be the effect of repletion by this fluid. One reason, among others, for this belief is the established fact that the internal sensations referred to different organs, as has been previously alluded to, are caused by some modified action or condition of the parts in the tissues of the organ itself. The modification in the parts to which the sense of hunger is invariably referred, I conceive to be a distension, by the gastric juice, of a particular set of vessels or glands, constituting in part the erectile tissue of the villous coat of the stomach. The sensation varies according to the different degrees or states of distension, from the simplest desire to the most painful sense of hunger; and is allayed or increased in proportion to the application or refusal of alimentary stimulus to the excretory vessels." (p. 47.)

He grounds his belief that the fluid is secreted and stored up in the vessels, on the suddenness of its appearance when called for by irritation of the mucous surface of the stomach; but, as Dr. Combe justly remarks, "when we remember the equal rapidity with which saliva flows into the mouth of a hungry man, when a good roast of meat is placed before him, we shall be disposed to question the fact; unless, indeed, we hold that the saliva also was stored up in its vessels ready for use. Besides, bad news cannot instantly empty the gastric vessels of their contents, and yet they dispel appetite most effectually." Moreover, we feel by no means inclined to agree with Dr. B.'s general proposition, that pain and uneasiness always arise from distended vessels. Dr. Combe regards the sensation of hunger as arising from "a certain condition of the stomachic nerves, arising out of their relation to the state of the general system and to the brain;" this state of the system causing the stomachic nerves to excite a feeling of hunger, in the same manner as atmospherical vibrations, impinging upon the nerves of hearing, produce the feeling of sound. We cannot altogether satisfy ourselves with this explanation; since we are convinced that there is a more direct cause of the sensation to be looked for in the condition of the stomach itself, by the fact that mere distension of the viscus with innutrient substances will temporarily relieve hunger. As to the exact nature of this condition, we must confess ourselves in the dark; but we quite accord with the remark of Dr. Alison, that, "whatever be the conditions under which the nerves of the stomach become the seat of these sensations, it is certain that, in the healthy state, they are a true index, not only to the state of the stomach, but to the immediate wants of the system at large." We are ourselves inclined to attribute the feeling to the determination of blood to the organ, which must be the preparatory step to the copious effusion of the gastric fluid; and we are by no means certain that the phenomenon of the digestion of the coats of the stomach and of the surrounding viscera after death* does not show (notwithstanding Dr. Combe's argument) that the gastric juice may be stored up in cavities of the tissue between the time of its secretion and that of its discharge. At this conclusion it seems necessary to arrive, unless we are disposed to allow that the secretion is formed after death.

There is still required a systematic and carefully conducted series of experiments, for the purpose of ascertaining the relative digestibility of different kinds of food. It is obvious that the time required for artificial solution of various aliments in gastric juice cannot be regarded as a sufficient indication; since it may be readily imagined that the consistence of one kind of solid may be of such a nature as to require the peculiar movements of the stomach for the separation of its particles, and that it may thus appear, if tested in this manner only, less digestible than substances which more readily dissolve. Moreover, it is very difficult to extract gastric juice by the methods formerly practised, with any certainty of its purity, and of its having the same powers at one time as at another. The case of St. Martin, therefore, affords us a most valuable opportunity for this important investigation. Dr. Beaumont's chief aim was to ascertain the nature and laws of the digestive process; and his observations upon the comparative digestibility of different substances were thus too incidental to be relied on as minutely accurate, though in a general way worthy of attention. The table, therefore, in which the results which bear upon this subject have been thrown together cannot be regarded as exhibiting certainties, but only approximations to truth. The rapidity of digestion is so much influenced by the quantity eaten, the degree of preparatory mastication, the amount of exercise, the mode of life, and state of health, that no *positive* conclusions on this point can be drawn, except where due attention has been paid to all these modifying circumstances. We have much pleasure, therefore, in doing our best to draw attention to the following suggestion of Dr. Combe's:

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* This digestion is effected by the gastric juice, not only in carnivorous and in omnivorous, but in herbivorous animals. We believe it is a rare thing to find a prepared calf's stomach (such as is used for rennet) without a perforation of this kind.

> "In the second edition of the 'Physiology of Digestion,' I ventured to suggest that some of our scientific associations, such as the Royal Society or British Association, would do science a service and themselves an honour, by using their influence and means to have St. Martin brought over to this country, and the remainder of the subject fully investigated under the direction of a committee of their number. An opportunity of this kind may never occur again, and it will be a source of lasting regret, and even of merited reproach, if it be allowed to pass away without being turned to the best possible account. If the suggestion now thrown out shall ever be acted upon, special care should be taken not to injure St. Martin's health, by withdrawing him entirely from his accustomed diet and mode of life; otherwise, the whole value of the experiment may be lost,—the object being to ascertain the laws and conditions of HEALTHY DIGESTION." (*Pref.* p. xiv.)

We cannot but hope that, if the object be taken up by the influential members of our profession, and properly brought forwards at the ensuing meeting of the British Association, it may be effectually accomplished. We are sure that there is not an enlightened member in the higher ranks of the profession who would not gladly contribute a small subscription, for a limited number of years, to aid any of our public bodies who might, in the first instance, advance a sufficient sum from their funds to get St. Martin conveyed to this country. In the event of this laudable design being carried into effect, we know no individual to whom the task of superintending the experiments could be confided with greater confidence than to the accomplished and honorable editor of Dr. Beaumont's book.

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## ART. XIV.

*Du Lait, et en particulier de celui des Nourrices, considéré sous le rapport de ses bonnes et de ses mauvaises Qualités nutritives, et de ses Alterations.* Par le Dr. AL. DONNÉ, &c.—*Paris*, 1837. 8vo. pp. 66.
*Considerations on Milk, and particularly that of Nurses, more especially in regard to its good or bad Nutritive Qualities, and the Changes to which it is subject.* By Dr. AL. DONNÉ.

IT is much to be desired that, when all the solids and fluids of the body, healthy and diseased, have been determined to consist of certain globules of various size, shape, and consistence, some useful information may be associated with such discovery. Hitherto we can say little more than that observers have been contented with stating what they have seen; some of them having acquired such amount of credit as belongs to their possessing a good microscope and an acute sense of sight: others, less fortunate, having either described forms for which they have been indebted to fertile imaginations, or such as were the distorted images of bad glasses or equally bad eyes. M. Donné has been some years known to the profession, for his minute enquiries on different physiological subjects, and has published essays on Saliva, Mucus, and the Spermatic Animalculæ. We notice preferably the present paper, because it is not only the latest, but that which bears most directly on practical medicine. There are certainly both remarks and facts in it which appear to be worthy of our attention. The condition of human milk in relation to the health of children has not been sufficiently attended to. When the apparently hopeless states of disease from which children are sometimes recovered by a change of milk are remembered, as well as the uncertainty as to the nature of these morbid conditions, it becomes a question, well worthy of an attempt at solution, whether there is any perceptible quality of the milk by which these may be explained. It cannot be said that the essay of Dr. Donné is very satisfactory on this point, but it is a beginning of the enquiry, and contains hints which may be easily adopted by others who are willing to examine the subject for themselves. We have satisfied ourselves, by examination, of the correctness of his description of milk in a mature state; and, taking this as a standard, the detection of deviations cannot be difficult, and may be in some instances very useful and important. Dr. Donné remarks, that inferences as to the suitableness of milk from the healthy or unhealthy appearance of the female are very fallacious; and that the external appearance of the fluid is often very good, when the microscope detects a morbid condition; so that, without this mode of examination, our knowledge of milk must necessarily be very imperfect. In addition to the microscope, Dr. Donné employs a few very simple reagents: these we shall mention in the course of our analysis. The essay chiefly treats of the milk of women in reference to the nourishment of children, and to this part we shall limit our notice.

The first chapter is devoted to a *Microscopical Examination of Milk.* It has been long known that milk contained globules. In their normal condition, these globules are of various sizes, perfectly spherical, with black and regular borders; and they swim freely in a fluid in which no other particles are suspended. The composition of milk may be thus stated:—A liquid, holding in solution sugar of milk, salts, a small quantity of fatty matter and caseum, and suspending globules of different sizes, consisting of butter, and soluble in ether. If some cow's milk or ass's milk be filtered, and the liquid which passes the filter be examined, scarcely any globules will be found, and such as do exist are extremely small. This fluid, however, contains a large quantity of caseum. The globules remain in the filter with the cream, which appears to consist almost exclusively of them. If this cream is agitated in a tube with ether, the globules are completely dissolved. Hence it follows that the globules belong to the fatty constituent of the milk. Concentrated ammonia has no effect on these globules, and caustic solutions of potass and soda act upon them with great difficulty, and only after a considerable time. At the end of twenty-four hours, the majority of the globules still exist in a solution of potass or of ammonia, and the solution requires to be heated to act upon some of them.

The second chapter notices the first kind of alteration in milk; *its Mucous State, and the Persistence of this Fluid in the Condition of Colostrum.* The author has examined human milk previous to delivery, at the time of delivery, and during the changes which occur in it, until it has arrived at its perfect condition. It is known as a general fact that the early milk differs from that which is subsequently formed, and that its qualities are adapted to the necessities of the infant. The more evident qualities of the colostrum are as follows: it is a yellowish fluid; consisting of two parts, one serous, the other viscid; the consistence of the latter is that of a syrup. If allowed to stand in a vessel, a layer of considerable thickness is formed on its surface, of a yellowish colour more or less deep, which is the cream containing a large quantity of butter; if this is taken off, and the fluid is heated, a second, and even a third, is formed. The same phenomena occur during the three days which succeed delivery; with this difference, that the layer becomes less and less thick the nearer to the fourth day after delivery (p. 20;) but, if the colostrum is examined by the microscope, it is found to possess other characteristics, much more marked, and more worthy of consideration.

We have already mentioned the appearance of perfect milk. The colostrum is very different. It contains some real milk globules, but they are irregular and disproportioned; some of them appear like large oleaginous drops, and cannot be termed globules: this is evidently the imperfectly elaborated butter; that which rises to the surface of the colostrum and forms the yellow layer. The majority of the other globules in the colostrum are very small, and look like dust in the midst of the fluid: these globules, instead of swimming separately, are mostly connected together by a viscid matter; so that, when they are moved about over a glass plate, they separate in small agglomerated masses, instead of rolling one over the other, as is the case in perfect milk. In addition to this, the colostrum contains particles which have no relation with the common globules of milk: some of these are very small, their diameter being about one hundredth of a millimêtre; others are many times larger. These are but slightly transparent, of a yellowish colour and granular appearance; they seem to be composed of a multitude of small grains, connected together or enclosed in a transparent envelope; and frequently there is found within one of these little masses a true globule of milk. Dr. Donné imagines that these *granular bodies* (as he terms them) may consist of a fatty matter and peculiar mucus. They are not soluble in alkalies, but, as is the case with the true lactic globules, they disappear in ether; and after the evaporation of this solvent, small acicular crystals remain upon the glass.

Of the above description, the author has given, in a plate, representations with which it very accurately accords.

This condition of the milk continues, almost unchanged, to the end of the milk fever: then the liquid gradually changes; the number of granular bodies diminishes day by day; the lactic globules acquire a more regular and definite form, and, without being all of the same size, they differ less in this respect than was previously the case. At the same time these globules, previously united "*en masse*," and associated in a confused manner by means of a viscid substance, separate themselves, become isolated, and move in the fluid, quite independent of each other. These modifications do not always take place in the same period of time; but some traces of the primitive condition of the milk are appreciable twenty-four days after delivery, in very healthy women. This is the rule; to which there are exceptions, to be presently noticed.

It has been shown that pure milk does not become viscid when treated with concentrated ammonia: but ammonia renders the colostrum glairy, fibrous, and tenacious. This is a character common to the colostrum and to purulent matter. Both the colostrum and milk are alkaline: Dr. Donné has never found them otherwise. This is contrary to the observation of others; but, after repeated examinations, made expressly to determine the fact, and being aware of the contrary opinion expressed by others, the author can but state that, in his examinations, the red turnsol paper has uniformly become of a blue colour. The human milk, that of cows, asses, and goats, are all alkaline; and this is the case at all times of the year, and under all circumstances of food, season, &c. The opinion which has been formed of the acidity of milk, Dr. Donné considers as arising from its not having been tested immediately after having been drawn from the animal.

Sometimes milk retains the characteristics of colostrum beyond the ordinary period; occasionally for months, or during the whole time of suckling: and this milk, to common observation, appears as white, as consistent, as when healthy. The condition of the fluid can, consequently, be only ascertained by microscopic examination. Some milk remains in the state of colostrum a long while; and, in comparing this condition with that of the female by whom it was formed, she has been found very frequently lean and ill-nourished. The following is a case in illustration:—A young woman was confined with her second child, July 23, 1836; she was apparently very healthy. On the 1st of August, the milk was abundant and its aspect healthy, except that it was somewhat viscid. The child was quite healthy and well formed, but it frequently refused the breast without any appreciable cause. For twenty days after delivery, the milk remained in the condition of colostrum, as above described, but its colour was normal, its consistence as in the healthy state, and externally this milk appeared as healthy as that of the best nurses. Eighteen days after delivery, the child had diarrhœa; the milk did not change its character; and, twelve days subsequently, the child died, having gradually become emaciated. The former child by the same female died at the age of five months. Dr. Donné merely mentions this fact without wishing to infer any necessary connexion between the deaths of the children and the condition of the mother's milk: he regards it, however, as a fact which well deserves attention. The microscopic examinations which the author has made of the milk of asses and goats, during the state of pregnancy, after delivery, and until the milk has arrived at its mature condition, have shown a great analogy between these and that of women; with one exception, that the granular bodies belonging to the colostrum of woman are much less abundant in that of animals. These experiments also further confirmed the fact that the external appearance of milk is not a fair test of its actual qualities.

The third chapter is devoted to *Pathological Changes in Milk.* The mammæ, it is well known, become occasionally engorged during the period of suckling. Such a case is related where the engorgement occurred the eighth day after delivery. The milk presented the appearance of a number of globules connected together by a mucous substance; became viscid on the addition of ammonia, and contained a certain number of granular bodies. Similar changes were observed in another case, in which the breast became tumid and painful after delivery. From what has preceded, it is considered as established that the agglomeration of globules of milk and the presence of granular bodies, are the signs of milk which is either imperfectly formed or not of good quality. This modification takes place either in consequence of a lesion of the gland, or of a change of the lacteal secretion depending on a general disturbance of the health, as in the following case:—A young woman, eighteen years of age, eight days after delivery, was seized with colic and fever, which created the suspicion of metro-peritonitis. As long as this condition continued, the milk was full of granular bodies, and became viscid when ammonia was added to it. Two applications of leeches to the abdomen calmed the pain, and convalescence was established. From this moment the milk acquired its normal character. There was still some agglomeration among the globules, but no foreign bodies could any longer be seen.

But in addition to these changes in milk, it may be mixed with actual pus, and the most scrupulous attention might not discover the mixture: Dr. Donné has seen several instances of this. The globules of milk are perfectly spherical, transparent, and with black borders; those of pus are spotted, jagged, and opaque. Alkaline solutions completely dissolve the purulent globules, and leave the milky globules untouched. Ether dissolves the latter, but has no action on the former. From an abscess in the mammary gland, the pus may of course escape; and, in doing so, mix with the milk: but Dr. Donné has also found that the pus may become mixed with milk in the lactiferous vessels, and escape with it from the nipple; a fact of considerable importance where it is the common practice to encourage the infant in sucking a breast in which pus has been formed. In one case, where the fluid escaping from the nipple presented globules both of milk and pus, Dr. Donné correctly diagnosticated an abscess which was not previously detected. That the author has looked in vain for venereal matter in milk will not require the long explanation which he has given of the fact. In animals, Dr. Donné has found blood in milk, but he has never met with a similar case in women.

The last chapter considers the *proportion of the nutritive Elements of Milk;* and it is stated that the quantity of fatty matter in the same species of milk is generally in proportion to the quantity of the other solid elements of this fluid; so that the richness of milk may be inferred with tolerable accuracy from the number of globules which it contains. The milk of nurses may be in fault, as well from superabundance as from a deficiency of nutritious parts. The diameter of the globules appears to increase the longer the time since the delivery; but this sign is of no use to ascertain the age of milk.

## ART. XV.

*Practical Observations on the Preservation of Health, and the Prevention of Diseases: comprising the Author's Experience on the Disorders of Childhood and Old Age, on Scrofula, and on the Efficacy of Cathartic Medicines.* By SIR ANTHONY CARLISLE, F.R.S. President of the Royal College of Surgeons, and Surgeon to the Westminster Hospital.—*London*, 1838. 8vo. pp. xlvii. 154.

IT has pleased Sir Anthony Carlisle to present once more to the public some old observations, united with some new ones, in a handsome volume, much too dear, although embellished, like his former publication on Old Age, with a title-page in red letters and black. The work is dedicated, out of pure respect for senility, to the Dowager Lady Cork; and it is addressed, the author says, "to a class of readers unknown in preceding ages—to well-educated persons, who have discovered the deficiencies of classical learning in physic, and who begin to doubt the mysteries involved in dead languages." He apprehends that, in publishing this volume, he is sacrificing worldly professional interests to a sense of public duty. To guard, moreover, the unthinking from any suspicion of his want of "profundity" in consequence of this gay appearance in a popular work, he takes care to refer them to two out of his numerous former productions, the alleged Discovery of the Uses of the Spleen and Thyroid Gland, and the Physiological Observations on Glandular Structures; and to obviate a suspicion, which might arise, he conjectures, from the absence of quotations, that the author is unlearned, he declares that he "possesses and employs the established wisdom of preceding authors." It is not for us to be captious with so ardent a cultivator of the medical art; and therefore, although Sir Anthony is no longer young, we accept the red letters, the dedication to the elderly lady, and the profession of extra-profundity and all manner of learning, with the utmost possible good humour. We may regret to see how strongly, withal, Sir Anthony, is prepossessed against the double-columns of the classics of physic; and doubt his assertion that "the most skilful practitioners in physic have been generally trained in the schools of surgery;" but come in what shape they may, the remarks of a veteran observer, trained up under the eye of Hunter, and full of experience gathered in the great field of London practice, must have something valuable in them, and should be received with respect.

We shall therefore merely avow the surprise with which we have read forty-seven pages of Introductory Exposition, in which certain ideas are repeated about forty-seven times; the ideas being the very novel ones, that medicine and surgery are becoming less conjectural as they become improved, and that ignorant practitioners are rash, and unprincipled practitioners presuming, and experienced men cautious; and the conclusions being, that the best books are those "written by experienced men in advanced age," of whom the author says that he is one, and one who has "contributed lavishly" to render the profession respectable, to augment medical science, and to improve surgical apparatus.

After an interesting account of the topography of London, Sir Anthony Carlisle makes some useful, although not very new observations on its climate and atmosphere. The London air, when dry, is loaded with the dust of ashes and soot, and horse-dung, and gravel and granite. Dr. Watson (Bp. of Llandaff) found, if we remember correctly, that chopped hay formed a material ingredient. The result of this "chaos of eternal smoke" and dust, is some difficulty of breathing and cough, but chiefly in those newly arrived from the country. But there is also "a volatile corruption" of which the effects seem to be fevers which often attack the visitors to the metropolis. We believe that no town affords more instructive instances both of the immunity from and prevalence of malignant fevers; immunity from them in the wider streets and squares, and in the better ventilated houses, among the better nourished inhabitants; and their prevalence in courts and allies, and

"in many a sullen bay
That never felt the freshness of the breeze,"

where every cause of deteriorated health is accumulated. Within the prisons of the metropolis, also, it would seem, from the testimony of a patient of Sir Anthony Carlisle's, whose testimony was worth regarding, the mental apathy and recklessness which form so shocking a part of the scene those receptacles of misery display, are in a great measure the product of confinement in bad air. Yet the general freedom of London from all common nuisances, from bad smells and offensive sights, is one of the wonders of well-ordered communities; but a wonder effected by systematic attention and mechanical arrangements. Nor is it a circumstance indifferent to humane persons, that those whose lot in life it is almost to live in the vast drains and sewers of this city of nearly two millions, without which London would soon be the city of the dead, enjoy good health, soon recover from the accidental wounds or injuries they receive, and appear to suffer as little inconvenience as any other class of labourers. When all is done, however, that medical police can do, it is yet perhaps true that the human being, in these populous cities pent, gradually degenerates; that his offspring, as Sir Anthony Carlisle observes, become more and more feeble, and families become extinguished "through a progressive degeneracy." (p. 23.)

> "I believe," he observes in another page, "that no persons, town-bred in both the male and female lines, ever extend their children to the fourth generation; for they progressively dwindle, and lose the respective sexual characters until their procreative ability ceases." (p. 34.)

Four times the number of children perish, in the London air, of those who die when nursed in the air of the country. He goes on to express his belief that our great manufacturing towns give rise to "a degenerate, enfeebled, and demoralized race of town-bred citizens, who lack that happy union of a sound mind united to a sound body, which was held to be superlatively valuable by Roman statesmen." (p. 27.) There is some truth, perhaps with a small admixture of extravagance, in the following passages; touching upon different points of political economy:

> "If ever men," he says, "should become so far civilized as to cooperate faithfully for each other's good, a further advancement in knowledge might show them the justice of moving whole families from the progressive injury of city residences, from unwholesome localities, and even from certain climates to others more genial, calculated to remove the bad effects of permanent settlements; but these views are Utopian, and in utter opposition to those prevailing doctrines which, I fear, are undermining

the bodily constitution of a large portion of our countrymen, and, by sanctioning the unsocial vices of greediness and monopoly, are rapidly driving all Europe into a state of violence and confusion, which may ultimately stop the progress of every science, and (were it not for the indestructible extension of printed books) bring darkness and barbarism again over the civilized world.' (p. 27.)

"Without pretending to be a competent judge of the modern doctrines of political economy, I cannot agree to the cold-blooded restraints and punishments attempted to be fixed upon those who have the strongest animal passions, and to prohibit the most powerful of our race from sinking into selfishness, instead of being the proud and responsible parents of future generations." (p. 35.)

"Whatever the ultimate political or moral results of the practical working of the abstract doctrines for accumulating wealth, and for reducing population may be, it is obvious to a medical philosopher, that the moral and physical debasement of the English character must be the certain consequences; and however flattering the illusory dreamings of these selfish economists may be for a time, the reverie must terminate either in the extermination of a manly, virtuous, and industrious people, or in some dreadful insurrection." (p. 36.)

We discern the observation of the experienced metropolitan practitioner in Sir Anthony's graphic medical sketches of the different classes which compose London society: these will well repay the reader's perusal. The sketches are slight indeed, but expressive, and comprehend many of the causes of the diseases which assail the rich, strike the luxurious, or devastate the poor.

The remarks on diet and clothing are judicious, and such as few medical authorities would now be found to dissent from. Sir Anthony objects to giving much vegetable food, or even much bread, to young children; and reprobates the cusom of exposing town-bred children, especially, to the inconveniences of insufficient clothing. He considers calico preferable to linen, both for personal covering and for beds; and thinks a wash-leather waistcoat worn over a calico shirt is a better defence against cold and damp than flannel. He justly condemns the flimsy materials of ladies' shoes.

Scrofula, Sir Anthony maintains, is not hereditary; yet, as he allows that "this over-delicacy of the frame, which affords the aptitude to scrofulous disease, may be hereditary," we see nothing novel in his arguments on this point, except a more than usually anxious desire to gratify the self-love of parents, who are well satisfied in such cases if their doctors can only keep the word of promise to the ear, and assure them that the malady which disfigures or destroys their children is incidental, and not constitutional. Indeed, the whole chapter on scrofula is the most striking exemplification we have seen for a long time of that species of composition, not unfamiliar to reviewers, where what is true is not new, and what is new is not true. Of the truths we need not speak; of the novelties we can only find room for one or two small specimens.

*Consumption*. "The chest is narrow, and that is vulgarly and erroneously supposed to confine the lungs, and thereby to occasion consumption; which is a scrofulous malady dependent on repeated attacks of feeble inflammation, progressively spoiling the natural texture of the lungs, and, if not prevented, leading to an accumulation of incurably diseased structure." (p. 56.)

*Bronchocele*. "If it should be asked, what can have happened to produce that unnatural enlargement, I reply, that it is a mere dilatation of a congeries of blood-vessels, called the thyroid gland; and that it arises from long-continued exposure of the bare throat to a piercing, damp, and cold atmosphere, which physically impedes the flow of blood through vessels benumbed by a low temperature: similar effects follow chilling exposure of the lips, hands, and feet of feeble and delicate children, who are unjustly condemned to the opprobrium of scrofula." (p. 59.)

In the foregoing statement we have specimens of the author's anatomy and pathology; we will conclude with a still more luminous example of his physiology: but we would ask, first, in reference to the above, how happens it that bronchocele is so frequent in many countries between the tropics, in Asia, Africa, and America; and so rare in the arctic regions? or, to come nearer home, why is it unknown in Caithness and Sutherland, and so rife in the warm and blooming vallies of Hampshire and Sussex?

*General Physiology.* "Throughout animal nature, the circulation of red blood and nervous sensations keep pace with temperature; a general coincidence most remarkable in cold-blooded creatures, such as frogs and fishes, whose powers of motion, like those of our fingers, are benumbed by cold." (p. 60.)

We leave it to Dr. Edwards, Dr. Sharpey, Mr. Owen, and other cultivators of the higher physiology, to profit by and expound the wisdom wrapt up by the President of the Royal College of Surgeons in this dictum: we confess that it is above our mark.

In the same chapter, Sir Anthony confidently asserts that pulmonary consumption may *always* be prevented "by undeviating attentions to clothing, to local residence, to domestic accommodation, and to diet," (p. 63;) and he states that he has "often witnessed recoveries from some of the most direful ravages of scrofulous ulcerations penetrating the spine, the joints of the knees, ankles, elbows, and wrists, under all the disadvantages of London air, and confinement to the same room for more than two years, by the simple and rational adaptations of warmth, clothing, and diet, with very little medicine." (p. 69.) We may admit the fact in the latter part of the sentence; although we deny the truth of the opinion in the former. At the same time we are most ready to allow that the "attentions" referred to, can do a great deal more than is commonly believed, in such cases.

The section on the Preservation of the Health of Children is not very correctly designated, for it rather relates to the preservation of health in general. It is somewhat superficial, but characterized by good sense. Sir Anthony appears to set little value on many of the popular works published within the last few years on this subject; and is evidently little aware of the great quantity of useful information which some of them contain.

Twenty years having passed away since the publication of the first edition of the treatise on the Disorders of Old Age, it comes before many readers almost like a new work; and in this point of view its reputation will not be increased by its reappearance. The more experienced author is still the same enemy to morbid anatomy, and to the general attempts of his professional brethren to advance pathological knowledge that he always was. He speaks of their labours in a tone of vague depreciation, as mystical as absurd, and yet strenuously maintains that the science they cultivate is becoming exact, and ought to be protected by public opinion from the invasion of quacks and pretenders. With this strain every treatise in the book begins; with this strain every treatise ends. We can imagine the morbid anatomists turning round upon him on reading the very first sentence of his republished essay, and pronouncing it at once to be artificial and incorrect.

> "When the age of maturity has passed, and the lungs have escaped a derangement of structure most incident to youth, the common dangers to life are to be discovered in disorders of the head, the stomach, the bowels, the blood-vessels, and the liver; and they display themselves by apoplexy, palsy, indigestion, obstructions, inflammations, jaundice, or dropsy." (p. 89.)

Every morbid anatomist, and every practitioner *less* experienced than Sir Anthony, will be inclined to add a few affections to this catalogue, in which the thoracic organs are primarily, seriously, peculiarly, and often fatally affected in old persons. Not a syllable is said, here or elsewhere, of the great liability of old persons to organic changes in the heart and great vessels; facts which morbid anatomy has pretty well established. Such alterations are, perhaps, disregarded by Sir Anthony; for, among the very few new paragraphs in this edition, we find the following:

> "Diseases, constituted by alterations of organic structure, are seldom within the reach of physic, unless they invade parts of little consequence to life, when the administrations of surgery may prove useful. Too much attention has been empirically attracted towards anatomy, which is only one of the rudiments of medicine, and common to all well-educated practitioners. But the incurable ravages of disorganized parts afford few indications as to remedies, while they expose the most hopeless and lamentable obstacles to our art." (p. 93.)

To a want of regard for the progressive state of opinions and facts in medicine, similar to that manifested in these remarks, we can alone ascribe the assertion made at p. 126, that "a popular hypothesis is now very prevalent, which attributes nearly all diseases to a disturbed state of the liver," &c. &c. Sir Anthony said this, and truly enough, some twenty years ago; but, if he had considered the vascular, nervous, gastro-intestinal, and humoral theories, not to speak of others, which have occupied the attention of most other medical men in the mean time, he would scarcely have repeated the assertion in 1838.

Among the few practical observations now added we find the following:

> "There is an affection, not uncommon to old persons, which resembles apoplexy, arising from scantiness, and perhaps also from poverty, of blood. This is accompanied by faintings, the person having a slow feeble pulse; the faintings happen in the upright position, and after long standing up. Such persons have their lives prolonged by full diet, and the fit is removed by resorting to the horizontal position *instanter*. I think that this condition is often dependent on the habitual stimulus of wine, which, I believe, *thins* the blood, and fails to supply a permanent suitable volume of the circulating medium. Milk diet, and frequent supplies of animal broths, have, under my experience, forced life on for some years beyond the apprehended close." (p. 130.)

The observations on Cathartic Medicines contain nothing that particularly demands our attention; and, upon the whole, we cannot but observe, that the collection of the different treatises comprehended in this showy volume would have been more favorably looked upon, without question, if placed within reach of medical readers at about half the price. To men of moderate reading and experience nothing new is presented; and to the student, such luxuries of medical literature are necessarily postponed. The didactic method, too, has lost its charm; and the bulk of mankind, breathing an atmosphere of general knowledge, are little affected by the most pompous announcements.

# PART SECOND.

# Bibliographical Notices.

ART. I.—*The Hunterian Oration, delivered in the Theatre of the Royal College of Surgeons in London, on the 14th of February,* 1838. By BENJAMIN TRAVERS, F.R.S. &c.—*London,* 1838. 4to. pp. 42.

WE are not surprised that Mr. Travers should have felt and expressed the difficulty of composing a fresh eulogium on the character and merits of John Hunter. Three-and-twenty times has this tribute to his memory been paid; and every succeeding writer must necessarily experience increased embarrassment in the selection of topics upon which to descant. As John Hunter, however, was neither anatomist, physiologist, surgeon, nor naturalist *alone*, but the most remarkable combination of all these which the world has yet seen, the occasion presents opportunity for considerable variety of panegyric; and it is natural that each writer should choose a subject most congenial to his own taste. Mr. Travers, therefore, directs our attention in this Oration to the peculiar claims of Hunter to eminence as a surgeon; and these he justly regards as not so much based on his improvement of the *art* of surgery as on the vast progress which he caused it to make as a branch of the *science* of pathology.

"The Hunterian era, the most memorable in the annals of our science, dates little earlier than the commencement of the present century. Shakspeare's *Marc Antony* says,

> 'The evil that men do lives after them;
> The good is oft interred with their bones.'

If to Hunter any evil could be imputed, it was buried with him: his great achievements in the cause of our science, the proudest trophies of his fame, are posthumous. For, with the solitary but splendid exception of the operation for the cure of the popliteal aneurism, how imperfect a title would the most elaborate recital of his merits as a surgeon prove to the admiration and gratitude associated with his name.

"No; it is to his discoveries as an observer, sagacious, comprehensive, and profound, of the animal machine and its economy, in health and in disease,—his development of the phenomena which characterize inflammation in all textures, in its several aspects, stages, and processes, of the signs by which they are indicated, the laws by which they are governed, the consequences to which they lead, and the modes of treatment by which they are influenced and regulated, to subserve nature's and our purposes;—it is to these that we point with a national and honest pride, as to the column upon which are engraven in imperishable characters the surgical triumphs of John Hunter.

"He was not remarkable either for his skill as an operator, or his facility of communicating knowledge as a teacher. He was not a scholar; and neither the arrangement of his thoughts, nor the language in which they are clothed, is free from many and obvious exceptions. But the habitual character of his mind, which in the largest sense imparted itself to his works, was acutely observant, profoundly contemplative, capable of large and comprehensive views of natural phenomena, and endowed with a microscopic faculty of seizing and analyzing their constituent parts and bearings. Grant that his inductions did not always bear him out, that his combinations were sometimes inaccurately formed, and his announcement of general laws sometimes premature; yet where, in the calendar of time, shall we look for an equal in the compass, the variety, or the depth of his researches into the mysteries of animal life, or for consequences such as those that have resulted from his labours to universal pathology." (pp. 27-29.)

In order to demonstrate more clearly the actual improvements effected by Hunter, Mr. Travers has entered into an historical review of the progress of surgery from the time of Hippocrates, Galen, and Celsus, to the last century; and, were we inclined to be minutely critical, we might perhaps suggest that, with less of minute detail, the general ideas might have been more prominently brought forwards. We shall not, however, attempt to disparage the intrinsic excellence of the review; but shall conclude the present notice by directing the attention of our readers to another of those remarkable *anticipations* which the works of John Hunter are found to present, in proportion as the advance of science develops the ideas which appear to have almost intuitively suggested themselves to his extraordinary mind.

"Even of that sublime science of these our days, which connects the history of our planet with that of its extinct races, which has based their classification on the anatomical correspondence of uniformity of design, manifest in the organic remains of periods remotely antecedent to the creation of man and the existing types of mortality, it is indisputable that Hunter had a prophetic vision. For proof of this, I may refer, not only to his rich fossil collection, including at his decease about 1050 specimens, but to his interesting posthumous paper in the Philosophical Transactions for 1794, on the Fossil Bones found in the Caverns of the Principality of Bayreuth. In this paper he compares these specimens with their recent analogies, and shows that they differ both from them and among themselves.

"He alludes to the different climates and localities of the globe to which animals are more or less confined, or their geographical distribution, which, considered in relation to fossil remains, elucidates, by implication, the changes of temperature to which different parts of the earth have been subject at different periods.

"With more distinctness and detail, he points out the evidence which fossils afford of the alteration of the condition of the earth's surface, as dry land or submerged; and, by frequent allusion to the many thousand years which must have elapsed while the earth was the theatre of these changes, he seems to have fully appreciated the necessity of an ample allowance of past time to account philosophically for the changes in question. Mr. Clift, who transcribed the manuscript of this paper, informs me that it was originally dictated, not 'many thousand years,' but 'many thousand centuries;' and he preserves the copy of the letter from the friend of Hunter,* who advised the change of expression, in conformity with the popular notion of the world's age. Geologists now know that the latter expression is nearer the truth. I will only further remark, that, of three hypotheses which he suggests for the occurrence of the countless bony remains in the Gailenreuth caverns, he inclines to that which has been adopted by the distinguished Professor Buckland for the analogous collection in the caves of Kirkdale,—viz. that the caves were the habitual retreat of the extinct species while living; and that there they brought the carcases of their prey, and fed; and that there they came, and died.

"This interesting paper, if candidly perused, must, I think, be considered as the dawning of that glorious daylight with which fossil anatomy, the handmaid of geology, has since overspread the summits and penetrated the depths, and thus illumined the history of 'the earth, and of the waters under the earth.'" (pp. 33-35.)

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* The late Major Rennell.

We cannot but regard this anecdote of Hunter as deserving to be placed on record by the side of Newton's anticipation of the combustibility of the diamond, from its highly refractive power.

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## ART. II.—VELPEAU'S *Anatomy of Regions. Translated from the French.* By HENRY HANCOCK, Lecturer on Practical and Surgical Anatomy at the Westminster Hospital School of Medicine, &c.—*London*, 1838. 8vo. pp. 565.

THE object of the translator in adapting the original treatise to the English reader has been "to furnish the student with a complete work on the anatomy of regions, restrained within such limits as would make it available to all members of the profession." For this purpose, he has omitted the portion of the original which treats of general anatomy, and also the plates. The merits of M. Velpeau's work are too well known to require eulogy from us; but we may be permitted to doubt whether, by translating it in full, Mr. Hancock has presented to the English student exactly the book which he requires. There is in this, as in many of the French anatomical works, an unnecessary minuteness of detail, which is, in a practical view, not only superfluous but absolutely injurious. It is not to be expected that any one should retain in his memory all the multitude of insulated facts enumerated in the work before us; and we cannot help thinking that, in attempting to learn too much, the student is liable to have his mind distracted from topics of more real importance. No surgeon can be too well acquainted with the detailed anatomy of those regions which contain parts whose distribution and arrangement may affect his treatment of injury or disease; but, to map out the whole body by lines drawn from its external prominences, and to investigate the collocation of every organ and every tissue contained in each division, whether practically important or not, seems to us a labour which should not be imposed on any but those who are to make anatomy the study of their lives.

We cannot give Mr. Hancock by any means unqualified praise for the execution of the translation. Plain statements of facts cannot be too simple and clear; and we regret to say that, whether from following his original too closely, or from his own want of practice as a writer, Mr. H. not only makes occasional use of idiomatic phrases foreign to our language, but even falls into errors of construction and style. Thus, we find in the first page the following sentence: "increasing in thickness, as *it* [the skin] proceeds, posteriorly, from the ribs; *it* thus explains why furuncula and anthrax are so frequent on the back." And, in the next page, we have a short paragraph of five lines and a half containing seventeen commas, unrelieved by any other stop. On the whole, however, the book is a valuable one, and will be useful to many who are not masters of the original.

A concise description of the anatomy of the regions concerned in surgical operations or peculiar local diseases, illustrated by good but cheap delineations, and leaving out of view all but directly practical objects, seems to us still a desideratum in our medical literature.

ART. III.—*Medical Portrait Gallery. Biographical Memoirs of the most celebrated Physicians, Surgeons, &c. &c., who have contributed to the advancement of Medical Science.* By THOMAS JOSEPH PETTIGREW, F.R.S. &c. &c. &c.—*London and Paris,* 1838. 8vo. *In Monthly Parts; each Part containing three Portraits.* Parts I. II. and III.

HERE is a most attractive publication, which nothing can mar but the manner of the execution. Of the portraits, generally, we may speak very favorably; but of their arrangement we can say little in praise: so incongruous, strange, and fantastic is that admired disorder, and so wonderful the contempt of concatenation as well as of chronology, which brings together, in Part I., Æsculapius, Albinus, and Sir Henry Halford; in Part II., Ruysch, Haller, and Sir Anthony Carlisle; and, in Part III., Linacre, Akenside, and Sir Charles Clarke. There is a most unfortunate degree of absurdity in these unions.

We greatly lament the want of a more useful, or, we might say, of a more independent plan, in what must otherwise become a very popular work. A history of medicine, in a series of biographical memoirs, and illustrated by portraits, would be interesting and instructive; as would also a work so arranged as to present the cultivators of different branches of medicine, with reference to some order or other: but a publication, like the present, which, instead of this, seems especially devoted to the gratification of living vanity, cannot possibly have all the success which Mr. Pettigrew's friends must desire. It may please the flattered to read their glowing epitaphs whilst yet alive; but the public, who should be the purchasers, are not sufficiently sentimental to pay for verse or prose so dedicated. It may gratify Sir Henry Halford to read that he has shed splendour around the College, and zealously laboured towards placing the members of the profession "in that station in society to which their merits, their education, and their utility justly entitle them;" but the members of that profession will not easily acquit Mr. Pettigrew of covert satire in these expressions, which have the misfortune to be so framed as to imply neglect on the one hand, thus delicately reproved, or demerit on the other, thus held up to the scorn of the politer physicians of London. We must confess that a perusal of a few passages of this kind has very much disinclined us to look steadily through the notices. The pen of history should be the pen of truth; but in this work the life of every living man becomes an inflated panegyric. As a collection of portraits, however, the work will still possess much interest. The newspaper critics are, we see, in ecstasies with the publication; but we sincerely hope Mr. Pettigrew will be rewarded by more discriminate praise. It is but just to him to quote a part of his address to the subscribers, accompanying the last of the Numbers published, and accounting for what seems disorderly arrangement. As the work is but commencing, we yet entertain a hope that, in its progress, we shall be able to speak of it more according to our desire. Perhaps, with the same good intention, we may venture to say a few words on the classical taste of the mottoes, prefixed, without any particular necessity, to each life. To the section headed Æsculapius is attached the hacknied quotation from Cicero, "Homines ad Deos nullâ re," &c., with propius converted into *propriis*. Albinus is introduced by an epigraph from St. Augustine; and Sir Henry Halford is made to say, iu the stiff language of Akenside,

"Me they sent,
To wait on pain," &c. &c.

Ruysch has a word or two from Horace: "Utilium sagax rerum." The immortal Haller is trumpeted on as "Artis Medicæ decus." Each of these last two mottoes would have done as well for the one as for the other great man, and each wants that which a motto should have—a distinct appropriateness. We turned with some curiosity to look for the motto given to Sir Anthony Carlisle, having some suspicion that he was his own biographer: here, however, we find no motto at all. But the absurdity is revived with Linacre, for whom Milton is quoted: "Nomen in exemplum sero servabimus ævo;" a regular smallhand copy. For Akenside, of course, something grand must be sought, and Virgil contributes "Quæ tibi quæ tali reddam pro carmine dona!" This is just as if Akenside's poem were of an order capable of making us forget his harsh, pompous, and unamiable conduct and manners as a physician. Sir Charles Clarke, we are sure, will smile at the fine conceit wherewith his life begins: "Rite maturos aperire partus." We hope that the mere difficulty of finding mottoes liable to no equivoques and puns will put a stop to this puerility, which savours little of the learning that refines and ennobles, and rather belongs to the frivolous literature that has flourished with those who have vainly considered themselves the Corinthian capitals of physic.

We subjoin Mr. Pettigrew's explanations:

> "My object, I cannot but fear, has been imperfectly understood. I have not professed to write a regular history of medicine and surgery: my intention is, to give all the chief points of that history in a series of memoirs, taking for the subjects of these such individuals as have laboured most effectually in the advancement of any particular branch of medical science. But, as it would not be possible to obtain the BEST PORTRAITS, or materials, in the natural order of time, I have not felt myself bound to any specific method. The work is not consecutively paged; but each memoir is perfect in itself, and is separately numbered at the bottom of the page: the memoirs can therefore be arranged according to the fancy of the possessor of the work, either in the order of time or subject; or the LIVING can be separated altogether from the DEAD. In excuting such a work, I have conceived that I have been doing service to my profession. There exists no collection of medical portraits; there are many of the nobility, statesmen, divines, artists, &c. The portraits of medical men are scattered abroad, and there is some difficulty in obtaining information of the best: none but the BEST, however, shall be engraved, and as many originals as possible."

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ART. IV.—*First Lines of Physiology; designed for the use of Students of Medicine*. By DANIEL OLIVER, M.D., Professor of the Theory and Practice of Physic in Dartmouth College.—*Boston*, 1835. 8vo. pp. 520.

WE are happy to correct an error into which we fell when reviewing Dr. Dunglison's Physiology, (in stating that it was the only systematic treatise on the subject that had appeared in recent times on the other side of the Atlantic,) by noticing this volume, which has since been transmitted to us. Without the comprehensiveness of detail which Dr. D.'s work embraces, this treatise is evidently the product of no inferior research; and, although we do not agree with *all* the opinions expressed by the author in his brief but usually clear summaries of controverted questions, they are not advanced with that dogmatism which leaves the student in the belief that he is in complete possession of the subject, but rather encourage him to further enquiry by stimulating his curiosity and exciting an interest in it. The somewhat abstract style in which many parts of the volume are written, (in which respect it resembles Dr. Alison's Outlines more than any other English work on the science,) is better suited for the use of a student attending lectures in which general doctrines are explained and illustrated, than for the ordinary reader. It appears to have been Dr. Oliver's intention to effect this purpose; and we are disposed to think very favorably of the general accomplishment of it. The concluding chapter, on Animal Magnetism, is principally derived from the writings of Rostan and Georget; and we think that our author has done quite right in directing the attention of his young readers to a class of phenomena, which however explained, must be admitted to be so remarkable, and on which Laplace has thus expressed himself with the philosophic spirit which is the best consequence of high scientific attainments.

> "The singular effects which result from the extreme sensibility of the nerves in certain individuals has given birth to different opinions on the existence of a new agent, which has received the name of animal magnetism. It is natural to think that the action of these causes is very feeble, and may easily be disturbed by a great variety of accidental circumstances; so that, from the fact that, in many cases, this agent has failed to manifest itself we ought not to conclude that it never exists. We are so far from being acquainted with all the agents in nature, and their different modes of action, that it would be unphilosophical to deny the existence of phenomena, merely because, in the present state of our knowledge, they are inexplicable." (p. 513.)

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## ART. V.—*On the Improvement of Medicine. The Oration delivered before the Medical Society of London, at their Sixty-fifth Anniversary, March* 8, 1838. By THEOPHILUS THOMPSON, M.D., Physician to the Northern Dispensary, &c. *Published at the Request of the Society.* —*London*, 1838. 8vo. pp. 23.

WHATEVER share the improvement of medicine may have in the honour of producing such a result, the fact that the average duration of human life, in England and Wales, has considerably increased during the last half century, is one of very great importance. But there is reason to believe that medical science has really contributed to this end; the cure of some diseases, and the prevention of others, having been effected by its gradual advancement. As a proof of the advance made in the art of curing diseases, Dr. Thompson refers to the records of the puerperal hospitals; in one of which, in London, fifty years since, the average number of deaths was more than one in sixty; whereas, during the last fifty years, except during the prevalence of malignant fever, the mortality in such institutions has not been much more than one in three hundred. The effects of the prevention of disease are, of course, strongly illustrated by the great diminution of deaths from small-pox: the deaths now arising from that disease in London being about three hundred in a year, instead of five thousand. The banishment of the sea-scurvy is another of the legitimate triumphs of medicine. Dr. Thompson mentions, also, the improved treatment of the insane, and the greater number of recoveries from mental disorders; fewer of which are consequently fatal. The employment of the stethoscope; the diminution of surgical operations; and the greater attention paid to physical education, in consequence of the excellent works addressed to the public by physicians, are enumerated among the causes of the improved public health and more prolonged life. Dr. Thompson alludes to the investigations of living physiologists, as pointing to further useful results; especially to those of Dr. Todd, noticed in our Seventh number; and he makes an eloquent apology for homœopathic practice, without naming it, by observing (and we think justly,) that, "at the limits of visible anatomy, there lies another anatomy, whose phenomena are unrevealed;" and that "beneath the surface of our present physiology lie hid processes and laws, full of interest and wonders;" reasons, he thinks, for not turning contemptuously away from the asserted effects of minute agencies. The numerical method of Louis; the effects of the medical arrangements under the new Poor-Law Act; and the subject of medical education, are judiciously noticed in this oration; the language of which is throughout correct, spirited, and even elegant.

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Art. VI.—*Ueber die in der Belgischen Armee herrschende Augenkrankheit: als Commentar zu Professor Dr. J. C. Juengken's Schrift über denselben Gegenstand.*—Von Dr. Burkard Eble, k. k. Regimentsfeldarzte, &c. *Wien*, 1836.

*On the Ophthalmia in the Belgian Army: a Commentary on Professor J. C. Juengken's Memoir on the same subject.* By Dr. Burkard Eble, Regimental Physician in the Austrian Service.—4to. *Vienna*, 1836.

Dr. Eble informs us, that finding, in Juengken's memoir on the Ophthalmia of the Belgian Army (which appeared in 1834, and which has been already noticed in this Review, Vol. II. p. 501,) much that was more or less opposed to his own experience in the Austrian Army, he thought it would be doing a service to write a commentary on it, and to state clearly wherein they differed. For this labour, Dr. E. informs us he has been rewarded by King Leopold with a valuable diamond ring, bearing his majesty's cipher.

Professor Juengken entertains an opinion that inflammation is only a symptom of the affection of the eye, and that the real disease consists in the unusual development of the papillary body of the palpebral conjunctiva, in the form of granulations. This Dr. Eble vehemently protests against, and correctly asserts that the change in the papillary body of the conjunctiva is always preceded by inflammation, though very often so slight as not to attract the patient's attention. Dr. Eble thinks there is no specific difference between the high degree of catarrhal ophthalmia and this so-called contagious ophthalmia. Again, he considers that no kind of conjunctival inflammation is absolutely infectious, nor is the Egyptian ophthalmia, called contagions *per eminentiam*, unconditionally so. He admits, however, that all kinds of inflammation of the conjunctiva, without exception, may, under certain circumstances, become infectious. Dr. E., though he admits the possibility of infection *per contactum*, says he is compelled by experience to assert that infection, *in distans*, is more common; a circumstance, says he, which should be carefully kept in remembrance on the occasion of an epidemic. As to the *name* of the disease, he says *Egyptian* is a very incorrect epithet to characterize it; for it might with as much justice be called African, Italian, or Prussian, and, we might add, Irish. The name, contagious ophthalmia, is not more correct; for there are several other kinds of contagious ophthalmia. That in question belongs to the genus inflammation of the conjunctiva. As a species, it ranks next to catarrhal inflammation of the palpebral conjunctiva, which it resembles very much. In the opinion of Dr. E., the two are not identical, or only in so far that it must be confessed that the ophthalmia of the army more quickly seizes the papillary body. "I might almost say," says Dr. E., "that our disease has the same relation to common catarrhal inflammation of the palpebral conjunctiva as the *influenza* to common catarrh, or as dysentery among soldiers to sporadic dysentery." Dr. E. makes the following systematic arrangement of this disease: *Ordo*, Inflammatio; *Genus*, Inflammatio conjunctivæ palpebrarum; *Species*, Inflammatio conjunctivæ palpebrarum, sive Blepharophthalmia catarrhalis bellica sive militaris.

In regard to treatment: Juengken justly advocates free bloodletting; Eble is not disposed to go so far; but it must be remembered that Juengken's experience is derived from the disease as it occurred among northern people, that of Eble from among the southern. In regard to the use of local remedies, Dr. E. rightly remarks that it is of no great consequence what astringent or stimulant is employed; no one is specific; but much more depends on the strength or weakness of the stimulating action of the remedy. Dr. E. does not give any credit to scarification of the conjunctiva as a means of removing the granulated state, but considers it rather injurious. In opposition to this, we have very often found decided permanent benefit, and always temporary improvement, from scarifying each granulation by a small crucial incision, and immediately thereafter applying to the so-scarified conjunctiva some strong salve, such as the red precipitate. These observations of ours are drawn from the disease as it occurs endemic in the south of Ireland, particularly the city of Cork and its neighbourhood, where numbers of people are to be seen with eyes destroyed or materially injured from this scourge. Eble says, (p. 47,) "whoever, provided he be called in time, does not in most cases cure the disease in the first stage by means of venesection, leeches, calomel, potio laxans, acid drinks, then cold water or ice as applications to the eye, besides the necessary regimen, is a bungler of an eye-surgeon."

When taken in time, we have found the disease, as it occurs in Cork, generally yield to pretty free bloodletting, and that repeated according to circumstances; diaphoretics, such as Dover's powder, after the bleeding, and then laxatives: as local applications, drops of nitrate of silver solution, or red precipitate ointment, together with a weak solution of corrosive sublimate combined with a little wine of opium.

Art. VII.—*A New and Familar Treatise on the Structure of the Ear, and on Deafness.* By A. W. Webster, Inventor of the Otaphone, &c.—*London*, 1836. 8vo. pp. 151.

Mr. Webster sets out by expressing some expectation that his little treatise will place our knowledge of the organ of hearing one step in advance of its present position, particularly as regards its successful treatment: a consummation much to be desired. Let us see if his expectations or pretensions be realized.

After some preliminary harangue on the erroneous conceptions which, he says, are prevalent in regard to the structure of the ear, he adds, p. 6, "he has presumed to treat it altogether in a novel, but, he trusts, in no less a correct and scientific manner. He has avoided as much as possible all technical phraseology and minuteness of detail, feeling convinced that they indispose the mind from taking an enlarged view of the subject. He hopes by familiarity of language to improve the science, by enlisting in its support and elucidation all those whom the preceding modes of description have hitherto excluded." This announcement, if anything more than the title-page had been wanting, lets us at once into the secret of the nature of the book, and the conclusion which we thus draw regarding it is justified by all that follows. The book, in fact, belongs to the same class of works which make up the whole amount of English medical literature on the ear, and which we have exposed on more than one occasion in this Review. The treatises in question are intended, not to communicate any information, which, considering the ignorance of all scientific knowledge evinced by their authors, were impossible, but, the gullibility and ignorance of the public in those matters being duly calculated on, to lead to a false belief of their skill.

Each author, of course, has something of his own on which he more particularly founds his pretensions: *Mr. Webster is the inventor of the Otaphone!*

According to him, (p. 131,) "the invention called the Otaphone is constructed" to remedy derangement of hearing depending on the state of the auricle. "The instrument supports the depressed parts of the auricle, and thereby conveying more vibrations to the inner ear, derives its name from οτα (ota) the ear, and φωνη (phone) sound. It is formed of pure silver, doubly gilt, which, taking the exact shape of the back of the ear, when supported in its most capacious form, spreads, as it were, a sail for the collection of sonorous impressions." We give Mr. Webster's description of his instrument, and his derivation of its name in his own words, confessing we cannot see the precise object or use of the thing, or in what it greatly differs from what is known by the name of a Wellington ear; but, we may remark, he informs us, the invention is intended as a substitute for the supporting of the ear with the hand, which people are much in the habit of doing when wishing to catch the words of a speaker in large assemblies; therefore, continues Mr. W. (p. 132,) "the principle on which the otaphones act is so natural as to be almost instinctive." So much so that, he tells us, Dr. M'Diarmid, who attended the last polar expedition, informed him, that the only two *Boothians* who were deaf adopted this practice. From this it would appear the Boothians are as good practical aurists as Mr. Webster himself.

Besides the improvement of the hearing, and the genial warmth which, according to the author, the otaphones promote behind the ear, Mr. W., in the true spirit of a proprietor of a paltry patent invention, enumerates one other advantage which they possess, and what this advantage is, the author must himself be allowed to tell (p. 133): "Sometimes, when the ear is very rigid, as is often the case with men, *the Otaphones cause a small crack in the under surface! This should be hailed as an advantage.* Nature supplying the best medicament to heal the wound, which, when it closes, unites the auricle in a new and better position."

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ART. VIII.—*De Nervo Faciali.* Auctore Doctore MASSALIEN.—*Berolini*, 1836.

*Dissertation on the Facial Nerve.* By Dr. MASSALIEN.—*Berlin*, 1836. 4to. pp. 23.

THE author gives a succinct account of the anatomy, functions, and pathology of the facial nerve, chiefly compiled from the researches of Sir Charles Bell on the Nervous System. An interesting case is detailed at full length, of facial paralysis occurring in a scrofulous child from tubercles in the brain. The paralysis was confined to the left side. The patient died hectic and emaciated. On examination after death, three tubercles of various magnitude were found in the cerebral substance. One of them, situated in the cineritious portion of the left hemisphere, was as big as a walnut. The others were of the size of small hen's eggs. On cutting them across, the section presented an external stratified tissue with a radiated core. The former had a bright yellow hue; the colour of the latter was a dingy yellow. The tubercles possessed more than ordinary density. It is a remarkable fact, and one not heretofore demonstrated by microscopic observation, that their structure was composed, not of acini, as is commonly the case, but of fibres. On minute inspection, the mass resembled dried cheese. The two larger tubercles, above mentioned, occupied nearly the middle fossæ of the basis cranii, which are formed by the temporal and occipital bones. They were, in consequence, inserted between the surface of the middle lobes of the brain and the dura mater lining the median fossæ; they were intimately connected with the cortical substance, and invested by the meninges. Of these two tubercles the left deserves most attention, as corresponding with the paralytic side of the face. Of it, the tuberculous matter was evidently traceable into the cerebral lobe; filling the mastoid cells, and passing into the canal of Fallopius; in which latter situation the facial nerve was obviously compressed. The tuberculous substance accompanied the nerve till its exit at the stylo-mastoid foramen. The nerve there seemed somewhat swollen. The external lamina of the petrous bone was necrosed. The membrana tympani was destroyed, and the ossicula auditus, with the exception of the stapes, had perished. The size of the tumours, and their relative position to neighbouring parts, are well shown in an annexed plate of a vertical section of the brain, from a drawing by Dr. R. Froriep.

ART. IX.—*A Dissertation on the Causes and Effects of Disease, considered in Reference to the Moral Constitution of Man.* By HENRY CLARK BARLOW, M.D.—*Edinburgh*, 1837. 8vo. pp. 79.

THIS dissertation, which appears to have been written as an inaugural essay, presents a favorable example of the habits of mind generated in the Edinburgh school of medicine, in which the practical part of physic has long flourished in combination with such philosophical views as have found much less favour elsewhere. Dr. Henry Barlow has shown, with copious references to authorities and examples, that diseases arise, for the most part, from the neglect of moral and physical laws; and are not so much to be considered evils as remedies for evils. He has also laboured, not unsuccessfully, to show that the phenomena of disease are so regulated as to produce the intended end with as little inconvenience as could possibly be united with the attainment of such end; and that, even beyond the proposed object, certain results flow from them which are conducive to the benefit of mankind. Such a dissertation cannot but be viewed as a work of most favorable promise, exhibiting, as it does, marks of extensive reading, just reflection, and elevated views.

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ART. X.—*Die vergleichende Osteologie des Schläfenbeins. Zur vereinfachung der herschenden Ansichten bearbeitet.* Von ED. HALLMANN. Mit iv. Kupfertafeln.—*Hannover*, 1837.

*The Comparative Anatomy of the Temporal Bone, and simplification of the prevailing views regarding it.* By ED. HALLMANN.—*Hanover*, 1837.—4to. pp. 130; with iv. Plates.

THIS is a work highly creditable to the industry and knowledge of Mr. Hallmann. His object, in composing it, has been to determine what constitutes the temporal bone in the vertebrate series; to reject the common idea of *a general plan*, which presupposes that a certain bone, or part of it, must necessarily be found in every class; that no piece will be found in one class for which there is not an analogous piece in another. All that is permanent, according to him, in the head is a cranial and a maxillary part. From his observations, he has been led to establish the following law for the development of the head:—" In their development, the skull and jaw proceed in opposite proportions through the series of vertebrate animals from man to fishes." In an appendix, he adduces confirmations of his views from the observations on development by Baer and Rathke, but recants one opinion which he had expressed in the former part of his work, viz. that the petrous bone belongs to the visceral system, and not to the class of cranial bones. An acquaintance, however, with Baer's observations regarding the development of the eye, the ear, and the nose, has led him to view the petrous bone as belonging to the cranium, and the tympanum to the jaw; that the fibrous bones are lateral parts of the cranium, but lateral parts without any proper vertebral body.

ART. XI.—1. *Memoranda on difficult subjects in Anatomy and Surgery, being a Pocket Companion for Students preparing for the College of Surgeons.* By ROBERT DRUITT, M.R.C.S.L.—*London*, 1837. 48mo. pp. 184.

2. *The Student's Companion to Apothecaries' Hall; or the London Pharmacopœia of* 1836, *in Question and Answer.* By E. OLIVER, M.R.C.S.—*London*, 1837. 48mo.

3. *Ophthalmic Memoranda, respecting those diseases of the Eye which are more frequently met with in Practice.* By JOHN FOOTE, F.R.C.S.—*London*, 1838. 48mo. pp. 90.

4. *Libamenta Praxeos Medicæ, or a Manual of the Practice of Medicine, for the Use of Students and Junior Practitioners.* By D. SPILLAN, M.D.—*London*, 1838. 48mo. pp. 192.

5. *A Collection of Medical Formulæ, selected from the Writings of the most Eminent Physicians.* By D. SPILLAN, M.D.—*London*, 1838. 48mo. pp. 111.

WE transcribe the preceding titles, not to attract attention to the literary abortions to which they are prefixed, but to warn all and sundry our younger readers who might be seduced by their exterior, to eschew them, as beneath the notice of every honorable student, who is conscious of a desire to study medicine in the spirit which so noble a profession should call forth. Notwithstanding the suspicious circumstance of the almost simultaneous appearance of these Lilliputian manuals for Lilliputian minds, we cannot bring ourselves for a moment to swerve from the opinion which we have gladly cherished, that our London students are progressively improving in their general character of attention to their studies, and in their disposition to learn medicine and surgery thoroughly and soundly. We are inclined to attribute the sudden outbreak of this epidemic of infinitesimal manuals and guides, rather to the teeming fancy of some hopeful publisher, than to the actual demand for such ware in the present state of the medical market. We strongly deprecate all such publications: they pander to the depraved taste of the ignorant and the idle, while their very existence is a sort of slander on the industrious and well-informed. The sole good we anticipate from them is that, by attracting the notice of the examiners at the different colleges, halls, and boards, they may induce these gentlemen to make their examinations still more searching and complete, and thus sift the honest students, the men of real knowledge, from the disgraceful fry—if there are indeed such—who have been stuffed for the nonce by Grinders and Guide-books.

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ART. XII.—*A Series of Anatomical Sketches and Diagrams, with Descriptions and References.* By THOMAS WORMALD, Assistant Surgeon and Demonstrator of Anatomy in St. Bartholomew's Hospital; and M. M'WHINNIE, Teacher of Practical Anatomy at St. Bartholomew's Hospital.—*London*, 1838. Part I. 4to. pp. 15.

THE design of these sketches and diagrams is explained in a brief prefatory notice, in which the authors remark, that "their experience has taught them how much the studies of their pupils are facilitated, and their memories refreshed by sketches taken from nature, or by diagrams made extemporaneously." The objects proposed are meritorious, and the execution of the sketches which compose the present number are remarkable for their correctness, perspicuity, and neatness of execution. We do not, however, quite coincide with the authors in believing that they will *permanently* benefit the student by assistance like the present. Our own experience in these matters disinclines us to favour the system of rendering the acquirement of anatomy too simple, or of affording assistance which may in any way tend to diminish the necessity of actual dissection. The retention, rather than the easy attainment of practical information, ought to be the ulterior object to be constantly kept in view. This remark is more or less applicable to the study of our profession generally, and indeed to the acquirement of every sort of information. Where knowledge is presented to the enquirer, as it were ready-made, or by wholesale, he rarely either appreciates it so thoroughly or retains it so long, as if the gaining of it had cost him more time and labour; and certainly the intellect loses that wholesome exercise which ought to be a main object in all mental training. We have ourselves found very considerable advantage in extemporaneous sketches for the purpose of illustration; and would greatly encourage the practice of copying such into their note-books, by the students: but this latter practice we regard as one of the principal objects to be attained by their employment. All the assistance of this kind required by the pupil, may be derived from large and boldly painted diagrams of regions, the relative anatomy of which is important in a surgical point of view. If the undertaking of Messrs. W. and M. should grow into a more systematic form as it increases in bulk, we feel satisfied that they will contribute a very valuable work of *reference* to the practitioner as well as the student.

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## Art. XIII.—*A Treatise on Hernia: comprising the Surgical Anatomy, Operative Surgery, and Treatment.* By M. W. Hilles.—*London,* 1838. 12mo. pp. 52.

We regard this little treatise as a correct epitome of the Anatomy and Operative Surgery of Hernia, and as likely to be useful. So much is here condensed into a narrow compass, than we can scarcely recollect anything of anatomical interest or practical nature connected with the subject, that Mr. Hilles has not culled, and interwoven in his pages. Nothing original is, we presume, affected by our author, if we except a reference to what is styled "a newly proposed operation for strangulated hernia," and which consists merely in applying the taxis to the sac after it is laid bare, without opening into it, and to which we formerly adverted, (Vol. IV. p. 256.) To compile, however, with brevity and yet with judgment is in itself a merit, and entitles our author to the attention of the surgical student; who, so far as hernia is concerned, will find in this little pamphlet a useful companion in the dissecting room, both in refreshing his memory, and adding to its store.

## Art. XIV.—*Handbuch der Menschlichen Anatomie.* Von C. F. T. Krause, m.d., Professor der Anatomie; an Ersten Bandes, dritte Abtheilung.—*Hannover*, 1838. 8vo.

*Manual of Human Anatomy, &c.* By Dr. Krause.

We noticed with approbation the preceding parts of this work in our Fifth number. The present Part completes the first volume, which comprehends the whole of the general and special anatomy of the adult. The two books now before us, containing the anatomy of the vascular and nervous systems, are composed in the same minute and careful manner as the preceding; and we repeat our former statement as to the very great merits of the whole work. We are happy to find that Dr. Krause's manual will shortly be presented to the English reader translated by Mr. Morley.

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## Art. XV.—*Neurologische Beobachlungen.* Von Dr. F. H. Bidder. *Neurological Researches.* By Dr. F. H. Bidder, Prosector in the Anatomical School of Dorpat.—*Dorpat*, 1836. 4to. pp. 58.

Dr. Bidder's researches lead to the conclusion that the nerves of the tentorium arise from the fourth nerve, and not from the first division of the fifth. This agrees with Arnold's *first* account of these nerves. They arise from the fourth nerve soon after it has entered the dura mater; then run in the groove between the posterior clinoid process and petrous bone. Sometimes these nerves may be traced continuous with those of the sympathetic which ascend to join the fourth: Dr. Bidder considers the nerves of the tentorium as sympathetic nerves.

The muscles of the soft palate which are brought into action in swallowing, vomiting, and in the commencement of sneezing, receive branches not only from the n. pterygo-palatinus of the second division of the fifth, but also from the glosso-pharyngeus. Dr. Bidder considers the former not only a sensitive, but also a motor nerve; and as the first two divisions of the fifth are merely sensitive, he derives the motor portion from the communication of the facial with the ganglion spheno-palatinum, by the nervus petrosus superficialis, which he says arises from the central part of the facial, and which is contrary to former investigations.

Dr. Bidder describes a nervus petrosus superficialis tertius: it proceeds from the plexus which accompanies the arteria meningea media; lies first in the base of the skull, nearly covered by dura mater, or rather between the laminæ of this membrane: soon, however, it enters an opening on the anterior surface of the petrous bone before and under the aditus canalis Fallopii, and passing to the facial, joins it behind the knee or in the swelling of the knee. Once he saw it come from the nervus petrosus superficialis minor, which divided into three branches,—one to the anastomosis of Jacobson, one to the knee of the facial, and the third joined the facial behind the knee. The nerves which accompany the arteria meningea media send twigs to the dura mater.

Dr. Bidder observes that he has never found the n. petrosus sup. minor lying free on the petrous bone, but always covered by bone; it does not pass through the foramen ovale, but through the sphenoid and petrous bones, and appears afterwards on the surface of the latter.

# PART THIRD.

# Selections from the Foreign Journals.

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## ANATOMY AND PHYSIOLOGY.

### *Microscopical Observations on the Anatomy of the Aqueous Capsule and System of the Crystalline Lens.* By Dr. WERNECK, of Salzburg.

*Aqueous Capsule.* DR. WERNECK first of all directs our attention to the development, or, as he terms it, *genesis*, of the membrane of the aqueous humour. He states that, when he examined the eye of the adult, by means of the knife and forceps, he could not convince himself of the existence of that membrane. It is only after boiling in water, and when the cornea is in a great measure reduced to glue, that the membrane of the aqueous humour can easily be separated. In the horse's eye, however, and still more readily in the eye of the lynx, especially in young animals of these genera, and also in the eye of the human fœtus, this membrane may be demonstrated without difficulty. It covers completely the posterior surface of the cornea and the anterior surface of the iris: it is closely applied to the cornea by means of short, firm, cellular membrane; but it runs loosely over the iris, descending into the depressions formed by the iridal vessels. In the fœtus, while the pupillary membrane is still entire, the aqueous capsule forms a short, compressed sac: indeed, it forms the anterior part of the pupillary membrane. There are many anatomists who teach that the aqueous capsule covers also the posterior surface of the iris, the ciliary processes, and the anterior surface of the crystalline capsule, so as to form a serous sac, lining both the anterior and the posterior aqueous chambers. Other anatomists deny the existence of this serous investment of the uvea and ciliary processes, and urge that, in that case, by dissecting the eye under water, such a membrane should be detected, and the pigmentum nigrum should not be so easily detached. Dr. Werneck describes minutely the state of the aqueous chambers, and of the pupillary membrane, during the fourth and at the beginning of the fifth month of pregnancy, towards the end of the fifth month, towards the middle of the sixth month, at the beginning of the seventh, in the course of the seventh, in the eighth and ninth months, and soon after birth. His description, however, does not differ in any important respect from that which is usually given; viz. that these cavities are formed by two shut sacs, in contact with one another at the pupil, where their apposition forms the double pupillary membrane. He describes the anterior lamina of the pupillary membrane as presenting itself in the course of the seventh month, perfectly clear and transparent, while the posterior is full of blood-vessels. The gradual attenuation of the pupillary membrane, its loss of vascularity, its rupture, and the shrinking of its fragments, have been carefully observed by Werneck: he states that, after birth, it is impossible to demonstrate the membrane of the aqueous humour, passing over the crystalline capsule. Towards the middle of the sixth month, injections through the aorta are generally very successful in filling the vessels of the eye; and indeed, without injections, the arteria centralis may be seen at that period, ramifying in a beautiful manner in the cellular substance between the posterior crystalline capsule and the vitreous body, as well as the vessels of the anterior crystalline capsule, which are partly derived from the circulus arteriosus Mascagni, partly from the ciliary processes. Werneck gives a very beautiful representation of the latter vessels from nature.

*Crystalline Capsule.* Dr. Werneck represents the crystalline capsule as a complete sac, bound down by its posterior segment to the fossula hyaloidea by means of a very short cellular membrane. Around its obtuse edge lie the retina, corpus ciliare, ciliary processes, and zonula Zinnii. The processes of the zonula Zinnii end in very delicate pointed fibres, not easily counted, which converge uniformly towards the capsule. It is here, according to Dr. W., that the membrane of the aqueons humour, after covering the anterior surface of the ciliary processes, unites with these processes of the zonula Zinnii, having loosely invested the former. The continuation of this united structure, from the points of the ciliary processes to its union with the capsule, is the peculiar structure called by Ammon *orbiculus capsulo-ciliaris*. According to Dr. W., the crystalline capsule consists properly only of one membrane; which, however, may be divided, by different manipulations, into two lamellæ. The external lamella is completely transparent, so as not, even under the microscope, either in the fœtus or in the adult, to present any characteristic structure. The blood-vessels do not appear to penetrate its texture, but to run over the surface which it turns towards the lens. No trace of lymphatics is detected in this membrane, but it appears completely clear like crystal, bringing to mind the remark of Rudolphi, that the dense smooth part of the serous membranes seems to consist of an exceedingly thin layer of cartilage, and like that is bloodless. This character of the capsule is very distinct in the lynx and beaver, but still more so in fish. This membrane, even after maceration for months in alcohol, never becomes cloudy, but retains its transparency: it does not putrefy upon being exposed to the air, but dries quickly, like any other piece of cartilage; and, being again moistened with water or other fluid, resumes its original appearance. Immediately on the internal surface of this *lamina crystallina* lies the other layer, which, in its structure as well as in its other properties, is very different. Dr. Werneck represents this lamella as seen through the microscope. In examining it, it is necessary to lay the anterior surface of the capsule, without folds, and moistened with a little water, upon a thin plate of glass, and so bring it under the compound microscope. It is formed of a loose texture, so that Dr. W. compares it to mucous membrane: it is, however, when quite recent, perfectly transparent. This lamella is exceedingly thin, measuring, after it is hardened in spirits, about the $\frac{1}{10000}$th part of a line in thickness. Under the microscope, there are observed in it many distinctly marked circular plates, the most of which measure $\frac{4}{10000}$ parts of a line. Dr. W. seems doubtful whether he should regard them as little vesicles. Between these round bodies very fine vessels form so regular a network, that one of these plates is placed within every mesh.

Although this delicate membranous layer cannot be demonstrated in an isolated state, still, by macerating it for a little time in alcohol, whereby it becomes opaque, it may be perceived with the naked eye, and scraped away in shreds from the cartilaginous lamella, which remains perfectly transparent. However much Dr. W. has laboured, and however often he has completely succeeded in injecting the fœtal eye with copal varnish, so as to show the vessels of the *lamina crystallina* as innumerable ramifications, he has never been able to discover a single red vessel in this membranous layer, although the appearance and distribution of its vessels correspond more with those of blood-vessels than of lymphatics. This delicate layer readily putrefies on maceration in water. It exists in all animals; only the little circular plates he has not been able to find, except in one species of ape, whose eyes he examined an hour after death: in most instances he saw only a structure consisting of confused scales. He regards this layer as the peculiar structure by which chiefly the liquor Morgagni is secreted, and thinks that this fluid deserves more attention than has hitherto been bestowed upon it, as upon it in part depends the life of the lens. In connexion with this layer there is still another structure, to which he gives the name of *Fächergewebe*, (honeycomb tissue,) through the medium of which the organic connexion is effected of the capsule and the lens. It is a very common opinion that the lens, like the simplest kind of hydatid, is enclosed within, but without any connexion with, its capsule. The lens, it is true, has no immediate connexion with the red-blood system, yet it enjoys no peculiar independent life. No arteries pass into the lens from the blood-vessels of the capsule, but on all sides serous vessels pass into it and nourish it.

*Honeycomb Tissue.* According to Dr. W., a peculiar honeycomb-like connexion between the capsule and the lens is to be seen with the microscope. This texture resembles in delicacy the original primitive texture, (the *Urthierstoff* of Seiller,) and consists of extremely short six-sided cells, each being about $\frac{10}{10000}$ parts of a line in thickness. This texture is most abundant behind the lens: it may be seen, however, by removing the cornea of a recent eye under water, opening the capsule by a puncture in its middle, and cautiously separating a strip of the capsule with a pair of forceps all the way to its edge, whereby the cells, which adhere more firmly to the capsule than to the surface of the lens, are torn through, and then, snipping the separated portion across, laying it on a plate of glass in such a way that the surface formerly in contact with the aqueous humour may touch the glass. So disposed, and covered with a very little water, the shred of capsule is to be brought under a microscope, magnified linearly 110 times, when the honeycomb texture will be distinctly seen, especially at the edge of the capsule. With one of Plössl's instruments, an experienced observer will see traces of the torn cells, even on the anterior surface of the lens, if it has been previously coloured with an alcoholic decoction of Brazil wood.

As the cells or texture which the honeycomb-work constitutes is exceedingly delicate, (much more so even than that which passes, in the eye of the fœtus, from the periphery of the capsule to the uvea,) it happens that, even with the most careful manipulation, the greatest part of the cells are distorted, and their symmetrical appearance destroyed. The fine substance by which these six-sided cells are formed is very transparent, but, like the inner lamella of the capsule, becomes cloudy when put into strong alcohol; never so much so, however, as the membrane of the hyaloid cells.

In the space of one of these cells, four or five of the circular plates of the internal lamella of the capsule are observed. The depth of these cells Dr. W. has not ascertained: judging, however, from the distance between the capsule and the lens, they cannot surpass $\frac{16}{10000}$ parts of a line. Through these cells, which communicate with one another, the humor Morgagni flows in upon the lens. Although some doubt that this humour exists during life in any appreciable quantity, or believe that it appears only after death, from imbibition or transudation, the oculist, who has seen what is styled *cataracta Morgagniana* even only once, will not hesitate about the existence of this fluid. Dr. W. formerly thought that, in the living and healthy state, the liquor Morgagni formed merely a vapour; but he is now convinced that it is an actual fluid. It will not surprise those who know from experience the rapid power of absorption of the vessels of the lens, and reflect that, in the act of death, when the *turgor vitalis* fails, the capillary system, in most cases, dies sooner than the heart,—and so all the secretions cease while absorption goes on, that, when the eyes of animals are examined soon after death, the humor Morgagni is found in so small a quantity, or not at all.

*Lens.* Whoever examines the lens in man and other animals, in its different stages of development, as well as at different periods of life, in its healthy as well as in its diseased states, and compares it with other organic structures, will be convinced that it is composed in a great measure of fibrous bundles, which are regularly laid upon each other, so as to form thin leaf-like layers, like the coats of an onion. Each concentric leaf, or shell, presents two different organic structures, more especially demonstrable in the fœtal lens, which has lain in some coloured fluid containing tannin.

1. *Membranous Part of the Lens.* At each pole of the lens we meet with a very fine, membranous, porous, completely transparent structure. At the anterior pole this structure forms three slender lines, meeting at equal angles, towards which lines the fibres, afterwards to be described, run, and to which they are connected: so that the anterior surface of every lamella is divided into three segments. In these three lines the fibres are lost, as the muscular fibres in a tendinous

expansion; but, at the points of the three converging lines, the fibres run in a vorticose manner, something like the fibres of a sphincter. On the posterior surface of the lens, this membranous structure is commonly formed of four lines, sometimes only of three. In a man of ninety-six, Dr. W. found only a small disc, not quite round, whence the fibres radiated towards the periphery of the lens. On this surface, as on the anterior, the fibres form a kind of vortex at the point of each of the three or four lines; while, towards the centre, where the lines meet, the fibres run nearly in a straight direction. It is this delicate texture which becomes torn in living animals when the solar light, or phosphorus light,* is directed upon the crystalline by means of a lens or concave speculum. When this is done, the crystalline flies into three or four pieces. This fine membrane passes from the poles inwards, and spreads out from the axis of the lens between the laminæ. On macerating the lens in water, this fine texture, which forms the poles and the spaces between the lamellæ, remains behind, so that it may be examined in water, or, which is better, in alcohol.

The three converging lines on the anterior surface of the lens are not opposite to the three on the posterior surface. We believe that, on the anterior surface, one of the three lines runs horizontally towards the nasal edge of the lens, while the other two lines diverge from it towards the temple; while, on the posterior surface, one of the three lines runs towards the temple, the other two diverging towards the nasal edge of the lens. This, however, is contrary to the statement of Dr. W.: he says that, on the anterior surface, one of the three converging lines is usually directed upwards, and the two others towards the sides. Dr. W. states that the three lines on the anterior surface of the lens are constant, but that he has met with slight deviations on the posterior surface in domestic animals. In the rodentia, the anterior surface of the lens presents a single transverse line, and the posterior a single vertical line; the same also in fish; so that the lens in these animals divides into four segments. In birds and the amphibia, there is no trace of segments: only in the middle, where the fibres meet, there is a small round structure, an actual membranous pole. In the adult lens, the segments are completely united, so that the lens can be separated only by mechanical or chemical means. This union of the segments is accomplished by means of the exceedingly fine cellular membrane, forming what Dr. W. calls the membranous part of the lens. It is only in the eye of the fœtus that this cellular connexion can be demonstrated clearly and completely. At this period, also, the three lines on the anterior and posterior surfaces of the lens are much broader, and vanish gradually in proportion as the roundish form of the crystalline changes to the lenticular.

2. *Fibrous Structure of Lens.* This structure forms the greatest part of the laminæ of the lens. Each fasciculus of fibres measures, in the superficial laminæ, $\frac{3-4}{1000}$ parts of a line: each such fasciculus contains four or five fibres. In the deeper lamellæ, the fasciculi, as well as the fibres themselves, appear to be thinner and more delicate. These fasciculi are quite perceptible when the lens is submitted to the microscope, enclosed in its capsule. The fasciculi can be torn into fibres, in the direction of their length, which cannot be done with lymphatic vessels. These fibres and the lamellar structure of the lens are not produced by hot water, alcohol, mineral acids, or mineral solutions; no more than by these means we can change the structure of the brain or of muscular fibres. Why (Dr. W. asks,) shall we not regard these fibres of the lens as similar to muscular fibres? Some may answer, because, when they are separated, or when they are rubbed, as Berzelius directs, they almost entirely dissolve in cold water; which does not happen to fleshy fibres. Dr. W. says, that not to mention that, through division and trituration, the whole organization of so delicate a structure is destroyed, he shall only state, by way of answer, the fact that the lens presents, in this respect, a similar condition with the muscles of many water-insects, and especially the rotatoria; and that all the organs in the eye present quite a peculiar structure,

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* By phosphorus light, we suppose Dr. W. means the light obtained by burning phosphorus in oxygen.

especially those which possess no red vessels. Also, in a morbid state of the lens, especially in arthritic cataract, we distinctly observe the fibrous concentric structure of the lens.

Chemistry affords us very little information on the peculiar composition of the lens. Fifty per cent. of it is water; the rest chiefly a peculiar matter which seems to hold a middle place between albumen and osmazome.

Of the spaces between the lamellæ, it is easy to convince one's self by allowing the lens to lie for several days in distilled vinegar, coloured with carmine; as, by the absorption of this, these spaces become filled with the colouring matter. In these spaces the Morgagnian fluid circulates, and every lamina is enclosed the one by the other; each capsule, and with it the membranous part, becoming smaller and smaller, and more and more delicate, till we come to what is called the *nucleus* of the lens.

*Development of the Lens.* Notwithstanding numerous researches in the embryos of quadrupeds, birds, and fish, Werneck has not been able to determine whether the eye is projected from the middle ventricles of the brain, or formed, originally and independently, out of the primitive animal tissue. He is certain that the lens, however, lies close to the fibres of the optic nerve, and is developed quite independently of the other parts, and without any blood-vessels being discovered in it. Already, by the end of the second day, in the chick, a trace of lens is seen, like a round elementary corpuscle. In the third day, the capsule is distinctly perceived, like a completely round vesicle. It is entirely shut and perfectly transparent; its contents are somewhat pappy, and consist, as seen under the microscope, of opaque, milky-coloured, partly round, partly oval grains, enclosed within a gelatinous material. These Dr. W. regards as primitive, granular substance. In the fourth day, the retina, forming already a completely fissured membrane, covered with blackish grains at the sides, and traces of the uncoloured edge of the iris, also fissured, appearing, there forms within the capsule of the lens, at every point of its internal surface, a peculiar cellular texture, which is the connexion of the lens with its capsule. In the beginning of the fifth day, the zonula Zinnii being already formed, the fibres of the lens begin to be visible towards its edge. The embryonic grains run in wreaths from the edge towards the centre, and, from the running together of these grains, the fibres of the lens are formed. This change does not take place piecemeal or by laminæ, but in the whole of the original mass of the lens the fibres are formed. There is no appearance of lobes or segments. At the end of the fifth day, the fibres, running from the periphery to the centre, are still more distinct; and, as the formation of the fibres proceeds, the lens from its periphery becomes clear. In the seventh day, when the ciliary processes show themselves, the granular, original substance retires still more, and the formation of fibres advances. By the tenth or eleventh day, the clearing of the periphery is far advanced, and the grains are only at the middle, but already arranged in the form of fibres. By the thirteenth day, the circumference of the lens is completely clear; its middle only is somewhat cloudy, but the grains are no longer visible.

The lens is formed exactly after the same manner in the amphibia; so also in fish, only that in them the development goes on in four segments. On the sixth day in rabbits, and on the fourteenth day in the calf, the formation of fibres out of the embryonic material is already evident within the completely round capsule; but the formation of the fibres goes on in these animals, as well as in the hare, Guinea-pig, &c., at once in four groups; in cattle, sheep, swine, and man, in six. Even here, however, the formation of fibres out of the original material does not take place by layers from the centre to the periphery, but the reverse: the fibres form in the whole extent of their groups, from the periphery towards the centre. In the calf already at the beginning of the second month, and in man in the course of the first fourteen days, the first trace of the formation of the lens is visible. By the end of the third week, the formation of the crystalline fibres is considerably advanced; the lens at its circumference is already transparent, and the segments are already half united. Werneck believes that, when the fibres have formed small segments of a sphere, such segments unite two and two together, and become so joined by the cellular substance already mentioned, that they form, commonly in cattle, swine, and man, a third segment of the lens; but in hares, rabbits, and fish the half. The union of the segments immediately follows. The traces of the union of the segments form the lines of demarcation which are well known.

In the calf, the cloudy appearance of the lens vanishes in the eighth week; in man, sometimes in the eleventh, and sometimes not till the fourteenth week. In pheasants, leverets, and partridges, Dr. W. repeatedly saw the cloudiness in the middle of the lens gradually vanish after birth. In the sixth month, the middle of the lens begins to turn firmer; and, in the eighth, the kernel is to be found, though still delicate, like all the textures of the fœtus.

Dr. W. represents the individual crystalline fibres in man, the mammalia generally, and in birds, as having the form of six-sided, elongated prisms, which in old animals become stiff externally, and are thus harder than in their interior. These fibres, in a separated lamina, are of unequal length, breadth, and thickness. At their extremities they are slenderer, and terminate in blunt points; they are broadest in their middle, where they form the periphery of the lens. The longest fibres occupy the middle of each lamina; at the sides of the laminæ, where, when the laminæ are joined together, the vortices are formed, the fibres are shortest. In the middle of the laminæ they are straight; at the sides they are bent. In the mammalia, some fibres are observed not to run the whole length of the laminæ to which they belong; and sometimes, in cattle, they run in a waving manner.

In birds and the amphibia, the course of the fibres is more regular, and their extremities become more rapidly pointed. In man, they are about the most slender, and, on maceration in water, they dissolve into fine threads. The deepest fibres are the most slender.

In fish and the amphibia, the fibres have the form of flat, little bands, the edges of which are variously indented according to the species; the indentations reciprocally receiving and being received, like the sutures of the skull. In man, the mammalia, and birds, the connexion of the fibres is accomplished in such a way that each fibre, with its prismatic surfaces, is received between two other fibres.

The lens consists of layers of different densities; and it is easy to distinguish the external, softer, and more glutinous laminæ from the internal, which are more consistent. On the various densities of its laminæ, their various curvatures, and the various positions of its kernel, depend the various refractive power of the lens. Treviranus distinguishes three different consistencies in a section of the lens, which gradually run into one another. In birds, the kernel lies always in the centre; not so in all the mammalia, though it is the case in man. If we macerate the lens of a newborn mammiferous animal, it is found that the central part is always exceedingly firm; but this does not consist of crystalline substance, but of cellular membrane, what Dr. W. calls the *membranous part* of the lens, which remains undissolved. Dr. Werneck contends that, in the lens of the embryo, there is no liquor Morgagni; and that, of course, the crystalline substance cannot be regarded, as some have been inclined to do, as a deposition from, or crystallization of, the Morgagnian fluid. He considers it an unlikely supposition that the honey-comb tissue, which serves as connexion between the capsule and the lens, can deposit the substance of the fibres as we find them, in various animals; of prismatic form, and with their edges sometimes smooth, sometimes denticulated, and arrange them so that each fibre is laid between two neighbouring fibres.

Dr. Werneck's communications are accompanied by no fewer than forty-six figures, illustrative of the structure of the capsule and lens in man and other animals.

The following curious experiments on the eye are mentioned by him in a note:

In a cataractous rock-shrite and an English hound, Dr. W. disruptured the cataractous lens by the use of a strong burning-glass, and had the pleasure to find that it gradually dissolved, and at length was totally removed. The retina was not at all injuriously affected by this experiment, nor did inflammation arise in consequence of it. He asks, whether this same means might not be used in the human eye affected with cataract, as well as on that of the lower animals, when the capsule is not much thickened, or the cataract purely lenticular. If the experimenter is careful not to bring the focus of the lens upon the cornea, but only permits it to fall for a moment on the surface of the capsule, there will be no danger.

As he wished to convince himself of the cause of this disruption of the lens, which could not consist in any action of the muscles of the eye alone, he opened the cornea in a number of animals, removed the iris, and then convinced himself, during the use of electricity, and while viewing the eye through a powerful glass, that the disruption of the lens was chiefly brought about by strong spasmodic contractions of the zonula Zinnii. In this very interesting experiment, he recognized the truth of Camper's opinion that the fibres of the zonula Zinnii are muscular, and thinks that Döllinger's explanation is not to be rejected, when it is evident that, through the contraction of this ring, the lens is pushed forward, and brought nearer to the uvea. He thinks this motion of the lens, taken in connexion with its lamellar structure, and the action of the muscles of the eye, ensures distinctness of vision at different distances.

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*On Reflex Motions*. By Dr. A.W. VOLKMANN, Professor of Physiology at Dorpat.

[IN the following article we have given all that is of any importance in Dr. Volkmann's paper. In several of his remarks and conclusions in the latter part of it, (less novel than the author seems to imagine,) we do not concur; but we have been unwilling to omit anything that bears on a subject at this moment so livelily discussed, and possessing such intrinsic interest.]

1. *Phenomena observed in Frogs after Decapitation*. On dividing the spinal column of a frog immediately below the skull, violent muscular motions are induced, which, however, are not of long continuance. Sometimes the posterior extremities are adducted to such a degree that the body makes a complete somerset; at other times the effect is entirely opposite, the extremities being violently extended. Dr. Hall has asserted that the decapitated animal, after the first disturbance produced by the operation has subsided, remains quietly in the position which it then assumed till the extinction of life, if no external excitant be applied. This is a point of great physiological importance, but Dr. Volkmann found the assertion to be erroneous. This physiologist has repeatedly observed decapitated frogs call the posterior extremities into action, without any external cause being applied, for the purpose, apparently, of assuming a more comfortable position. It is generally remarked that, after the head has been separated from the body, and the convulsive motions have subsided, a period of quietude succeeds, the result apparently of exhaustion. In this period, generally a few minutes after decapitation, there is an interval in which the body is little irritable, and, although at a later period the slightest touch produces reflex motions, it may then be handled without producing these movements. If, when in this condition, the body be extended on a hard surface, it will retain the position in which it has been put for five or ten minutes, till suddenly, and without the application of any external stimulus, it will draw up the thighs, and change its recumbent to the sitting position; in which, however, it will remain, in all probability, till the extinction of life.

2. *The simplest Phenomena of Reflex Motion*. On irritating a decapitated frog by pricking, pinching, burning, or even merely by touching it with the finger, muscular motions are induced, which correspond exactly with those of voluntary motion, and are not merely sudden convulsive twitches, such as ensue on irritation of a muscle. These phenomena continue in lively frogs, salamanders, and water salamanders, always for several, often for many, hours after decapitation; but cease immediately on the destruction or excision of the spinal cord. If any of these amphibia be divided into several parts, each part which contains a portion of the spinal cord will exhibit reflex motions, which cease in each part with the destruction of the corresponding portion of the spinal marrow.

3. *Reflex Motions, with longitudinal Section of the Spinal Cord.* The upper part of the spinal cord of a decapitated frog was laid open, and the cord divided in a longitudinal direction from its commencement, as far as the fourth spinal nerve. Uninterrupted convulsive twitchings of the anterior extremities, and slight convulsions of the posterior extremities, were the consequence; and, when they had subsided, the animal assumed the sitting position. On touching one of the anterior extremities, it was withdrawn; and, on pinching it severely, not only was the irritated foot withdrawn, but both the posterior extremities were violently moved. On reapplying the irritation, the same phenomena were observed; but no irritation of one anterior extremity produced motion in the other. On irritating the posterior extremity, reflex motions were induced, not only in the opposite extremity and in the anterior extremity of the same side, but also, to all appearance, in the anterior extremity of the opposite side; for, on pinching the left posterior extremity, the animal threw itself over on its left side, which could only be effected by extension of the right arm: convulsive motions were at the same time observed in the muscles of the right shoulder, which derive their nerves from that part of the cord which had been longitudinally divided.

The lower portion of the vertebral canal of a decapitated frog was now laid open, and that part of the spinal cord which supplies the posterior extremities with nerves divided longitudinally. Irritation of the anterior extremities was followed by motion of all four; but, on irritating one of the posterior extremities, distinct reflex motions were perceptible only in the anterior extremity of the corresponding side: in the anterior extremity of the opposite side they were doubtful, and in the opposite posterior extremity not a trace of motion could be perceived. In another frog, the spinal cord was divided longitudinally from its commencement, as far as the last lumbar nerve; but in this case the reflex motions were very indistinct; a result apparently dependent upon the great organic destruction. Irritation of the anterior extremities produced no movements in the posterior; irritation of a posterior foot produced motion in the muscles of the corresponding thigh and side of the abdomen, and, when very severe, even in the musculus cucullaris of the same side; but there was never the slightest movement in the muscles of the opposite side of the body, although the spinal cord below the sciatic nerve remained uninjured.

In another instance, the whole of the vertebral canal, excepting the fourth and fifth vertebræ, was laid open, and the cord divided longitudinally, except where it was covered by these two vertebræ; thus leaving a small portion of the cord, supplying the middle part of the abdominal region with nerves, uninjured. Irritation of an anterior extremity produced reflex motions in it alone; but irritation of a posterior extremity was followed by motion of all four.

In drawing conclusions, proof can be deduced from those cases only in which motion occurred, not from those in which it failed; for it is evident that reflex motions can take place only when their organic conditions are fulfilled; whereas, they must often fail on account of the great destruction produced by the knife. With these considerations in view, Dr. Volkmann thought himself justified in assuming that longitudinal division of the spinal cord does not prevent the extension of reflex motions over all the muscles of both sides of the body, so long as any part of the proper cord remains united in the mesial line. The proper cord he limits to that part which gives origin to the first ten spinal nerves; the part posterior to this does not seem to be capable of reflecting stimuli from one half of the body to the other.

4. *The Reflex Motions partake of the character of Adaptation to a determinate end.* The motions which ensue on irritating a decapitated frog not only sympathise in so far as those muscles which are associated together in life are thereby called into simultaneous action; but that action is of a kind adapted to the attainment of an object, for the nature of the movement varies according to the peculiar nature of the stimulus. A decapitated tortoise, when irritated, conceals itself beneath its shell; and a decapitated frog, on being irritated, comports itself according to the nature and degree of the irritation. The fore-feet of a frog were irri-

tated, and it withdrew them: they were further irritated, and it withdrew them further: they were still further irritated, and it then drew them in below the belly, and changed its sitting position into the recumbent. If the posterior extremity of a decapitated frog be violently irritated whilst it is in the sitting position, it will bound forwards: if it be roughly seized in the thoracic region, it will plant its fore-feet upon the hand which holds it, and try to free itself; and, when the skin of the abdomen or back is seized with the forceps, it is by no means uncommon for the mutilated animal to scratch the part with the posterior extremity of the corresponding side.

[From these experiments Dr. Volkmann concludes "that the decapitated animal is aware of the action of the stimulus, and chooses, amongst a variety of means, those which are best calculated to free itself from the annoyance;" but we cannot see that this inference is warranted by the facts. It involves the supposition that not only sensibility but the power of willing remains in the spinal cord after decapitation; and not simply in the spinal cord, but in each segment of it, when separate from the rest. On this hypothesis, we might separate, not plants and polypes only, but human beings, into as many distinct individuals as each possesses pairs of spinal nerves; since we cannot conceive any higher qualification for a separate individuality than the power of feeling and of willing. The mere adaptation of particular movements to designed ends cannot be regarded as a reason for supposing that these movements are voluntary, or even dependent upon sensation. The whole organized fabric is made up of a number of such adaptations. The motions of the heart, alimentary canal, &c. are instances of their simplest forms. The motion of the pupil is a case more directly to the purpose, being a reflex action evidently adapted to a particular purpose,—the conservation of the eye in the state best adapted for the visual function; yet every one knows this to be independent of volition, and pathology shows that it may be performed without sensation.]

5. *The Extent of the Reflex Motions is chiefly dependent upon the amount of Irritation and the degree of Excitability.* When one of the amphibia, recently decapitated, is gently irritated, the motions are confined to the parts in the immediate vicinity of the irritated spot: it happens sometimes, for instance, that tickling of a toe will produce movements which are confined to the irritated foot alone; but, if a greater degree of irritation be employed, the whole limb is thrown into motion; and, if a still greater irritation be employed, the movements extend over the whole muscular system. In the same manner, the extent of the reflex motions depends upon the degree of excitability. Some time after decapitation, irritation of a limb is insufficient to produce motion in any part of the body, except in the irritated limb itself, owing to the diminution of excitability.

6. *In the Reflex Functions the posterior Roots of the Spinal Nerves are exclusively Excitors, the Anterior Roots exclusively Reflectors.* By excitor nerves are meant those which conduct the stimulus which affects a peripheral organ to the spinal marrow; and, by reflectors, such as transmit the stimulus from the centre outwards to the muscles. After division of the three posterior roots of the sciatic plexus of a decapitated frog, the most violent irritation of the limb to which these nerves are distributed is insufficient to produce reflex motions; although, as it is easy to prove, the operation destroyed neither the reflecting power of the spinal cord nor the muscular irritability of the extremity; for, on irritating one of the anterior feet, reflex motions in all four extremities ensue. Hence it appears that, if division of the posterior roots of the sciatic plexus deprives the nerve of the power of producing reflex motions, it is because the posterior roots alone are capable of transmitting a peripheral stimulus to the spinal cord.

When, on the contrary, the anterior roots of the sciatic plexus are divided, irritation of the corresponding extremity produces reflex motions in the other three extremities, but none in itself; whilst irritation of one of the uninjured extremities produces motion in all the muscles of the body, with the exception of those of the extremity the nerves of which had been divided. It is, then, apparent that the posterior roots of the spinal nerves are incapable of transmitting a stimulus from the spinal cord downwards to the muscles.

These experiments were in imitation of those of Prof. Müller, (*Physiologie*, Band i. s. 703;) but, as this physiologist experimented upon undecapitated animals, some doubts might have been thrown upon the conclusion drawn from his experiments, as the disturbing influence of the will was not sufficiently attended to. His experiments might be said to prove incontrovertibly that the sentient filaments are the exclusive means of transmission inwardly, and the motor fibres those of transmission outwardly; but they left the point undecided whether, on the one hand the sentient and excitor filaments, and on the other the voluntary-motor and reflector fibres are identical.

7. *The Efficacy of the Stimuli which produce the Reflex Motions is modified or exalted by the extent of the Surface.* In a recently decapitated frog, the slightest touch is sufficient to produce reflex motions, although the epidermis prevents the direct action of the stimulus upon the nerves; but when, on the other hand, a nerve is exposed, it requires irritation of considerable energy to produce these movements. The great excitability of the skin is peculiarly manifest in frogs which have been brought under the influence of opium and then decapitated; tickling with a feather being in such cases sometimes sufficient to produce general convulsions, although, when a portion of the integuments has been removed, pricking and pinching of the subjacent muscles is insufficient to produce reflex motions. But, even in frogs which have not been affected with opium, the skin is possessed of a greater degree of excitability than seems to belong to the trunks of the nerves which supply it with sensation. A large flap of integument covering the whole dorsal region was separated, by an oval incision, from communication with the rest of the integuments, and was thus left in union with the body solely by some bands of cellular tissue and the cutaneous blood-vessels and nerves. Irritation of this flap with the forceps produced distinct reflex motions. It was then raised at one end, and its adhesions to the body divided. During this process, very indistinct reflex motions were only twice observed in dividing some nervous twigs, but at no other time. This experiment was repeated by isolating a large flap on the abdominal surfaces: irritation of it produced not only lively reflex motions, but also motions of adaptation; for, on pinching it with the forceps, the animal placed its fore-foot upon the irritated spot; but no réflex motions ensued on separating the flap cautiously from the body with the scissors. In dividing larger nervous trunks, such as the posterior roots of the sciatic plexus, reflex motions are occasionally observed; but they never partake of the character of adaptation. Had the animals which formed the subjects of experiment not been previously decapitated, it would very probably have been concluded that irritation of the skin produces perception of the stimulus, and, consequently, rational reaction; and that irritation of the nervous trunks excited the spinal cord only, producing organic, but no mental reaction.

8. *Irritation of the Sympathetic Nerve excites extensive Reflex Motions.* The thoracic and abdominal cavities of a frog were laid open, and, after the animal had subsided into a state of repose, those parts which derive their nerves from the sympathetic were irritated. It was thus ascertained: 1, that the reflex motions which are produced in frogs by the irritation of the sympathetic, although not so striking as in cases of cutaneous irritation, are still very decided, extending, in favorable cases, over the muscles of the abdomen, back, and four extremities; 2, that irritation of the bladder, of the large vessels in the vicinity of the liver, and of the oviducts during the period of sexual intercourse, produced the most lively reflex motions, whilst no reaction was observed to follow irritation of the heart, lungs, or liver; 3, that no reflex motions followed the irritation of the peripheral extremities of the sympathetic in the mucous membrane of the intestines, whilst they were exceedingly lively when a small nervous branch of the omentum was seized with the forceps; 4, that movements of adaptation were not observed to follow irritation of the sympathetic, although the reflex motions produced were

different from the mere convulsions of muscular irritability, as was evident from their continuing after the stimulus was withdrawn, and from the requisite associated action of the muscles as flexors and extensors being induced; 5, that irritation of the intestinal canal by pinching produces contraction of the intestine, which extends upwards and downwards over a greater or smaller space; 6, that, when the spinal cord is destroyed, irritation of the intestine produces mere local contraction, by which it appears that the sympathetic ganglions have no reflecting power; and, lastly, that it does not appear that motions are produced more readily in those muscles adjacent to the irritated sympathetic branches than in those which are more remote.

[It may be reasonably questioned whether some filaments of the spinal nerves may not be the source of these reflex actions: at any rate, we know that the parts here mentioned as originating them have some connexion with the cerebro-spinal system, from the sensations of pain or distress to which these diseased states give rise. The effect upon the intestines mentioned in 5 and 6, as resulting from destruction of the spinal cord, is only an instance of what has long been known of the connexion between any violent impression on the nervous centres and the diminution or loss of general muscular irritability.]

Dr. Volkmann endeavours to show, in the following paragraphs, in how far the above experiments correspond with the prevailing doctrines of the nervous system:

*The Transmission of the Nervous Action, from the Surface to the Centre, and from thence back to the Surface, is not subject to the same law as its transmission along the Nerves, as it does not follow the course of isolated fibres.* Recent investigations have proved that the stimuli of sensation, as well as those of motion, pass only along the fibres to which they were originally communicated, and do not pass into others; and, hence, that each elementary nervous filament may be considered as isolated: but the above experiments prove that the filaments of the spinal cord are possessed of no such isolating power, but that, on the contrary, the nervous action may pass from one fibre of the medulla to another. On pricking the web of the foot of a recently decapitated frog with a fine needle, reflex motions ensue in all the muscles of the body. The point of the needle cannot have irritated above one or two nervous filaments; and it would be necessary, in order to explain the consequent phenomena upon the known laws of nervous transmission, to assume that the irritated fibres stand in direct communication with at least one motor fibre of every muscle in the body. But this would still be insufficient; for Prof. Müller has shown that partial irritation of a motor nerve produces only partial motion of the muscles; and as, in the reflex motions, the muscles are not partially but wholly thrown into action, the irritated fibres would require to communicate directly, not only with one, but with many nervous fibres of such muscle. As each fibre which transmits the irritation to the medulla can produce general reflex motions, it would naturally be supposed that the spinal cord must consist of innumerable nervous anastomoses and interlacements; a structure, however, which examination does not confirm.*

It is certain, therefore, that the elementary fibres of the nerves exert an isolating power over the nervous action; but it is equally certain that the fibres of the spinal cord are capable, under certain circumstances, of transmitting their action to each other. On irritating the crural nerve of an amputated posterior extremity of a frog in the region of the knee, all the muscles situated below the irritated point are thrown into convulsions; but none which are supplied with nerves arising above the irritated point are affected, because their nerves do not stand in union by anastomosis with the irritated point, and because there is no transmission of the nervous action from one fibre to another in the nervous cord, and, consequently, there can be no transmission of motor excitement in a centripetal direction. But, on irritating the crural nerves of a frog which has been deprived of its brain, but which retains the spinal marrow, not only are those muscles which are situated below the irritated point thrown into convulsions, but likewise those which are situated higher; a phenomenon which can be explained only on the supposition that there is a crossing, as it were, of the nervous action from fibre to fibre of the medulla.

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* Müller decidedly refuses to admit the anastomosis of the medullary fibres, and, as Treviranus, in his Microscopical Investigations of the Nervous Substance, takes no notice of them, it may be supposed that he did not observe them. Ehrenberg, in innumerable investigations of the structure of the spinal cord noticed an anastomosis of the filaments only four times; but E. H. Weber has lately maintained that he has frequently seen the medullary fibres giving off branches to anastomose with others. Dr. Volkmann, since writing the above, was invited by Dr. Weber to attend a microscopical investigation of the structure of the brain of the pigeon, and was shown several preparations by this physiologist, which were said to exhibit the nervous anastomosis; but in three instances only was the ramification undoubted: in by far the greater number, the appearance was owing simply to the crossing of fibres, without any communication.

There is no anastomosis of nervous fibres in the brain, and yet a sensation proceeding from a single sentient fibre develops an action which, by means of the will or imagination, is transmitted to innumerable motor fibres: for instance, when an individual lost in thought is suddenly pricked with a pin, the sensation causes him to jump up, and thus call into action almost all the muscles of the body. It remains to be determined why, in reflex motions, the nervous actions cross from one fibre of the medulla to another; whilst, in other cases, its fibres form conductors as isolated as those of the nerves. It deserves to be borne in mind that a stimulus produces reflex motions in decapitated amphibia, which, previous to decapitation, had no such effect; and hence it would appear that the brain contains the cause which prevents the nervous action from crossing from filament to filament in non-mutilated animals.

On consideration that all reflex motions in the body are involuntary, and in general the result of stimuli, of the operation of which we are unconscious; and that, during sleep, motions are induced by touch, which, in the waking state, either would not have taken place or would have been performed through the influence of the will; it is impossible to avoid the conclusion that the transmission of the nervous influence from filament to filament is favoured by deficient mental energy, and is, on the contrary, in certain cases, prevented by an exertion of mental power.

The will prevents reflex motions, because, of two opposing forces, the weaker must yield, and the power of the will is often sufficient to keep muscular motions in control. It is not asserted, however, that decapitation favours the production of reflex motions solely by the removal of the mental influence: on the contrary, it appears probable that there are other, but unknown, causes engaged; for the inclination of the nervous action to cross from filament to filament is so great in decapitated amphibia, that it would require a very uncommon degree of mental influence to control them, on the assumption that it forms the only controlling power.

When the degree of irritation is small, the nearest muscles only exhibit reflex motions; but, when the degree of irritation is increased, the more distant muscles likewise are called into action. Hence, it may be inferred that the transmission of the nervous action from one nervous filament to another is favoured by an increase of the degree of irritation, and that contiguous nerves communicate their nervous action easier to each other than to nerves lying more remote. This second axiom is true only in a limited extent; for, when the hind-foot of a decapitated frog is gently irritated, the motions induced are confined to the foot alone; but, when the irritation is increased, they extend likewise to the muscles of the thigh, but without implicating the muscles of the other posterior extremity. But the feet derive their nerves from the second and third roots of the sciatic plexus, whilst the muscles of the thigh are supplied from the first root. The first and third roots, however, are separated by a greater space from each other than are the third roots of the right and left extremities; and, consequently, the transmission of the nervous action appears to be easier in a longitudinal than in a transverse direction; assuming it as granted that the apparent root is the real root of the nerve.

10. *Experience has not yet sufficiently proved that all Reflex Motions of decapitated animals, and particularly of Amphibia, are independent of the cooperation of the mind; namely, of Sensation and Volition.* Dr. Marshall Hall has excluded the cooperation of the mind in every case, chiefly because decapitated animals, when they have once become quiet, remain immoveable in their assumed position, if not irritated. The incorrectness of this assertion, as far as regards tangible irritation, has already been shown; but it cannot be denied that the influence of the air upon the raw surface of the wound might be sufficient to produce reflex motions, although the admission of such stimuli would rest upon mere hypothesis. His reason for supposing that sensation is not active in producing the reflex motions are unsatisfactory: he calls attention to the circumstance that they not unfrequently cease when the body is in a position which must prove highly uncomfortable, or even painful to the animal, if it retain sensation: they ceased, for instance, in a serpent whilst the tail was hanging over the sharp edge of a table, and were not renewed by pricking it or burning it with the flame of a candle: but their non-occurrence here was owing to the exhaustion of the excitability; and this is obvious, because pricking and burning in general produce reflex motions in decapitated animals. On the other hand, those motions which we have termed the motions of adaptation speak strongly in favour of the cooperation of the mind; although it must be admitted that there are many vital functions which partake of the character of adaptation, that are altogether independent of the will.

11. *Dr. Marshall Hall's proposed Classification of the Nerves.* It has already been stated that Dr. Hall admits two kinds of centripetal nervous fibres, the sentient and the excitor; and two of centrifugal fibres, those of voluntary and involuntary motion. The former two constitute the cerebral, or sentient and voluntary system; the latter two, the spinal or excito-motory system. The reflex motions belong exclusively to the true spinal nerves; that is, to nerves of which the fibres are incapable *per se* of transmitting both sensation and volition: but, if this view be adopted, it excludes from the class of reflex motions a number of phenomena which appear to belong to them. Indeed, it may be said that every voluntary motion is reflected, as being the result of a mental act called into existence through the medium of the impressions of the senses; the received stimulus being converted by the brain into one of volition. The motions of sneezing and the contraction of the pupil cannot be separated without violence from the reflex motions, and yet they cannot be said to belong exclusively to the spinal system. It is also doubtful whether the reflex functions, in a more limited sense, are confined to the excito-motor nerves. The point of the finest needle cannot penetrate the skin without producing pain; and hence it must be inferred that each minute point is endowed with sentient fibres. But the prick with the finest needle causes reflex motions in the decapitated frog; and, if these motions be dependent solely upon the true spinal filaments, we shall be forced to admit that each minute point of integument contains two specifically different nervous fibres.

Dr. Volkmann views Dr. Hall's classification of the individual nerves as highly objectionable, scarcely one nerve being assigned a position in which it could not be assailed. The nerves of the senses have, for instance, been classed under the sentient and voluntary system, but they are equally excitors, as is proved by irritation of the retina producing contraction of the iris and closing of the eyelids. The positions assigned to the pneumo-gastric, the trochlearis, and abducens may likewise be questioned, and Dr. Volkmann objects to the ganglionic system being separated from the other nerves, without reference to the circumstance that this system of nerves includes in its composition both sentient and excitomotory filaments.

12. *On the Participation of the Nerves in Sensation.* This paragraph contains some speculative ideas on the question, whether the nerves, in conducting

impressions to the brain, act merely as passive conductors, as wire in conducting galvanism; or whether they effect a change in the nature of the impression before it reaches the sensorium.

*Müller's Archiv.* 1838. Heft i.

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### *Anatomical Observations and Remarks.* By Dr. Krause, of Hanover.

### I. *On the Capillary Vessels.—The Thymus Gland.—The Intestinal Glands.*

1. The *capillary vessels* in many parts of the body, examined by the author, have presented a diameter considerably less than that of the smallest globule of the blood. The following is the measurement of the diameter of the most delicate capillary vessels in various parts:

| | |
|---|---|
| In the retina . . . | $\frac{1}{340}$ of a line. |
| choroid . . . | $\frac{1}{501}$ — |
| pulmonary cells . . | $\frac{1}{501}$ $\frac{1}{505}$ — |
| intestinal follicles . . | $\frac{1}{353}$ — |
| muscular integument of small intestine, | $\frac{1}{740}$ — |
| the tibialis anticus muscle . | $\frac{1}{1110}$ — |

In proportion to the capillary vessels of the ordinary diameter, (i. e. $\frac{1}{200}$ to $\frac{1}{300}$ of a line,) these very delicate ones are always fewer in number, and are generally placed intermediately between two larger branches. Krause has never found that the larger quantity of any capillary tissue was formed by these extremely delicate vessels. The injections employed were vermilion and size, or the successive injection of a solution of neutral chromate of potass, and acetate of lead, with some mucilage of gum arabic: the granules of chrome yellow thus formed are of a diameter of from $\frac{1}{800}$ to $\frac{1}{1200}$ of a line. The granules which remain after human blood has been macerated for two days in distilled water, (the kernels of the globules,) have a diameter of $\frac{1}{1894}$ of a line; but, on account of the feebleness with which they intercept the light, they are seen with more difficulty than some bodies of a smaller size. Contrary to his former opinion, the author is now convinced from observation that the capillary vessels have membranous parietes.

2. *Thymus Gland.* The author opposes the notion that the thymus gland is not found after twelve years of age. He has found it in almost all individuals between twenty and thirty years; and very often larger than in young children; and he has seen it of considerable size between the ages of thirty and fifty, and has also met with the brownish red remnants of it still later in life. In younger men its form is generally cleft into two parts, as in its original condition: these are generally adherent in the middle only by cellular tissue, so that their decrease appears to commence at this part. The lower cornua never, as in children, descend to the upper part of the pericardium, but frequently extend far into the neck.

The following is the measurement of the thymus gland in some very healthy and well made individuals who had committed suicide:

| Age and Sex. | Length. | Breadth. | Thickness. | Weight. | Volume. | Specific grav. |
|---|---|---|---|---|---|---|
| | | | | Grains. | Cubic Inches. | |
| 25, m. | 34 lines. | 18-25 lines. | 4 lines. | 292.5 | 0.977 | 1.0352 |
| 25, m. | 42 — | 32 — | 2-3 — | 380.3 | 1.156 | 1.0311 |
| 20, m. | | | | 356.5 | 1.083 | 1.0309 |
| 28, f. | 22 — | 16 — | 2 — | 69.2 | 0.211 | 1.0267 |

3. *Intestinal Glands.* Dr. Krause has carefully repeated the observations of Böhm on the intestinal glands (recorded in No. II. of this Journal,) and is quite satisfied of their accuracy. He adds some farther investigations, chiefly respecting the size of the various follicles and crypts of the intestinal canal. He does not, however, regard the glands of Peyer as essentially different from the solitary glands in the jejunum. The manifest differences are, that the glands of Peyer are crowded together and are naked and prominent on the free surface of the mucous membrane, whilst the solitary glands are abundantly beset with follicles. Both are of the same size, between $\frac{1}{8}$ and $\frac{1}{2}$ of a line, their cavity is rather more than half the

size of their external circumference, the parietes proportionally thick; the contents of both are not readily squeezed out, but its appearance is that of opaque mucus, the granules of which, flattened and irregularly rounded, are from $\frac{1}{330}$ to $\frac{1}{250}$ of a line in diameter; a size which corresponds with that of the granules of the mucus of many mucous membranes, though they are occasionally found of a larger size. But both the solitary glands and those of Peyer present a character essentially different from that of the glands of other mucous membranes, i. e. a slight roughness of the internal surface of their cavities, produced by a slight prominence of secreting cells, and a plurality of openings, whilst the glands of mucous membranes generally possess but one opening connected with their cavity. It is very rare to find but one opening to either the solitary glands or those of Peyer, the average number of their apertures being from five to ten. The openings traverse the parietes of the follicles in an oblique direction, and it is consequently difficult to assure one's self that they communicate with their cavities. The evidence of this, however, may be obtained thus: Take a large follicle of a gland of Peyer, and open its cavity by removing that half of it which is inserted into the submucous cellular tissue of the intestine; into the little rounded hollow, which is thus rendered visible, insert a very small drop of carmine in solution. On examining the opposite part of the follicle, i. e. that which projects into the intestinal canal where the openings are situated, the red fluid will be seen to escape from these apertures, before the entire follicle becomes coloured by imbibition.—*Archiv. für Anat. Physiol. und Wissent. Medicin.* Von MÜLLER. Heft i., 1837.

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## *On the Relative Motions of the Globules of the Blood and Lymph in the Blood-Vessels of Frogs.* By T. M. ASCHERSON.

IN our Eighth Number we noticed Professor Weber's views on the visible motion of the globules of lymph in the lymphatics of tadpoles, and to it (p. 500) we beg to refer our readers for an account of them. M. Ascherson has been led by his own microscopical observations on the mesentery of the Bufo cinereus, the Rana temporaria, and R. esculenta, and also on the lungs of the two last species, to doubt the correctness of Professor Weber's conclusion that the veins in tadpoles either lie in the cavity of a single large lymphatic, or are surrounded by a plexus of smaller lymphatic vessels; a view which the professor adopted, as that which best explains the appearance, as seen by the microscope, of a transparent border to the veins, into which the globules of the blood are not observed to pass, and which border Professor Weber supposed to be separated by an invisible partition from the venous trunks. According to M. Ascherson, the globules of the blood and those of the lymph move in the cavity of the same vessel; the lymphatic globules, he says, certainly move slower than those of the blood, but he is inclined to believe that the velocity of the one stands in a definite relation to the velocity of the other; the faster the motion of the red globules, the faster also will be the motion of those of the lymph. The latter, however, are apt to lag behind; they differ in shape from, and their surface is rougher than that of the globules of the blood, and hence they do not glide so smoothly along, and their sharp angles, by rubbing against the walls of the vessels, sensibly impede their motion, or sometimes even stop them altogether. A lymphatic globule may thus get attached to the walls of the vein and remain adhering till the projecting edge of a globule of the blood, or the coming up of another lymphatic globule pushes it off and sets it again in motion. In cases where the circulation is proceeding very slowly, the lymphatic globules remain altogether at rest, except when pushed on for a short space by the projecting edge of the globules of the blood. In the centre of the stream of blood the lymphatic globules move evidently quicker than at the edges. The preceding remarks refer more particularly to the appearances in those veins which have a transparent border; but M. Ascherson has convinced himself that there is likewise a difference in the motion of the two varieties of globules in those veins in which the stream of blood appears to be in immediate contact with the walls of the vessel: in them also the lymphatic globules evidently move slower and often get attached to the sides of the vessel.

In the course of his investigations, M. Ascherson was led to pay attention to the elasticity of the globules of the blood, a property which has been denied them by several able physiologists. The globules may be observed in the fine capillaries of the lungs of the frog to assume almost every variety of form and shape in passing through these short and narrow ducts, but this phenomenon becomes extremely apparent when the excised lung is compressed between two plates of glass. From the facility with which they regain their original form M. Ascherson infers that, excepting the elastic fluids, no body is endowed with so much elasticity as the globules of the blood.

M. Ascherson cannot satisfactorily explain the appearance of the transparent border, but he is satisfied that it does not depend upon a separate lymphatic vessel, as he has occasionally, though very seldom, observed a red globule to penetrate the transparent layer. The difference of velocity of the two varieties of globules he explains by the difference of shape, the highly lubricated state of the red, and the rough and gritty surface of the lymphatic globules.

*Müller's Archiv. Jahrgang,* 1837. Heft iv.

### *Absence of the Right Lung.—(Cyanosis.)*

The following deformities were found in a child, six weeks after its birth. From birth it had been affected with cyanosis: death was attended with symptoms of congestion in the head. The right lung was entirely wanting: there was only a rudiment of the right bronchus; no right pulmonary artery or vein. The septum ventriculorum cordis was imperfect. The origin of the aorta communicated with both ventricles. The foramen ovale and the ductus arteriosus were both open, the latter widely so, this supplied the left (the only) lung with blood, as the pulmonary artery was imperforate at the base of the heart.

*Wochenschrift für die gesammte Heilkunde.* No. 33. 1837.

## PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

### *On the Atmospheric and Epidemic Constitution of Berlin in September,* 1837.

[We give the following extract chiefly as indicative of the methodical nature of the German character, and of the attention paid in Germany to subjects comparatively neglected in this country.] Disease in this month occurred pretty frequently among the domestic animals, although not to any unusual extent. It was chiefly of a rheumatic character, frequently complicated with catarrhal and gastric affections, and more prone to become inflammatory than in the preceding months. Acute and chronic rheumatism, sore throat, rheumatic ophthalmia, inflammation of the lungs and pleuræ, inflammation of the hoofs and tendons occurred very frequently in horses. Several cases of pericarditis, peritonitis, gastritis, and enteritis preceded by rheumatic affections were also met with. Pure gastric diseases were rarely seen; colics sometimes occurred, but not so frequently as in the preceding months. Nervous diseases were rare, excepting a few cases of the staggers and a case of idiopathic tetanus. Most of the diseases were of a mild character, so that, in spite of the dangerous aspect which they in many cases assumed, few horses died. The oxen suffered chiefly from rheumatism, rheumatic and gastro-rheumatic fever, and also considerably from inflammation of the thoracic and abdominal viscera. A bull was affected with hydrophobia, about seven weeks after having been bit by a mad dog. There was little disease among the sheep, but one flock suffered considerably from inflammation of the bowels, probably from having been put to pasture on potato fields. A good many goats and some sheep suffered from indigestion, flatulence, and gastric fever. Excepting the distemper and its nervous complications there was no particular disease among the dogs, but there were a few cases of gastric disturbance, want of appetite, vomiting and diarrhœa, and one or two cases of tetters. The epidemic, alluded to in a former report, still continued in several farms among the fowls.

The prevailing diseases during the same period in the human species were cholera, dysentery, gastric fever, pulmonary inflammation, and measles.

*Medicinische Zeitung*, 1837. No. 41.

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### *Case of slow Poisoning by Oxide of Zinc.* By Dr. BUSSE, of Berlin.

A GENTLEMAN, aged forty-five, of regular habits and previously of good health, was, in 1825, attacked, without evident cause, with epilepsy, which continued to return at longer or shorter intervals. His physician recommended change of climate; and the three following years, during which there was no return of the disease, were spent in travelling, chiefly in Italy and France. But scarcely had he returned to Berlin when the fits again appeared. Various remedies were tried in vain; and the patient, happening to notice oxide of zinc, in combination with hyosciamus, recommended in Hufeland's Journal as a cure for epilepsy, resolved, without consulting any physician, to submit himself to this treatment. He secluded himself from society, and took daily, at an average, twenty grains of the oxide, till he consumed 3,246 grains, and would probably have continued his experiment to a fatal termination had not one of his relations insisted on being admitted to him, and by whom he was found in a most deplorable condition. Dr. Busse was immediately summoned, and found the patient of a pale earthy hue, wasted away, and almost idiotical: his tongue was thickly coated, the bowels were constipated, the inferior extremities cold and œdematous, the abdomen tumid, the superior extremities cold and shrivelled, and their skin dry like parchment; the pulse was about sixty, thready, and scarcely perceptible.

The oxide of zinc was immediately stopped, but not without some opposition on the part of the patient, and a purgative ordered. Light nutritive diet was allowed, with tonic and diuretic medicines; and under their use the patient rapidly improved, and soon regained his previous robust and healthy appearance, although he remained subject to epileptic attacks, which continued to recur every six or eight weeks. *Wochenschrift für die gesammte Heilkunde.* 1837. No. 19.

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### *On the Juice of the Root of the Elder-tree in Dropsies.*

THE author of this communication has employed the above juice as a hydragogue cathartic in several forms of dropsy, with the effect of completely relieving the system of the collected fluid. Its use appears to be attended with less colic than the other hydragogues with which we are acquainted; it is apt, however, to become exceedingly disgusting to the individual employing it; on which account it should be suspended for a short time, and then resumed. It should be given unmixed with other medicines. The cases which are given as illustrative of its action it is unnecessary to quote, as, from the want of very minute diagnosis, they scarcely can justify any application to other cases; but, as an addition to our hydragogues, the juice of the root of the elder-tree may occasionally be employed with advantage. It is obtained simply by subjecting the fresh root to pressure; the dose with which to commence being from two to four ounces.

*Bulletin général de Thérapeutique. Février*, 1836.

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### *Researches on Spontaneous Perforations of the Stomach.* By M. LEFEVRE.

THE design of the investigations pursued by M. Lefevre is, 1st, to explain the mode of formation of spontaneous perforations of the stomach; and 2d, to establish those characters which make them distinguishable from perforations the result of poisons.

It is difficult to assign the predisposing causes of this terrible accident. Nevertheless, Mr. L. thinks that persons who suffer habitually from stomach complaints are more predisposed than others. He reports six observations, all females. Can this be attributed to a modification of the form of their stomach, which Soemmering attributes to the pressure of stays? M. L. says that in all the observations which he has collected, aliments of an indigestible nature (such as peas, bacon, cabbage, &c.) have been the occasional causes of the symptoms which preceded the rupture; he therefore thinks that all substances capable of producing indigestion can equally occasion solutions of continuity of the stomach. In other words, M. L. thinks that if, by weakness or otherwise, the stomach is unable to free itself from that which embarrasses it, it is liable to rupture. This rupture has its seat naturally in the part which offers the least resistance; that is to say, in the lowest part of the organ.

The following appears to be the progress of the symptoms. Some hours after a repast, the patients are seized with a violent pain in the epigastric region with a sense of weight and swelling. To these symptoms succeed nausea and sometimes vomiting of a glairy and pituitous matter, as if it were more easy for nature to free herself from these matters than the aliments which incommode her. Little or no alvine evacuations occur, or, if they are obtained by lavements, it is evident they are the result of anterior digestions. During this period of the complaint the subjects are tormented with an extraordinary agitation: they roll upon the bed from side to side, often the belly is retracted at the middle portion whilst the epigastrium is tender and painful. The deglutition of liquids, although easy, immediately provokes nausea, and they are rejected. The tongue is pale, large, and humid. The pulse soft, regular, sometimes a little hard. However, this state of anguish is interrupted by moments of calm, although of short duration. Lastly, after a very violent attack the symptoms are aggravated and change their character. Some patients experience in the stomach the sensation of a body which displaces itself. The belly is tympanitic, and acquires an extreme sensibility. The pulse becomes frequent, small, hard, imperceptible. The skin of the extremities becomes cold and is slowly covered with a cold perspiration. The countenance expresses apprehension and terror. Finally, the patients expire with a piercing shriek which describes the immensity of their suffering. They preserve their intellectual faculties until the last moment.

*Bulletin de l'Academie Royale de Médecine. September*, 1837.

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## *On the Use of the Subcarbonate of Iron (Ferri Sesquioxydum,) in Hooping-Cough.* By Dr. Steymann.

Of all the medicines recommended in this painful and lingering disease, the subcarbonate of iron, according to Dr. Steymann, when exhibited in the second stage, is by far the best.

The following cases prove the utility of this remedy.

Case i. Henri Schrœder, eleven years old, had been suffering for nine weeks from hooping-cough, for which all the usual remedies had been exhibited, without relief. The subcarbonate of iron was administered in the dose of two grains every three hours. After the tenth dose of this remedy, all the symptoms were considerably diminished. Ten doses more, each consisting of five grains, completely subdued the disease.

Case ii. The sister of the former patient, five years old, was affected with the cough at the same period, had been treated in the same way, and with the same results. She commenced with two grains of the subcarbonate every three hours; this was increased to three grains, which completed the cure in about eight days. From the first dose the symptoms were alleviated.

Case iii. Jules Etier, five years old, was placed under the care of Dr. S. in the third week of the disease. The cough did not yield till she took the iron; and, after four days' treatment, she was completely cured. Dr. S. says he never heard the fits of coughing more violent than in this child.

Dr. S. holds out a caution against the exhibition of this remedy in the early stages, as he has found it at this period produce considerable irritation, in lieu of hastening the cure. In the first stage he employs leeches, opiates, and emetics;

and, before commencing the subcarbonate, he recommends the exhibition of an emetic.

The following he has found a convenient formula: Subcarbonate of iron, twenty-five grains, white sugar, a sufficient quantity to make into ten powders; one to be taken every three hours. This dose is increased according to the age of the patient, adding a grain for every year. The effect is generally prompt; and in a few days nothing remaining but a slight catarrhal cough, which gradually disappears. *Bulletin Général de Therapeutique. Mars,* 1838.

## *On the Nature and Treatment of Syphilis in the Province of Lithuania.*

By Dr. SCHNUHR.

SYPHILIS, in East Prussia, Lithuania, and the neighbouring provinces of Poland and Russia, is a disease so frequent, occurring amongst individuals of all ages, both old and young, that the conclusion is in a manner thrust upon the enquirer, that it is an endemic disease, capable of being communicated by other methods than by impure sexual intercourse. It is a feature of the disease in these countries, that, whilst chancres, buboes, and gonorrhœa are of comparatively rare occurrence, condylomata are met with in extraordinary abundance. The broad condylomata are much more frequent than the narrow: they are found on almost every part of the body, and may be either of primary or secondary origin. Rhagades are also extremely common, and syphilitic eruptions on the skin, swellings, and pains of the bones are also occasionally seen; but such of the last-named affections as have come under the notice of Dr. Schnuhr have always been preceded by the use of mercury.

From 1st November, 1835, to 1st November, 1837, 665 cases of syphilis came under the care of Dr. Schnuhr, viz.:

1. Broad condylomata, affecting, in the greater number of instances, the hips, anus, thighs, labia, and scrotum; and, in some cases, the back, armpits, forehead, scalp, mammæ, or angles of the mouth, occurred in 85 men, 105 women, and in 16 boys and 12 girls under fifteen years of age.—Total, 218.
2. Narrow or pointed condylomata, affecting, in men, the prepuce and glans; in women, the nymphæ and vagina, occurred in 6 men and 12 women.—Total, 18.
3. Primary ulcers of the throat and mouth occurred in 68 men, 82 women, and in 15 boys and 8 girls under fifteen years of age.—Total, 163.
4. Secondary ulcers and broad condylomata of the mouth and throat occurred in 25 men and 35 women.—Total, 60.
5. Diseases of the skin in 30 men and 21 women.—Total, 48.
6. Primary sores (chancres) on the organs of generation in 28 men and 20 women.—Total, 48.
7. Gonorrhœa and leucorrhœa in 16 men and 12 women.—Total, 28.
8. Buboes in 6 men.
9. Affections of the bones in 8 men and 16 women.—Total, 24.
10. Secondary ulcers of the extremities and various parts of the body in 18 men and 20 women.—Total, 38.
11. General syphilis in 4 men and 8 women.—Total, 12.

In the returns of preceding years, the proportion of cases of gonorrhœa and primary sores to those of condylomata is nearly the same. Condylomata were found in a great many cases, where, from minute medical examination, as well as medico-legal investigations, it appeared impossible to believe that any primary sore upon the organs of generation could have preceded their appearance. The tender age of many of the patients was in itself sufficient to rebut the idea of their being the result of sexual intercourse. Among the poorer classes, whole families sometimes occupy the same bed, and in such cases it is by no means uncommon to find all the members of the family, from the grandfather to the infant, affected with condylomata on various parts of the body.

Hospitals have been established by the Prussian government in various districts of the country for the reception of syphilitic patients. The object of these institutions is twofold; first, the immediate relief of the patient, and, secondly, the final eradication of the disease. With this latter view, each patient, on being admitted, is interrogated by the magistrate, in order, if possible, to ascertain in what manner the patient has received the contagion; but, in the generality of cases, he is unable to give any information on this head. Both man and wife may be suffering from ulcerated sore throat, and yet the organs of generation of neither will be the seat of sores or be marked with cicatrices; the older children will perhaps at the same time be found with broad condylomata round the anus, and the younger with condylomatous excrescences of the commissures of the hips and rhagades of the anus.

The peculiar features of the syphilis of Lithuania, according to Dr. Schnuhr, are, 1st, the comparatively large number of condylomata and ulcerated sore-throats; 2dly, the occurrence of these two affections as primary symptoms; 3dly, the frequent propagation of the disease by other means than by coition; 4thly, the comparatively rare occurrence of chancres; 5thly, the almost complete immunity of the lymphatic system; and, lastly, the rare occurrence of general lues, in spite of the frequent and great neglect of the primary affection.

It is supposed by Dr. Schnuhr that these peculiar features of the disease were introduced by the Russians during their occupation of the country in the seven years' war; but this idea is mere hypothesis.

Dr. Schnuhr, during the first years of his practice, employed the mercurial method of treatment; but frequent relapses induced him afterwards to abandon it for the non-mercurial plan, which he has since found to meet his most sanguine expectations. *Medicinische Zeitung.* 1837. No. 50 and 51.

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## *On Large Doses of Tartar Emetic in the Treatment of Articular Synovial Effusions.* By Dr. GIMELLE.

THIS is a new application of a very active medicine. Hydarthrosis is a very obstinate disease, and has frequently resisted the most energetic treatment; but can now be cured (according to Dr. Gimelle) with a surprising facility, without regard to the age, sex, or temperament of the patient, by administering, for a few days, large doses of tartar emetic.

The following cases will indicate the method of administration, and the beneficial effects produced.

CASE I. Picard, a soldier, thirty-six years old, of a lymphatic temperament, and a constitution much undermined by syphilis, had his leg crushed by a fall from his horse, and was obliged to walk by the aid of a wooden leg. Fifteen days after his admission into the Hotel des Invalids, he was seized with pain in the right knee: one morning the pain became so severe that he was obliged to confine himself to his bed. In a short time there was a remarkable swelling of the knee, without change of colour, with lancinating pains upon motion: the swelling increased, and fluctuation became evident. For twenty days emollient applications were used, and antiphlogistic treatment adopted. Leeches, blisters, simple and anodyne cataplasms, were all inefficacious. Dr. G. then commenced with the tartar emetic. The first day he administered four grains; the second, six grains; the four following days, eight grains; the seventh day, ten grains; and the following day, twelve grains. It produced at first some febrile action, accompanied by vomiting and purging. From this time there was a remarkable amendment; the pain entirely ceased; the remedy was tolerated; the serous effusion was absorbed, and the movements of the articulation were made with facility. On the twelfth day the remedy was suspended, and on the fifteenth the member had recovered its normal state. Ten days after this, the left knee was attacked in a similar manner: leeches were applied which subdued in some degree the inflammation; but the pain continued acute, the volume of the knee increased rapidly, and fluctuation became apparent. The tartar emetic was at this time commenced with, in the former doses, and in a similar manner, for the first three days: it produced vomiting and purging, which then ceased, and from this period the effusion was absorbed, and the cure perfected two or three days after discontinuing the remedy. Recourse was had to small doses of opium to procure tranquil nights. There was abundant perspiration, and an abundant flow of saliva.

CASE II. Clavel, forty-seven years of age, of a lymphatic temperament, had experienced for eight or ten days, without any appreciable cause, a pain in the right elbow: at the posterior part of the elbow and upon the sides of the olecranon process, there were two round tumours, fluctuating and painful to the touch. For ten days, leeches, cataplasms, blisters, mercurial and iodated frictions were tried without benefit. The tartar emetic was then given, commencing with four grains and increasing two grains every day. It was tolerated, and the cure was complete in about ten days.

CASE III. Demolombe, a soldier, of a good constitution and sanguineous temperament, had suffered from repeated venereal attacks. The knee was of an enormous size and the fluctuation was very manifest. For four days cataplasms were applied; then thirty leeches, which alleviated the pain, but the engorgement remained stationary. Two blisters were applied, but without effect. The tartar emetic was now given, but was not tolerated for the first three days; on the fourth it was, and two days after this date the swelling was diminished four-fifths, and the cure made rapid progress. At this period, for the sake of experiment, the remedy was discontinued for two days; after the interruption the cure proceeded; it then became stationary, and then retrograded. The remedy was again administered with a similar result, and the cure perfected after taking forty-eight grains of the medicine, and never exceeding eight grains for the daily dose.

The first effect of this remedy is to calm the local pain; the second, to produce absorption of the articular effusion; and the last, to remove all traces of the results of the affection. The effect is the more prompt when the remedy is tolerated; nevertheless, in the absence of this tolerance, the disease sensibly diminishes. Dr. G. recommends, if the patient is robust, to commence by general or local bleeding.

*Bulletin Général de Thérapeutique Médicale et Chirurgicale. Mars*, 1838.

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### *On the Use of Strychnine.* By M. BALLY, Physician to the Hospital of La Charité.

IN addition to the violent spasmodic contractions produced by strychnine, M. Bally has observed the following symptoms of disturbance of the cerebral circulation: an appearance of stupor, vertigo, tinnitus aurium, sleeplessness, and turgescence of the capillaries of the face. The strychnine, therefore, ought never to be employed in diseases combined with, or resulting from, determination of blood to the brain. The use of the strychnine should be restricted to cases of paralysis depending on disease of the spinal marrow; and, where this part has suffered no severe injury, the greatest benefit is often derived from its employment. One remarkable case is mentioned, of a man, about fifty years of age, paralytic for five years, who was radically cured by the internal employment of strychnine. M. Bally does not strongly recommend this remedy in cases of amaurosis, on account of their frequent complication with disease of the brain. In paralysis of the extensor muscles of the hands and feet, some benefit is derived from the administration of the strychnine by the skin; but more credit is due to time than to the remedy. In cases of colica pictonum, a combination of strychnine and hydro-chlorate of morphine has been found highly successful; one-sixteenth grain of the former with one-thirty-second grain of the latter were given in the form of pill, at first twice a day and subsequently more frequently. M. Bally's object in administering this combination is—1, to alleviate pain; and, 2, "to transform the disease of the spinal marrow into another morbid affection much less severe and more easy of cure." No success attended the use of strychnine in cases of diarrhœa and dysentery, though the nux vomica is strongly recommended in those diseases by Hufeland. With regard to the dose of the strychnine, M. Bally recommends us to begin by a twentieth or one-sixteenth, and, in case the stomach is not very irritable, by one-eighth of a grain. The dose may be increased at intervals of three or four days to one, two, three, or four grains. It is rarely necessary or prudent to surpass this last dose. Two or three grains are usually sufficient to produce the desired effects. In some instances the effects produced by the strychnine are very violent; so violent, that it has been sometimes necessary to fix the patient firmly to his bed, as the strength of two persons could scarcely hold him down.

*Bulletin Générale de Thérapeutique. February,* 1838.

### *Neuralgia of the Head, produced by a Tumour in the Brain.*

### By Professor D. Holst, of Christiania, Norway.

[In the Eighth Number of this Review we have given, from Guy's Hospital Reports, an abridged account of Dr. Bright's highly interesting "Cases and Observations illustrative of the Diagnosis where Tumours are situated in the Basis of the Brain, &c." As such cases have been hitherto very rare, we are happy to be able to communicate a similar one observed by Professor Holst, of Christiania, which was read before the Medical Society of that city, and published in the medical journal edited by Dr. Holst.*]

A lady, thirty years of age, at the end of the year 1835, began to be affected with pains in the head, soon assuming a character of great severity, apparently occupying more especially the whole galea aponeurotica capitis, but also extending downwards to the neck: the paroxysms returned at uncertain times. Many medicines were tried; amongst which, especially, quina, colchicum, and vapour-baths seemed to have beneficial effects, as the paroxysms, instead of returning several times weekly, did not recur oftener than once in four, six, or thirteen weeks. However, in the spring of 1837, after a slight catarrhal affection, they returned with the former violence. Dr. H. advised the patient to go to some warm mineral bath, such as Töplitz or Ems; but she preferred the Russian vapour-baths at Copenhagen, in which she placed much confidence. During three months' stay there, the paroxysms became complicated with atonic spasms in the muscles of the neck; she also complained of weakness of sight, not of a permanent character, nor always equally great. On her return to Christiania, she was much worse than before; the paroxysms were more frequent, more violent, and of longer duration. The pains were highly aggravated by the spasms in the muscles of the neck, by which the head was suddenly turned backwards, often to such a degree that the occiput almost touched the back, and the face was turned directly upwards. She complained now, in the intervals of paroxysms, of an obtuse but severe pain within the head, in the fore part of the brain, behind and over the right eye, occasioning the sensation as if the skull was too narrow for the brain. When walking or sitting, she kept the head fixed, as if she feared to move it, and, when lying down, she was most easy on the back. There was now also observed a permanent and complete amaurosis of the right eye, and an amaurotic amblyopia of the left likewise; and the sense of smell was so depraved that fetid things (for instance, assafœtida,) were agreeable, and fragrant scents (such as eau de Cologne) disgusting. The memory, and other mental functions, were weakened. Things remained much in the same state until the middle of December, when she was carried off by an attack of apoplexy.

The examination of the body, made thirty-two hours after death, gave the following results:—The substance of the brain was almost as soft as thick soup or gruel, especially on the right side, and most so the anterior lobe. The cortical substance was diminished in quantity, superiorly and at the sides, particularly in the right hemisphere; but at the base it was normal. There was water in all the ventricles; and, in the choroid plexus of the left lateral ventricle many hydatids. The corpora striata were soft, particularly the right, which resembled a thin jelly. The right optic thalamus had lost all natural shape, and was soft; that on the left side was also soft, but retained its form. The chiasma of the optic nerves was small, flat, soft; and in its neighbourhood, a large hydatid. The right optic nerve behind the chiasma was small, thin, soft; anteriorly flat, but not diminished in size. The left was flat, but not diminished; soft, but not in the same degree as the right. The olfactory nerves were also soft and atrophied. In the right anterior lobe of the cerebrum there was a tumour, which showed itself immediately on the removal of the dura mater from the lateral parts. It was surrounded on all sides by the mass of the brain, though unconnected with this except on its inferior aspect, where it received its vessels from the cerebral substance. It had a white colour, was of an irregular form, although approaching the quadrangular: it was composed of four lobes, and was so firm that incisions made in it with a knife gave a feeble strepitus: its largest circumference was 6¾ inches, its smallest 5¾; its largest diameter 2⅓, its smallest 1⅝, (Norway measure;) its weight 845 grains. The cerebellum was small and soft. The consistence of medulla oblongata was nearly normal.

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* The learned author of this paper is now in England, on a royal mission to examine the prisons and prison-discipline of this country; a mission from which, we doubt not, both humanity and medical science will derive profit.

The author explains, from these appearances, the various morbid phenomena—the amaurosis, amblyopia, and vitiated smell,—and is inclined to believe that some severe contusions of the head, which the deceased had suffered, both in childhood and a few years before her death, might have laid the first foundation of the tumour.

A case very similar to this, observed by O. Sandberg, physician in the Norwegian navy, is inserted in the same Number of *Eyr*. The patient, a man, suffered first from a severe neuralgic affection in the head: he became afterwards blind in both eyes, and died in an apoplectic attack. A tumour as large as a duck's egg was found in this case, situated between the anterior extremities of the hemispheres, and resting on the sella Turcica.

*Eyr*. 1837. Ellevte Binds, tredie Hefte.

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### *On an Epidemic Phthisis Pulmonalis*. By M. MENARD. (Read before the Academy of Medicine, 15th May, 1838.)

DURING the last four years, consumption has been so prevalent at Lunel and the neighbouring communes (in France,) that M. Menard has been led to regard it as epidemic. The disease generally begins by some visceral inflammation, chiefly of the stomach and intestines, the symptoms of which mask those of the pulmonary affection. Hæmoptysis is very rare, only having occurred thrice in thirty-three cases; and cough is very slight, particularly at the commencement. During the period of this supposed epidemic, the inflammatory affections of the chest, pleurisy, pneumonia, and pulmonary catarrh, were not observed more frequently than usual. On dissection, the ordinary tubercular state of the lungs is found, together with the changes belonging to the visceral inflammation; which, it is proper to observe, accompany the disease to its close.

*Bull. de l'Acad. Roy. de Méd.* 30 *May*, 1838.

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### *On the occurrence of several Spontaneous Fractures of the Ribs in a Scorbutic Individual*. By Dr. GOERDECHEN.

ON August 22d, 1834, a sailor was brought to the marine hospital of St. Petersburgh, who complained of extreme weakness, oppression at the chest, and a dry cough. On examination of the thorax, it appeared that, on the right side, the second, third, and fourth ribs were fractured near to their anterior extremities; and, on the left, the third and fourth were separated from their cartilaginous appendages: the abdominal extremities of the ribs and their cartilages crepitated audibly on external pressure, and formed, at intervals, angular protuberances by raising the neighbouring integument: in two places the fracture was oblique. All the fractured surfaces appeared smooth, and there was no indication of an attempt at reparation by the deposit of callus. There existed no external evidence of injury, nor could the patient recollect having sustained any hurt by fall or blow. He had not been permitted to quit his ship for a lengthened period previous to his admission into the hospital; and he, moreover, bore a good character for sobriety; which facts forbade the notion that an injury might have been received in some drunken brawl.

A careful consideration of his condition,—viz. the brownish-blue complexion, the melancholy and hectic aspect, the general wasting and want of power, with the feeble pulse and languid eye, the slow articulation, and the dry state and mottled appearance of the skin of the lower extremities,—suggested the inference, that the fractures, and separation of the ribs from their cartilages were alone referrible to a scorbutic diathesis; which conclusion was rendered more probable by the fact of the patient having been closely confined to his frigate during the summer months. Although careful bandaging was immediately had recourse to, and a wholesome diet enjoined, together with the use of acids and iron, and other active measures both internal and external, the ribs, notwithstanding, separated from their cartilages at four fresh places, and two more fractures occurred. Within two months after his admission into the hospital, the patient sunk under hectic and colliquative diarrhœa.

Examination of the body presented much general emaciation, but more especially of the lower extremities; and the integuments of the wasted abdomen were of a bluish-brown colour, dry and wrinkled; the gums were swollen and disorganized. There was nothing remarkable in the head. In the chest,—there was considerable wasting of the lungs, which were flaccid, and of a bluish-grey appearance: the heart and great vessels healthy. In the abdomen,—the omenta were deprived of fat, and the membranous viscera shrivelled; the spleen natural; the liver atrophied, shrunk, and of a carcinomatous hardness and sandy colour with speckled surface. An examination of the fractured ribs proved that the periosteum was stripped from their bodies to the extent of half an inch on either side of the seat of fracture; and that a sort of pouch had been formed around the fractured extremities of each bone, which was filled with a soft dark-red coagulum, containing small fragments of bone. The surfaces of fracture were rough but not splintered; and the neighbouring costal pleura presented no appearance of inflammation. At those spots where separation of the cartilages from the ribs had taken place, the same morbid changes had likewise occurred; the extremities of the cartilages being further softened, but neither remarkably rough nor thickened.

*Zeitschrift für die gesammte Medicin.* Band vi. Heft i.

## SURGERY.

### *Questionable Cause of a fatally terminating Case of Phlebitis, after Venesection.* By Dr. Spörer.

A single woman, ætat. 32, of fresh complexion, was attacked ten days before her admission into the hospital, with rigors, dry cough, and pain in the head and hepatic region: this was followed by a jaundiced aspect, and, on the night previous to admission, by delirium. When admitted, March 17, 1834, the pulse was full, incompressible, and 100; skin yellow, hot and arid, and the tongue brownish red, dry, and hard: the bowels had not been relieved for two days. A mild aperient was ordered, to be taken every two hours. The succeeding night was spent in great restlessness and delirium; pain in the head and right hypochondrium continued with increased fever: the veins of the extremities were turgid. Venesection was now performed in the right arm to about ℥x.; and sinapisms were applied to the epigastrium and calves of the legs: an anodyne mixture was also ordered, to moderate the purging which had now set in. The blood which had been withdrawn was coated with a greenish-yellow crust, and presented much green serum. On the two following days (19th and 20th), the symptoms were mitigated in some respects: the pulse sunk to ninety-five and became softer, and the tenderness of the hypochondrium had decreased; but the pain in the head was very severe, the tongue continued dry and the skin had assumed a deeper tinge of yellow. On the 21st there was increased febrile excitement with delirium; the pain in the hypochondrium had returned in full force, and the evacuations from the bowels were frequent, watery, and fetid. It was at this time (the third day after venesection) that an erysipelatous blush appeared at the bend of the elbow, surrounding the cicatrix of the lancet puncture; and, extending upwards in the course of the brachial vein, a distinct, dark streak was observed: further, there were evidences of decreasing power, in a weak, quick and small pulse, and tremulous hand. The remedies now employed were calomel. gr. i., opii gr. $\frac{1}{4}$, ter die, and small doses of acid. This obtained some temporary mitigation of symptoms, but, in spite of further treatment, the patient sunk, and died on the 25th.

Sixteen hours after death, the inflamed arm was examined. Some tea-spoonfuls of fetid pus flowed from the gaping wound where venesection had been performed. The median vein, and the brachial as far as the clavicle, were red, inflamed, and elastic like an artery. The neurilema of the median nerve was intimately adherent to the vein, and, in the vicinity of the puncture, inflamed and of a florid appearance; but the nerve presented no evidence of having been injured by the lancet. A further examination of the cavities was unfortunately not permitted; a point much to be regretted as detracting materially from the interest of the case.

Dr. Spörer remarks that, in this case, it appears very questionable whether the phlebitis could have resulted, as a secondary consequence, from injury of the nervous sheath: but he is far more inclined to regard as the cause, an inflammatory disposition on the part of the veins, in hepatitis, induced by the irritation of bile circulating with the blood. Of three cases of phlebitis following venesection, which he has treated, two did well under the free application of leeches and repeated drastic purges. In one of these, acute inflammation of the liver was likewise present; and in all three the hepatic system suffered in a greater or less degree.

[The neglect of a very simple precaution in the mode of opening a vein is, probably, a very frequent and overlooked cause of phlebitis: we allude to the propriety of making the section in a longitudinal direction, instead of either transversely or obliquely. By this plan the adhesive process is most readily promoted,—a point of much importance, as will be readily admitted, when we consider the risk which may attend the reparative process by suppuration, which is then the only alternative that remains. The facts here noticed have been experimentally verified by an intelligent French surgeon who communicated to us the result of his investigations. The practical importance, if not the novelty of this suggestion, may be deemed sufficient apology for the iutroduction of this note.]

*Zeitschrift für die gesammte Medicin.* Band vi. Heft ii.

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### *Luxation of the scapular Extremity of the Clavicle, downwards.*

By Dr. TOURNEL.

THIS case is interesting, as being, apparently, with one exception, the first of the kind hitherto detailed; and as being one, also, the possibility of which has been questioned.

A soldier was thrown from his horse. The horse fell on its rider, and, in recovering itself, placed its foot on the front of his left shoulder, where was an ecchymosis of almost the exact shape of the horse's shoe. The pressure separated the scapula backwards. The clavicle remained attached to the sternum; but, its superior and inferior and coraco-clavicular ligaments having been torn, its external extremity slipped from its articular surface beneath the acromion. The accident was regarded at first as a dislocation of the humerus, but the true nature of it was ascertained by an examination in the following manner: The summit of the shoulder was grasped with one hand, resting on the acromion, whilst, with the other hand, it was ascertained, by various motions, that the axis of the humerus was in its ordinary direction. There was no bony projection in the axilla. The left arm was somewhat longer than the right; the elbow and all the rest of the limb were in contact with the lateral part of the trunk. Voluntary movements, and especially upwards, were impracticable: communicated movements were easy, and unattended with pain. The shoulder had lost its rounded form, and there was a depression outwards, beneath the acromion. The shoulder presented, in addition, two projections; one internal and superior, formed by the acromion; the other external and inferior, formed by the external extremity of the clavicle. There was no numbness or pain of the fingers. The summit of the left shoulder was much nearer the sternum than that of the right; and, when the finger was passed along the spine of the scapula from behind forwards, as far as its acromial extremity, it was not stopped by the clavicle. This had been perfectly recognized; and it disappeared, together with the sub-acromial depression, when, the knee having been placed between the two shoulders, they were both drawn backwards: but, when this traction was discontinued, the projection, formed by the external extremity of the clavicle, and the depression, were reproduced. The association of all these symptoms leaves no room to doubt that this accident was a dislocation of the scapular extremity of the clavicle, downwards. The reduction was easily effected by placing the knee against the vertebral column, and drawing the shoulders backwards. A cushion was then placed, and retained by bandages in the axilla. The arm pressing upon the cushion was kept applied to the trunk upwards and backwards, by Desault's bandage for fracture of the clavicle. Spirituous lotions were applied to the shoulder. The fore-arm was placed in a sling, and the whole kept in position by a bandage passing round the body. The impatience of the soldier required the removal of this apparatus. Instead of it was substituted that of M. Flamant, which has the advantage of leaving the injured part uncovered. This consists of a grooved bag,* to the angles of which are sewn two rollers, and of a pad, which is placed, as above, in the axilla. The arm being placed in the bag so that the elbow corresponded to its middle angle, the roller, sewn to its anterior angle, was passed over the middle and dorsal part of the fore-arm, and continued in front of the chest. The other was continued over the back part of the arm, and crossed the former over a thick compress placed upon the uninjured shoulder. The rollers were continued in these directions for two or three turns, crossing one another over the uninjured shoulder, and beneath the elbow of the opposite side. The remainder of the rollers was then passed round the trunk, in order to fix the arm. At the elbow, the bandage was kept in its situation by four tapes, two of which were sewn to the inner side, and two to the outer side of the sac in which the elbow rested. The whole apparatus was covered by a bandage of the trunk, and the scapulary of this bandage was employed to keep resolutive compresses applied to the injured shoulder. Notwithstanding his impatience, the soldier perfectly recovered, after thirty-two days' treatment. The first use which he made of his arm was to severely castigate his horse. He remains in his regiment, and experiences neither pain in the shoulder nor difficulty in the movements of the arm.

In the "Ephémérides Nat. Cur.," is an account of a similar displacement. In both cases the cause was violence from above, and directly upon the scapular extremity of the clavicle.

*Archives Générales de Médecine. Décembre*, 1837.

### *On the Excision of Ulcers, which succeed to small Subcutaneous Abscesses of a Syphilitic and Scrophulous Nature.* By M. Bonnet.

The syphilitic ulcers here spoken of are preceded by an accumulation of pus, which elevates the skin without changing its colour. Subsequently the skin acquires a coppery hue, becomes thin, more elevated, and ulcerates at its centre. A small quantity of pus escapes, and the ulcer increases in size until it has destroyed all the detached skin. It is rounded, and its base is of a greyish colour. As these ulcers are numerous, all their stages may be examined in the same individual. They, of course, require a constitutional treatment; but if, after this treatment, they continue stationary, and refuse to heal under the employment of local means, they should be converted into simple wounds, either by cauterizing them, or, what is better, by excision. Cases are related in proof of the advantage of excising ulcers of this kind, under the circumstances above mentioned.

The author appears to have applied the same treatment to ulcers of a scrophulous character; the object being always to remove the whole ulcer, its base and borders. A great number of cases is said to have been thus treated, and with results which greatly recommend the treatment; the process of healing being very rapid when the diseased surface has been entirely removed.

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* Sac en forme de gouttière.

*Archives Générales de Médecine. Décembre*, 1837.

### *New Method of reducing Dislocations of the Os Humeri.* By M. MALGAIGNE.

SOME months since, M. Malgaigne published, in the "Bulletin de Thérapeutique," an account of a method which he practised successfully for the reduction of dislocations of the os humeri. The following is an instance of the total failure of the old method, and the success of the new:

A stone-cutter, forty-one years of age, fell from a scaffold four feet high, and dislocated his shoulder. A surgeon, who saw him immediately after the accident, having made some attempts, pronounced that the reduction was accomplished. After some time, however, the patient, finding he did not regain the use of his joint, entered the hospital Saint Louis, under the care of M. Jobert. A dislocation of the os humeri into the axilla was easily recognized—now twenty-three days after the accident. M. Jobert proceeded to attempt the reduction by the usual method of extension and counter-extension; the arm carried in the horizontal direction. The first, second, and third attempts were equally futile, the pain increasing in proportion to the efforts at reduction. The method of M. Malgaigne was then adopted. An assistant stood upon a table close to the seat of the patient, placed his foot upon the left shoulder to make counter-extension, and pulled with his two hands the dislocated arm raised to a nearly vertical direction. The reduction took place immediately almost without effort, and, above all, with very little pain. M. M. reports a case in which he succeeded after twenty-one days. These cases seem to point out the truth of his assertion, that the cicatrization of the broken ligaments, and the formation of the new capsule, is not perfectly finished till the expiration of twenty-five or thirty days.

*Bulletin Général de Thérapeutique. Avril*, 1838.

### *On a new Method of distinguishing Cataract.* By M. SANSON.

IF a light is held before an amaurotic eye, the pupil of which is dilated, either in consequence of the disease or by the action of belladonna, three images of the flame are always distinctly visible. Two of these are upright; the third is inverted. The images are situated in the following order. The one in front is upright and the brightest; the one behind is the palest, and is also upright; the third, which is situated between these, is inverted. If the light is moved from side to side, or round and round, the inverted image moves also, but always to the side opposite to the light, whilst the other images follow a uniform course in relation to the light. Hence, to observe these images, the light must be carried in various directions, observing the same positions. These three images are always seen, provided there is no disturbance of the crystalline apparatus: but, whatever may be the degree of the disease, they are not seen when cataract exists. In several cases, where patients were supposed to have had cataract, the three images were seen on employing the light; and it was determined by subsequent examination, that they were either affected with glaucoma or amaurosis.

Experiments were made to determine the causes of these reflected images; together with those on which depend the changes in their number and position. If a light is placed before the convex surface of a watch-glass, an upright image of the flame is seen. If several such glasses are placed, one over the other, the number of upright images corresponds with the number of glasses. Now, in the eye, there are two superimposed convex surfaces: 1, the cornea; 2, the anterior capsule of the crystalline lens. Thus, the two upright images are accounted for. On the other hand, if a light is placed before the concave surface of a watch-glass, an inverted image is seen. If afterwards, another glass is placed in front of it, so as to form a convex lens, two images become visible; one upright, the other inverted. But, conformably to certain physical laws, the inverted image is situated anterior to the upright one. This remark explains the situation of the third image observed by M. Sanson, and which is consequently produced by the concave surface of the posterior capsule of the crystalline lens.

It is now easy to understand how the opacity of a portion of the crystalline apparatus must destroy this phenomenon. Experiments have been made by some medical men, in order to ascertain if the absence of one or more of the images could serve as a means of diagnosticating the seat of cataract, in any particular point of the crystalline lens. These researches have not yet been conducted to any certain results; but several cases have already been witnessed, where the signs above mentioned have enabled M. Sanson to recognize amaurosis mistaken for cataract; and others, in which the absence of these images has led to the inference that cataract existed where the disease was supposed to be simply amaurotic.

[If the above observations are confirmed by the experience of others, and their application is as simple as it is beautiful, there are many cases in which they may materially assist those whose opportunities of examining diseased eyes are not very frequent, in arriving at a correct diagnosis. Sir David Brewster long ago pointed out the use of catoptrically examining the eye in diseases of the cornea. Holding a candle about fifteen inches from the eye, he observed the variations in the size and form of the image of the candles, reflected from the cornea, as he made the candle pass in different directions; and in this way detected the irregularities in conical cornea. The observations of M. Sanson afford a most important extension of the same principle.]

*Archives Générales de Médecine. Décembre,* 1837.

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## *On the Development of Adhesions of Serous Membranes, and their Application to several Surgical Diseases.* By M. BELMAS.

THIS essay was read to the French Institute. M. Belmas quoted a case of spontaneous cure of a hernia, by means of a false membrane obliterating the hernial opening. He afterwards endeavoured to effect, by art, an imitation of this spontaneous cure; and, in his communication, he asserts that he has ascertained the method of developing, at pleasure, adhesions of serous membranes, determining their nature and regulating their extent!

The first experiments were made by means of small bladders of gold-beater's skin, filled with air, and fixed by a small metallic tube in some part of the internal parietes of the abdomen of certain animals: but, as the necks of these bladders did not afford sufficient resistance to the action of the intestines, they were constantly carried away from the points of their insertion. This mode of operating was therefore abandoned; although it served as the basis of a procedure by which several cases of hydrocele were cured. In order to observe the effects produced by the presence of the above bladders of gold-beater's skin, and to prevent their rupture, they were inserted loose into the abdominal cavity of several dogs. The definitive result was, the absorption of the foreign body and adhesion of the parts which it occupied. In order to apply this fact to the obliteration of the neck of the hernial sac, after having made many experiments upon dogs affected with hernia, M. Belmas operated several times on the human subject. The observation of the results having shown him that the quantity of animal matter was far too considerable, he tried the action of simple filaments of dried gelatin, covered by small pieces of gold-beater's skin. Having now ascertained the means of developing linear adhesions between serous surfaces by means of this new agent, he inserted these filaments, by a needle constructed for the purpose, into the neck of the hernial sac. Five cures have been effected out of ten cases, to which this mode of operating has been applied. In three cases there was partial relapse, and in two the disease speedily returned: but, adds the author, as not one of these attempts was attended by the slightest accident, it is to be hoped that more dexterity in the performance of the operation, and a more regular compression upon the ring by means of well-adapted compressing apparatus, will lead to more satisfactory results.

[The facts contained in this communication (although we lament that the journal from which they are extracted has not entered into a little useful detail of the steps, duration, &c. of the operation in the ten cases alluded to,) seem to establish the conclusion that, by the introduction of some foreign body, a sufficient amount of adhesive inflammation may be safely excited on the serous surface of hernial sacs to prevent the reescape of their contents from the abdominal cavity. If the gelatine filaments employed by M. Belmas are equally efficacious, they appear to be far preferable to the double row of pins recommended by M. Bonnet for the same purpose.]

*Revue Médicale française et étrangère. Décembre,* 1837.

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### *Reduction of an old Luxation of the Elbow.* By M. MALGAIGNE.

M. MALGAIGNE has lately reduced, with M. Lisfranc, a luxation of the elbow, backwards, which had existed three months and twenty-one days, in a child ten years of age. "The nature and age of the luxation in so young a child were probably the causes of this success," says M. Malgaigne; "a fact without a parallel in the history of surgery, and one which should reassure the surgeon against the fear of breaking the epiphyses, when he makes use of an appropriate apparatus." These two surgeons employed direct traction with pulleys, and at one time carried it to a force of 300 pounds. The reduction was afterwards accomplished by a novel procedure, which consisted in drawing the arm and fore-arm backwards, whilst the olecranon was pushed with the knee gently forwards and downwards.

*Ibid. Décembre,* 1837.

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### *Ligature of the Carotid Artery of a Child aged Fifteen Months, for the Cure of a Vascular Nævus.* By DR. ZEIS, of Dresden.

A. D. was born with a vascular nævus on the left cheek, which gradually increased in size, notwithstanding the employment of various remedies; and when seen by Dr. Zeis, in the fifteenth month, extended anteriorly across the cheek to within a short distance of the angle of the mouth, upwards as high as the zygoma, and backwards behind the ear involving the lobe. The central and more elevated part exhibited enormous dilatation of the capillary vessels, some of which were of a bright red, others of a deep blue colour. The child's general health was good, with the exception of some irritation produced by dentition.

The extirpation of the tumour presented so much difficulty and danger, that Dr. Zeis determined on tying the carotid artery, and having given a full trial to astringents, without any satisfactory result, he proceeded to the operation, which was performed on the 30th August. The different steps of the operation are minutely described; but, although rendered difficult by the confined space, presented nothing remarkable. After much trouble, Dr. Z. succeeded in passing a blunt ligature needle, bent laterally from the handle, under the artery. A portion of cellular membrane which lay over the head of the needle was divided; a strong silk thread introduced, and, having satisfied himself that there was no nerve or other part included, the needle was withdrawn and the ligature knotted.

During the operation the child screamed and lay quietly by turns, and the temperature of the body had somewhat diminished, although the loss of blood amounted only to a few ounces. Towards the end of the operation she began to sob, and when the ligature was tightened, broke out into a violent fit of screaming, during which her voice appeared somewhat altered, and hoarser than usual. The wound was covered with a fold of lint dipped in creosote water, and united by means of strips of adhesive plaister. In the evening, the pulse was 108, with increased thirst, and restlessness alternating with sleep. She was ordered an antifebrile mixture with small doses of calomel, and in a few days everything went on favorably.

For some time after the operation, the little patient was attacked with fits of coughing whenever she swallowed anything; this symptom, however, disappeared after the eighth day. Her respiration was at all times easy and natural, but her voice remained for a long time hoarse and squeaking, and it never regained the strength or clearness of tone which it had possessed before the operation. It was also remarkable that she had a habit of sobbing frequently, which she had never exhibited previously, and for which no cause could be assigned. These phenomena, according to Dr. Z., appeared to show that a nervous twig was included in the ligature.

On the morning of the eighth day, Dr. Z. cut short the ligature applied to the carotid, with the view of allowing the wound to heal; and having been obliged to remove the dressing in the evening, in consequence of its being soaked with blood, he was surprised to find the little loop of the ligature lying in the lower angle of the wound. Its early detachment was in all probability favoured by an attack of vomiting which the child had about this period. After this, the wound filled up rapidly, with the exception of a single point, from which matter continued to be discharged until the 11th of November, when complete cicatrization took place.

Immediately after the ligature was tied, the tumour collapsed greatly, but began to swell a little again in the evening, when the temperature of the body rose. It afterwards gradually increased, and in a few weeks attained a very considerable volume, but never exceeded its original size, and the portion behind the ear entirely disappeared. On the 3d of November, a few days before the wound was completely cicatrized, the child was suddenly attacked with convulsions, and hemiplegia of the *right* side. These symptoms were followed by increasing debility, emaciation, and exhausting perspirations, and the little patient died on the 22d of December, sixteen weeks after the operation. For several days before death, she laboured under tonic spasms alternately of the flexor and extensor muscles of the right extremities. The nævus had completely disappeared, leaving behind only a few loose folds of skin. The parents would not permit an examination of the body.

*Zeitschrift für die gesammte Medicin*, iii. Band; i. Heft.

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## *Case of Abscess beneath the Pectoralis Major, communicating with the Cavity of the Chest.* By M. Boinet.

This was the case of a young woman, æt. 15, who was received into the Hôtel Dieu, under the care of M. Blandin. Having severe pain on the right side of the chest, she was supposed to have pleurisy, and was treated accordingly. In a few days considerable tumefaction occurred in the axilla and fore part of the chest, and the presence of pus being evident, an incision was made, from which a large quantity of pus escaped in jets during expiration, whilst during inspiration air entered with a hissing sound: at the same time, a remarkable gurgling sound was heard within the chest, even at the distance of two feet from the patient. A silver catheter, introduced into the incision, readily entered the cavity of the chest to the depth of five or six inches. This communication, however, could not be discovered on the following days. The symptoms were at first somewhat ameliorated after the opening of the abscess, but they soon became worse, and the patient died about ten days afterwards. Percussion had, during life, indicated a clear sound in front to the distance of two inches and a half from the sternum; externally to this, on the side of the chest, there was complete dulness at the level of the sixth and seventh ribs, which gradually diminished both above and below. The respiratory murmur was but little affected. On the left side, the sound from percussion, as well as the respiration, were natural.

*Dissection.* Between the pectoralis major and the eight first ribs is a large cavity extending backwards into the axilla: it contains no pus, and presents a dry and blackish appearance. In the intercostal spaces, between the second, third, and fourth ribs, are nine or ten fistulous openings communicating with the chest; the largest about the size of a fourpenny piece. There are recent but firm adhesions between the lung and the ribs, separating the right side of the chest into two cavities, which communicate with each other by an opening capable of admitting the finger, and through which it is probable that the catheter passed on the day that the abscess was opened. The costal surface of the lung is covered with a thick false membrane. The pulmonary tissue is gorged, but is firm, and does not appear inflamed. The left lung is perfectly healthy, and the other viscera do not present any remarkable pathological appearance.

*Gazette Médicale. May*, 1837.

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### *Successful Application of Creosote in an old Ulcer of the Vagina.*

A female had, in 1833, a syphilitic ulcer of the vagina, which was treated with mercury. The ulcer healed, excepting a part of it on one side of, and beneath, the orifice of the urethra. Again she was mercurialized. In 1834, she was treated by Dr. Bürkner: the ulcer was about the size of a penny, and a quarter of an inch in depth. Regarding it as an atonic fistulous ulcer, it was treated according to the usual rules in such cases, but without effecting any change in it. Creosote was then employed, and applied, by means of charpie, directly to the sore. At first it was used twice, and subsequently once daily. The secretion of pus immediately improved and cicatrization commenced, whilst granulations were formed in the depth of the sore: the patient was completely healed in four weeks.

*Ibid.* No. 36. 1837.

## MIDWIFERY.

### *Report of two Cases of Pregnancy requiring Incision of the Os Uteri to allow Delivery.*

I. By Dr. **Burdach**, of Finsterwalde. *Cartilaginous State of the Os Uteri.*

A strong robust woman, æt. 28, pregnant for the first time, felt the pains commence on the 24th of June, and the membranes broke in the course of the day. The midwife was unable to reach the os uteri, and, although the pains in some degree continued, no progress had been made towards delivery on the 26th. Borax, in doses of six grains, were then administered, with a view of increasing the pains; but, although this object was attained, no further progress was made. On the 27th, Dr. Burdach was called in; and found, on examination, the anterior lip of the os uteri hard and callous. Vapour-baths, and the application of an opiate ointment to the os uteri, were ordered, and continued till the forenoon of the next day; when matters remaining precisely in the same condition, an incision of one inch and a half was made into the anterior lip. Only a small quantity of blood escaped. Doses of eight grains of borax, with one of musk, were given to increase the pains: but, matters proceeding slowly, the forceps were applied, and a dead child extracted, without any tearing of the incision of the os uteri. The wound healed kindly, and the woman made a good recovery.

II. By Dr. **Gruhn**, of Reppen. *Prolapsus Uteri.*

A strong, robust woman, æt. 28, had enjoyed good health during the first four months of pregnancy. At this time, whilst raising a heavy weight, she had the feeling as if something had given way in the abdomen, and shortly afterwards the uterus began to protrude from the vagina, and continued to do so till the end of her pregnancy. When Dr. Gruhn arrived, she had already been in labour for thirty-six hours, and twenty-four hours had elapsed since the waters had come away. The woman was lying on her back in bed, in a very low, dispirited state, with the uterus, containing the fœtus, between her thighs: its longitudinal diameter was equal to about six inches, and the transverse diameter to eight inches. The vertex of the child presented, and the os uteri had dilated to the size of half a dollar. All attempts to dilate it further proving abortive, an incision, three inches in length, was made into one side of the os uteri, and a fully developed though dead child extracted. The placenta followed immediately, accompanied by a gush of blood, which greatly reduced the patient. The flooding was stopped by injections of cold water into the uterus; and the accoucheur, introducing his hand into its cavity, proceeded to reduce it. The operation was successful: no further flooding followed, the milk was secreted, and the lochial discharge appeared. The woman was recommended to wear a pessary, but neglected to do so on the plea of inconvenience. *Medicinische Zeitung.* 1837. No. 39.

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### *Singular Case of Abortion.*

Dr. Erbkam, second physician of the Obstetric Clinical Institute of Berlin, has described a very remarkable case of abortion in the fourth month, where the motions of the fœtus continued strong for some time after birth. Finding, upon being expelled, that it moved, he tied the cord, and placed it in warm water. The movements, which consisted in drawing up the feet and arms, turning the head from one side to another, and opening the mouth as if endeavouring to breathe, continued for half an hour. The action of the heart was visible for ten minutes after all other movements had ceased: as soon as fresh warm water was added, these motions again returned. The length of the fœtus was six inches, its weight eight ounces; the head was sufficiently developed, and the cranial bones considerably advanced in the process of ossification; the eyes were closed. From a superficial inspection of the external genitals, it might have been taken for a female fœtus, there being well-marked labia, between which a large clitoris projected: on opening the abdomen, however, the testes were found in its cavity. The fœtus was shewn to Professor J. Müller, who expressed it as his conviction that the fœtus was not more than four months advanced.

*Neue Zeitschrift für Geburtskunde.* B. v. H. 2.

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### *Inversion of the Uterus resembling Polypus.* By M. Fleury, Jun.

A woman, aged forty-six, mother of five children, and who had always had easy deliveries, enjoyed good health up to the occurrence of the disease under which she now labours. Three years since a tumour first made its appearance in the vagina; this was preceded by a discharge of a red colour which had existed eighteen months. When the tumour first made its appearance externally, a leucorrhœal discharge took place so abundant as speedily to exhaust the strength of the patient. A portion of the tumour was excised in the month of August, 1836, (about two years after the first appearance of the disease,) and she experienced great relief; the discharge diminished greatly, but the amendment was of short duration; and at the same time that the tumour regained its former size the discharge re-appeared in the same quantity as before. In the month of February, 1837, she was received into the Hôtel Dieu at Clermont. At that time her constitution seemed exhausted, her face bore traces of great suffering, and her skin had that strongly marked straw colour, which, at first sight, seemed to be the effect of cancerous cachexy. The finger, introduced into the vagina, discovered at once a pyriform tumour with its base inferior. The circumference both of the base and pedicle was easily traced, but when the finger reached the summit of the tumour it was found quite impossible to penetrate the uterus. It seemed as if the pedicle was inserted into the whole circumference of the os tincæ, or that it had formed attachments to it. Its texture was firm and resisting, and it was quite insensible to the touch. The red discharge still continuing, and further delay appearing dangerous to the patient, an operation was determined upon. The patient was placed upon a low table, in the same position as in the operation for the stone. A pair of uvula forceps was then introduced into the vagina, and passed upon the fore-finger of the left hand up to the tumour, which was easily seized and brought to the os externum by gentle and continued pulling. Its colour was a pale red, its surfaces quite smooth, and its form that of the body of the uterus. The thumb and fore finger carried beyond the pedicle of the tumour were separated from each other only by the sides of the vagina applied the one against the other. No doubt remained that this tumour was the inverted uterus, for had it been a polypus, the neck of the uterus would have been found around and above the pedicle. On applying the finger to the upper part of the tumour the pulsations of arteries, apparently of considerable size, were plainly perceived. The exposed surface, then, was the internal face of the uterus, which had become external and was the source of the sanguineous discharge. The patient made no complaint and suffered no pain during the operation; even when the uterus was between the blades of the forceps its structure seemed quite insensible. The diagnosis having been thus clearly established, what treatment was to be pursued? Ought an attempt to be made to replace the inverted viscus? was a ligature to be applied? or ought the uterus to be left to itself? The last alternative was adopted; a little charpie was introduced into the vagina with a view of exercising a slight pressure on the uterus, and thus lessening the size of the tumour. The result of this simple treatment, however, could not be known, as the patient soon left the hospital. The question naturally suggests itself, what was the nature of the tumour which was extracted in the summer of 1836? Was it a polypus attached to the body of the uterus or a portion of the uterus itself? Had it been a portion of the uterus itself that organ must have lost its regularity, and had it been a polypus some traces of cicatrization might be expected: they did not, however, exist. It is most probable that the tumour removed was a polypus, and that the attempts to extract it had caused inversion of the uterus.

[This case furnishes a marked exception to a diagnostic rule generally laid down, that in inversion of the uterus the tumour is painful to the touch, in case of polypus insensible. The sensibility of the uterus is perhaps overrated: a case related by Dr. Sparman in his travels in the Gape of Good Hope would lead us to think that it has a very low degree of sensibility. He says that he met with a native who had had prolapsus of the uterus, and finding the tumour troublesome had pulled it away piecemeal. We will not pretend to fix the degree of credit which should attach to narratives of cases of disease contained in books of travels, but as Sparman was a physician we feel justified in quoting his authority.]

*La Presse Médicale.* No. 58. Tom. i. 1837.

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## *Remarkable Case of Embryotomy on account of Exostosis of the Pelvis.*

### By Dr. KYLL, of Cologne.

THE patient was a robust peasant woman, æt. forty-five, mother of seven children: her former labours had presented nothing unusual, except that in her last, which occurred in her forty-second year, medical assistance had been required to remove the after-birth, which the practitioner declared had formed an usually firm adhesion to the uterus. She experienced no ill effects from its extraction, beyond suffering a good deal of pain at the time. On the sixth day after labour, she was seized with feverish symptoms and violent pain, at the spot where the placenta had been removed. The attack yielded to proper treatment, but she continued feverish at night, with perspirations, bowels frequently deranged, difficulty in passing water, with severe pain in the abdomen, especially when she tried to stand on the right leg. An abscess formed in the right groin, which was opened, and discharged a large quantity of pus. Her recovery was very slow; she became extremely emaciated, and did not leave her bed for seven months. On getting up for the first time, it was observed that she limped, and the right leg was evidently shorter than the left; but, with this exception, she completely recovered her health and strength, and again became pregnant three years after. On labour coming on, the midwife could reach no presenting part; but, after some hours, when the os uteri had dilated considerably, a foot presented, and shortly after the other descended also; and, after long and severe pains, they approached the os externum, but no extractive force could make them advance further. During the night, a coil of the cord prolapsed: it was without pulsation; but it was not until the following morning that the patient could be induced to permit a medical man to be sent for.

On his arrival, Dr. Kyll found, upon external examination, the uterus very high and inclined to the left side. On examination per vaginam, he found the child resting with the hips on the brim of the mother's pelvis, and completely wedged fast by a hard tumour, which sprung from the upper part of the sacro-iliac symphisis: it was about the size of a small hen's egg and immoveable. Below this projection, the pelvis was sufficiently spacious. From these circumstances, it seemed evident that, during her last confinement, she had suffered from pelvic abscess, which had, in all probability, caused this exostosis. The child being evidently dead, the perforation was determined on.

Dr. K. first of all endeavoured to bring the pelvis of the child through the superior aperture, by pulling firmly at the feet, and succeeded at length in bringing it as far as the breast, beyond which it would not stir: he therefore opened the abdominal cavity with a bistoury, passed his hand through the diaphragm, and evacuated the contents of the thorax. The thorax now descended, but the two arms had become turned up on each side of the head. As there was not sufficient space to bring down the arms, Dr. Kyll removed them at the shoulder-joints with a pair of scissors: he then perforated the head at the occiput, in the hopes of bringing it down when the bones collapsed. As, however, he found that this was impossible, owing to the breadth of the basis cranii, the only means of bringing it away would be by reducing the size of the head as much as possible, and then bringing it down with the occiput foremost, and the face upwards, through the left oblique diameter of the brim.

The head was very far from being sufficiently diminished by perforation; the narrow space required that its perpendicular diameter should be lessened, in order that the vertex should not be stopped by the anterior wall of the pelvis. In order to effect this, it became necessary to remove the body of the child, in order to gain sufficient space to get at the head. This was done close to the foramen magnum, by means of the scissors, leaving the head, which rolled about upon the brim of the pelvis with every movement of the mother. To diminish the size of the head, Dr. K. introduced his right hand to the left side of the pelvis, fixed the head by the left hand on the outside, and placed it with the face looking upwards, the sagittal suture turned to the left, and then pushed it as much as possible to the right side of the pelvis to gain sufficient space for his right hand; and, in order to fix the head in this position, he passed a sharp hook into the foramen magnum, and held it with the left hand. He now introduced a bistoury with his right hand, passed it up to the anterior fontanelle, which he cut through, and then divided the cranial integuments the whole length of the sagittal suture. Having withdrawn the knife, he removed first one and then the other parietal bone: this was the most difficult part of the whole operation, but it succeeded completely. He took this opportunity of removing the rest of the brain; and, having thus lessened the head, he placed it in the left oblique diameter of the pelvis, with the face upwards, and brought it through the pelvis without any exertion. The uterus contracted well, the placenta was easily expelled, and the patient recovered very quickly.

Immediately after the labour, he examined the tumour carefully, and felt convinced that it was an exostosis growing from the right sacro-iliac symphisis.

*Neue Zeitschrift für Geburtskunde.* B. v. H. 1.

## MEDICAL STATISTICS.

### *Statistics of the Lunatic Asylum at Aversa, (Naples.)* By FILIPPO VOLPICELLA.

*Proportion of the Sexes.* THE proportion of male inmates to the females is usually about two to one: thus, in 1833, out of 640 insane, 429 were males and 211 females, who were thus distinguished:

| | | |
|---|---|---|
| Maniacs . . . | Males, 73 | Females, 50 |
| Monomaniacs . . | ... 207 | ... 87 |
| Fatuous . . . . | ... 60 | ... 40 |
| Idiots . . . . | ... 58 | .. 23 |
| Epileptic, with delirium | ... 31 | ... 11 |

A little less than half the insane are monomaniacs, and a fifth part maniacs; of these, the chance of cure is greatest: there is but little hope of the cure of the fatuous, still less of the idiots, and none at all of the epileptic.

*Occupation.* With regard to the occupations of the men, there were 35 priests, 9 monks and friars, 5 public officers, 45 soldiers, 7 tradesmen, 70 gentlemen having property, 4 advocates, 2 schoolmasters, 6 copyists, 6 students, 10 sailors, 58 artisans, 124 peasants, 14 house-servants, 9 porters. Of the women, 2 were nuns, 37 gentlewomen, 88 artisans, 71 peasants, 13 servants.

*Causes.* As far as could be ascertained, these were as follows:

*Physical Causes.* Masturbation, male 3; blindness, male 1; venereal excess, male 12; excessive drinking, male 27, female 12; starvation, male 4; insolation, male 2; repelled eruptions, male 6; repelled lactation, female 2; suppressed and irregular menstruation, 28; suppressed gonorrhœa, female 2; syphilis, male 1; piles, male 4; gout, male 2; fever, male 3, female 1; hysteria, 2; encephalitis, male 1; apoplexy, male 9; epilepsy, male 25, female 8; congenital idiocy, male 8, female 2; hereditary madness, male 1; chronic headach, male 1.

*Moral Causes.* Natural insuperable sadness *(tristezza)*, male 48, female 25; wounded vanity, male 8, female 1; disappointed ambition, male 12, female 4; regrets, male 10, female 2; rage, male 3, female 1; despair, male 1; discouragement *(avvilimento)*, male 2, female 1; fear, male 15, female 9; terror, male 13, female 3; hatred, male 5; religious scruples, male 19, female 13; disappointed hope, male 3, female 2; deluded hope, male 4, female 1; remorse, male 1; infidelity, conjugal, male 4, female 1; jealousy, male 21, female 18; disappointed love, male 28, female 21; nostalgia, male 5; excess in study, male 1; excessive labour, male 1; family quarrels, male 11; domestic anxieties, male 2; ruined fortune, male 24; poverty, male 52, female 31; exalted imagination, male 6, female 3; death of relatives, male 5, female 7; attendance on the insane, male 1; depraved habits, male 4; sensuality, male 8, female 11.

*The following is a Table of the per centage of Cases, Cures, and Deaths, for the two decennial periods between* 1813 *and* 1832.

| | Periods | Jan. | Feb. | Mar. | Apr. | May | June | July | Aug. | Sep. | Oct. | Nov. | Dec. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Cases, | 1 | $5\frac{6}{10}$ | $4\frac{4}{10}$ | $6\frac{3}{10}$ | 8 | $9\frac{8}{10}$ | $11\frac{3}{10}$ | $10\frac{6}{10}$ | $12\frac{4}{10}$ | $8\frac{6}{10}$ | $7\frac{8}{10}$ | $6\frac{8}{10}$ | 8 |
| | 2 | $5\frac{1}{10}$ | $7\frac{3}{10}$ | $7\frac{7}{10}$ | $7\frac{3}{10}$ | $11\frac{1}{10}$ | $11\frac{3}{10}$ | $12\frac{8}{10}$ | $10\frac{8}{10}$ | $8\frac{4}{10}$ | $8\frac{3}{10}$ | $5\frac{5}{10}$ | $4\frac{6}{10}$ |
| Cures, | 1 | $7\frac{8}{10}$ | $2\frac{6}{10}$ | $6\frac{8}{10}$ | $5\frac{8}{10}$ | 5 | $8\frac{4}{10}$ | $11\frac{1}{10}$ | 8 | $12\frac{1}{10}$ | $12\frac{4}{10}$ | 9 | $10\frac{8}{10}$ |
| | 2 | $5\frac{8}{10}$ | $6\frac{7}{10}$ | $11\frac{6}{10}$ | $10\frac{6}{10}$ | $7\frac{3}{10}$ | $5\frac{6}{10}$ | $11\frac{5}{10}$ | $8\frac{6}{10}$ | $8\frac{8}{10}$ | $10\frac{3}{10}$ | $9\frac{3}{10}$ | $4\frac{6}{10}$ |
| Deaths, | 1 | $14\frac{4}{10}$ | $7\frac{4}{10}$ | $9\frac{6}{10}$ | $4\frac{6}{10}$ | $5\frac{2}{10}$ | $3\frac{8}{10}$ | $5\frac{3}{10}$ | $9\frac{3}{10}$ | $7\frac{8}{10}$ | 9 | 12 | $11\frac{7}{10}$ |
| | 2 | $13\frac{8}{10}$ | 12 | $7\frac{8}{10}$ | $7\frac{4}{10}$ | $1\frac{6}{10}$ | $6\frac{7}{10}$ | $7\frac{8}{10}$ | $6\frac{8}{10}$ | $6\frac{4}{10}$ | $6\frac{3}{10}$ | $8\frac{4}{10}$ | $10\frac{1}{10}$ |

The admissions, in the course of these twenty years, have been—men 2,775, women 1,122: total 3,897. The readmissions, from relapses, were—men 22, women 39; total 61, (131 in the paper.) There have escaped, of men 70, women 1; total 71. Relieved and intrusted to their friends, men 369, women 179; total 548. Cured—men 1,072, women 442; total 1514. Dead—men 909, women 313; total 1,222. The number admitted, and of those previously in the hospital, are, to the cured, as 1 to $2\frac{67}{100}$ of the males, and as 1 to $2\frac{88}{100}$ for the females: to the dead, as 1 to $3\frac{14}{100}$ for the males, and as 1 to $3\frac{71}{100}$ for the females. [This is evidently a mistake: it should be, the proportion of cures, &c. to admissions and patients already in confinement; but, for want of the latter element of the computation, or even an average of annual admissions, I cannot prove the correctness of the assertion.—*Translator's note.*]

The deaths for one year were—sanguineous apoplexy, males 14, females 8; apoplexy supervening upon a violent epileptic attack, males 7, females 1; serous apoplexy, females 5; aneurism of the heart, male 1, females 2; angina œsophagæa, male 1; convulsive asthma, male 1, females 4; synochus, males 3, female 1; colliquative diarrhœa, males 8, females 2; dysentery, males 7, females 11; nervous phthisis, males 9, (perhaps marasmus is meant;) phthisis pulmonalis, males 10, females 3; phthisis mesenterica, males 8, females 7; intestinal phthisis, (probably ulceration of Peyer's glands,) female 1; hæmoptysis, male 1; gangrene from lying, male 7; pneumonia, males 2, females 2; hydrothorax, male 1, female 1; ascites, male 1, female 1; anasarca, males 3, females 3. Total males 84, females 52. *Filiatre Sebezio.* 1836.

### *Statistics of Insanity in Westphalia and Saxony.*

The number of cases of congenital idiocy in the Prussian province of Westphalia amounted, in 1834, to 454 males and 274 females; together to 728: equal to 47 per cent. of the total number of cases of insanity in the province, which amounted to 1,535, or to one in 846 of the population. Of this number 507 were males, giving one case for every 712 male inhabitants, and 339 females, or one for every 1010 of the female population. Classed according to religious denomination, there was one case for every 962 of the members of the reformed church, one for every 786 of the Roman catholic population, and one for every 758 of the Jewish population.

In the province of Saxony there were, in 1836, 1,580 insane in a population of 1,529,607, or one in 968. Of this number 882 were males, and 698 females: namely, unmarried, males 732, females 526; married, males 114, females 86; widowers 36, widows 86. Classed according to age, the numbers were—under fifteen years of age, 85 males and 57 females; between fifteen and thirty, 286 males and 212 females; between thirty and forty-five, 289 males and 226 females; between forty-five and sixty, 177 males and 147 females; and, lastly, above sixty years of age, forty-five males and fifty-six females.

It is a matter of extreme difficulty to decide with certainty upon the cause of the disease in a vast number of cases, but it often happens that a probable cause may be assigned. In this way, corporeal causes were assumed in 196 males and 137 females; mental causes in 127 males and 114 females; hereditary predisposition in 119 males and 66 females; and, lastly, no probable cause could be assigned in 440 males and 381 females. Under the head of dementia were classed 657 males and 485 females; under that of monomania, 178 males and 177 females; and under that of mania, 47 males and 36 females. The first class comprehended 118 epileptics, the second 32, and the third 12. Of the total number, 597 belonged to towns, and 983 to the country.

*Medicinische Zeitung.* No. 47. 1837.

### STATISTICAL NOTICE OF THE CHOLERA-EPIDEMIC IN BERLIN, IN THE YEAR 1837.

BY DR. CASPER.

| Weeks. | | Sick. | Dead. |
|---|---|---|---|
| 1. | August 12-18 | 117 | 65 |
| 2. | 19-25 | 575 | 305 |
| 3. | 26, September 1 | 693 | 395 |
| 4. | September 2-8 | 573 | 335 |
| 5. | 9-15 | 486 | 331 |
| 6. | 16-22 | 513 | 289 |
| 7. | 23-29 | 254 | 181 |
| 8. | 30, October 6 | 128 | 120 |
| 9. | October 7-13 | 65 | 42 |
| 10. | 14-20 | 59 | 37 |
| 11. | 21-27 | 44 | 32 |
| 12. | 28, November 3 | 29 | 18 |
| 13. | November 4-10 | 15 | 16 |
| 14. | 11-17 | 6 | 5 |
| 15. | 18-24 | 3 | 1 |
| 16. | 25 | 1 | 2 |
| | Total | 3561 | 2174 |

The cholera-epidemic of 1831 prevailed, in Berlin, during a period of forty-six days longer than that of 1837. The numbers, in 1831, were—*taken ill*, 2271; *died*, 1426; being less than in 1837 by 1290 and 748 respectively, and making the proportion of deaths to the sick—in 1831, 0.372 : 0.628; in 1837, 0.391 : 609.

*Wochenschrift f. d. ges. Heilkunde. Feb.* 10, 1838.

## STATISTICS OF SUICIDE IN LONDON. BY M. MOREAU DE JONNÈS.[*]

### I. *Number of Deaths from Violence, in general.*

| | 1824. | 828. | 1829. | 1830. | 1831. |
|---|---|---|---|---|---|
| Suicide . . | 52 | 41 | 35 | 25 | 48 |
| Executed . | 10 | 1 | 26 | 7 | 6 |
| Murdered . . | 2 | 6 | 4 | 2 | 5 |
| Poisoned . | 4 | 27 | 7 | 4 | 7 |
| Found dead . . | 5 | 15 | 6 | 13 | 5 |
| Drowned . | 149 | 150 | 36 | 97 | 131 |
| Burnt . . | 36 | 47 | 53 | 61 | 35 |
| From famine . | 1 | 1 | 0 | 0 | 1 |
| From excessive drinking | 5 | 7 | 3 | 4 | 0 |
| From suffocation . | 5 | 10 | 10 | 5 | 5 |

### II. *Number of Suicides in London, during a century and half.*

| | | | |
|---|---|---|---|
| From 1690 to 1699 . | 236 | From 1760 to 1769 . | 351 |
| 1700—1709 . | 278 | 1770—1779 . | 339 |
| 1710—1719 . | 301 | 1780—1789 . | 224 |
| 1720—1729 . | 478 | 1790—1799 . | 274 |
| 1730—1739 . | 501 | 1800—1809 . | 347 |
| 1740—1749 . | 422 | 1810—1819 . | 362 |
| 1750—1759 . | 363 | 1820—1829 . | 381 |

### III. *Number and Proportion of Suicides in the chief Capitals of Europe.*[†]

| Places. | Periods. | Suicides. | Proportion to Population. |
|---|---|---|---|
| Berlin . . | 1813—1822 | 360 | 1 in 750 |
| Copenhagen . | 1804—1806 | 100 | 1 — 1,000 |
| Naples . . | 1828 | 330 | 1 — 1,100 |
| Hamburg . . | 1822 | 59 | 1 — 1,800 |
| Berlin . . | 1799—1808 | 60 | 1 — 2,300 |
| Paris . . | 1836 | 341 | 1 — 2,700 |
| Milan . . | 1827 | 37 | 1 — 3,200 |
| Berlin . . | 1788—1797 | 35 | 1 — 4,500 |
| Vienna . . | 1829 | 45 | 1 — 6,400 |
| Prague . . | 1820 | 6 | 1 — 16,000 |
| Petersburgh . . | 1831 | 22 | 1 — 21,000 |
| London . . | 1834 | 42 | 1 — 21,000 |
| Naples . | 1826 | 13 | 1 — 27,000 |
| Palermo . . | 1831 | 2 | 1 — 173,000 |

"From this it appears," says M. M. de Jonnès, "that the inhabitants of London are much less inclined to suicide than those of most of the cities of Europe: consequently, that the notion that the climate of England predisposes to suicide is altogether erroneous." *Gazette Méd. de Paris.* 24 *Feb.* 1838.

# FORENSIC MEDICINE.

## *Murder by Carbonic Acid.* By M. DEVERGIE.

[THE rare circumstance of murder being attempted or perpetrated by an aërial poison has induced us to select the following case, as one possessing some medico-legal interest.]

In February, 1836, a woman residing in the Rue Descartes, Paris, was found dead in her apartment under suspicious circumstances. Her husband was accused of having murdered her. The statement which he made relating to the death of his wife, was that they both wished to destroy themselves. After their usual evening meal they retired to their room, which was small and ill-ventilated. A charcoal-fire was lighted in the middle of the room, and they placed themselves close to it, so as to inhale the vapour of the burning fuel. The deceased fell asleep, and in the course of a few hours he found that she was quite dead: while he himself was not affected by the vapour. He procured more charcoal, and continued to burn it in the room, still closed up, for five days and five nights successively, sitting, during that time, by the side of the body of his wife. He took no food and only a small quantity of liquid to satisfy his thirst. He occasionally had two charcoal-fires lighted at once, for the purpose of producing more vapour; and placed himself between them, in order more effectually to suffocate himself, but without success. Such was the account given by the prisoner. The charge laid against him, was, that he had destroyed his wife and that he had concealed her body in the room for four or five days.

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[*] From an unpublished work on the Population of London.

[†] Compare this with the tables given at pp. 371-2 in the last Number.—ED.

The medical men to whom the investigation was intrusted, found the body of the deceased on the floor of the room with the head reclining against the foot of the bed. It was much putrefied. The face was swollen and of a greenish black colour, an appearance which was also observed on the trunk and limbs. The skin of the neck was puffy and discoloured: there was no trace of any impression of a ligature, nor were there any marks of violence. On a close inspection, no violence or mechanical injury was to be seen either externally or internally. The viscera were in a high state of decomposition: the lungs were gorged with blood. The contents of the stomach, on analysis, presented no trace of poison. The quantity of food found in the stomach, however, was not such as to correspond with the prisoner's declaration, that his wife had made a full meal just before her death.

Several marks of violence but of an unimportant nature, were found on the person of the *prisoner*. The medico-legal questions required to be answered were as follows:

1. *Whether the room in which the deceased's body was found was such as to allow of her being suffocated by charcoal-vapour, in the manner stated by the husband?*

This question was answered in the affirmative, for the following reasons: Although the apartment was not so perfectly closed as entirely to exclude the external air; yet, as the witnesses very properly contended, this condition is by no means necessary to death from carbonic acid. If the access of air were even more free than it was shewn to have been in this case, still so soon as the air of a room becomes impregnated with the gas in the proportion of about ten per cent. by volume, that is enough to bring on all the effects of poisoning or suffocation. Thus, then, even a door or a window of an apartment might be partially open, and yet an individual perish from this cause. In the case of the deceased the closure of the room, judging from the prisoner's own description, was sufficiently perfect easily to account for her death in the manner supposed.

2. *Whether it was possible that the deceased should have died from this form of asphyxia, without the prisoner himself at the same time experiencing the deleterious effects of the gas?*

A decided answer in the negative was returned, for the reason that carbonic acid is equally fatal, when respired, to both sexes,—to man as well as to animals. It was admitted that two persons equally exposed might not experience the noxious effects in the same degree: but it was impossible to allow under the well-known properties of this gas, that of two equally exposed, one should perish from it in about three hours, while the other should not experience the least ill consequences. It was also highly improbable that a man, in the course of four or five days, should have consumed upwards of five bushels of charcoal (the quantity stated by the prisoner) without suffering any serious symptoms; and this in an apartment in which, according to his own declaration, a quarter of a bushel had sufficed to destroy his wife in a few hours. The improbability of the story was strengthened by the consideration that this large quantity of charcoal was not consumed in small portions: but that two fires were kept burning at once, so that the slight ventilation which might have taken place through the crevices in the room, could not have prevented a speedy and fatal contamination of the air. Either the man himself must therefore have suffered from the symptoms of asphyxia or his statement was false.

3. *Whether the advanced state of putrefaction within less than five days after death, confirmed or invalidated the supposition of death by asphyxia?*

This question seems to have been answered somewhat evasively. The witness admits what is generally believed by medical jurists, that, in death from asphyxia, putrefaction is commonly much retarded. This is ascribed by him to the antiseptic properties of carbonic acid when absorbed into the blood. On the whole, he thinks that the advanced state of putrefaction in the deceased, dying as she did in the month of February, is repugnant to the supposition of her having been asphyxiated by carbonic acid.

4. *Whether if death took place from asphyxia, the skin of the deceased should not have appeared livid or red in spite of the putrefactive process?*

In answer to this question, it is alleged by the witness (M. Devergie) that the red colour of the skin in asphyxiated subjects remains for a long time visible, notwithstanding the access of putrefaction. He therefore considers it surprising that it was not found on the skin of the deceased, if she had really been asphyxiated. In regard to the question whether this appearance be a constant accompaniment of asphyxia, he states that in eight years' experience he has never failed to meet with it in asphyxiated subjects.

5. *Do females resist the effects of such an asphyxiated medium longer than males?*

To illustrate this enquiry a few statistical facts are mentioned. During the years 1834-5, there were in Paris 360 cases of death from asphyxia by carbonic acid. Of this number there were nineteen cases of two persons together, male and female. There were only three instances out of these in which one of the two could be resuscitated; and in each instance it was the *female*. The witness was inclined to think, not only from this but from other observations, that females are better capable of resisting the asphyxiating effects of carbonic acid than males.

6. *Whether asphyxia, under such circumstances, takes place most readily in individuals who are placed in contact with the floor or soil?*

If, observes the witness, a stratum of carbonic acid had collected on the floor of the apartment, this could only have happened after the entire combustion of the whole of the charcoal, and when a perfectly uniform temperature had become established throughout the whole mass of air in the room. Asphyxia must have taken place long before these conditions could have existed. The prisoner and the deceased were not therefore, by their position on the floor, in a situation more favorable for the access of asphyxia.

7. *What quantity of charcoal would be required to be burnt in order to asphyxiate two adult individuals, in such an apartment as that of the prisoner?*

The reporter remarks that suicides, (judging from the published reports,) rarely consumed more than a bushel of charcoal to accomplish their purpose; and he argues, it was highly probable that a quantity less than this, would have sufficed to produce a fatal vitiation of air in the prisoner's apartment. The fatal effects of the vapour, however, depend almost as much on the rapidity with which the charcoal is burnt, as on the absolute quantity of fuel consumed. A question of this nature could, of course, only be answered approximatively. Thus much was certain: the prisoner, according to his own confession, had burnt a much larger quantity than was necessary to render the air of a confined room insupportable to life, and the combustion had been rapidly conducted. The last question proposed was:

8. *What quantity of ashes ought a given weight of charcoal to yield?*

This was to test the accuracy of the prisoner's statement, as to the quantity of charcoal which he alleged he had consumed; in other words, to discover whether he had thus really made an attempt on his own life after the death of the deceased. It was not possible to furnish any positive answer to this question, the quantity of ashes from burnt charcoal varying according to the nature of the wood, from which the charcoal has been made. A bushel of charcoal ought to yield a little more than one-sixteenth of its bulk of ashes.

The prisoner upon the evidence adduced, was found guilty of having caused the death of his wife, and was condemned to the gallies for life.

[The circumstantial evidence seems to leave but little doubt in this case, that the deceased had really perished from the effects of carbonic acid. The rapid supervention of putrefaction might be accounted for by the heat of the room: and the absence of livor on the body, by the discoloration which the skin had undergone from that process. It is scarcely possible to imagine that the prisoner remained in the room and made the repeated attempts to destroy himself by suffocation, in the manner urged in his defence. It is evident from a consideration of the whole of the circumstances, that his intention was to destroy his wife and to save himself. If he had used any violence to confine the deceased in the room, or if he had rendered her insensible prior to placing her there, it was not to be discovered by any external marks: or if such marks had existed, they had become subsequently effaced by the rapid putrefactive changes.] *Annales d'Hygiène. Janvier*, 1837.

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## *On the Absorption of Sulphuric Acid, when introduced into the Stomach.*

### By A. Bouchardat and the late M. Cæsar Couriard.

It has been hitherto supposed that the mineral acids destroy life either by the local impression which they produce on the coats of the stomach, or by the inflammation and disorganization which they cause in that viscus. Since it is known that sulphuric acid speedily coagulates albumen and all albuminous liquids, it becomes difficult to understand how, when taken internally, this poison should become absorbed so as to react upon the blood; yet it is possible that the acid may enter and be prevented from combining with albumen, just as antagonist bodies may be intermixed and prevented from entering into combination, while exposed to the influence of an electric current. Or, allowing that an insoluble compound of acid and albumen is formed on its first absorption into the smaller vessels, it is possible to conceive that this compound may itself become absorbed, and react upon the blood in the larger trunks. But without resting the opinion upon mere suppositions, let us expose the facts which have induced us to admit that sulphuric acid may become absorbed into the circulating system.

*1st Observation.* A young woman, aged eighteen, swallowed, as it was conjectured, about an ounce of the sulphate of indigo. Immediately afterwards she felt an acute pain in the throat and in the stomach. She threw herself on the ground, and her cries soon brought her neighbours, who found her vomiting a bluish-coloured liquid, which effervesced on the pavement. A quantity of oil and milk was immediately exhibited: the milk was speedily thrown up coagulated and of a blue colour. When brought to the hospital three hours afterwards, she was in the following condition: Her face was pale, her features somewhat altered, her eyes were sunk, and her lips of a violet tinge. There was a yellowish coloured spot on the upper lip at each angle of the mouth. The tongue was blue, and the throat was painful, with a strong sense of constriction. The epigastrium was tender; but there was no pain in the abdomen. The respiration was difficult, there were great anxiety, coldness of the upper extremities, and a quick and small pulse. Her intellect was clear, and her answers to the questions put were sensible and proper. Four drachms of calcined magnesia in a pint of water were administered. A great portion of this mixture was rejected by vomiting, accompanied by bluish clots. In a few hours the pain in the throat became very severe, the upper extremities cold, and the pulse imperceptible. The urine which she passed had a *slight tinge of blue.* She continued to become worse, vomiting of chocolate-coloured matter supervened; and she died about *eleven* hours after having taken the poison.

The body was examined twenty-seven hours after death. The head presented no particular appearance. There was no sign of corrosion in the mouth. The mucous membrane of the pharynx and œsophagus was easily detached in dry white brittle layers. The heart was filled with three ounces of *coagulated blood;* and the aorta contained brown and semi-liquid *clots.* The lining membrane of this vessel was of a bright red colour. The stomach was distended, containing two ounces of a dark-coloured liquid. The mucous membrane was carbonized and of the colour of soot, intermixed with slight patches of redness throughout its whole extent, except for about an inch near the pylorus, where it was of a rose-red colour.

It was easily detached in layers, but there was no trace of ulceration. The lining membrane of the duodenum was inflamed and ulcerated, and in parts corroded and blackened. A dark-coloured mucus was found in the small intestines, and patches of a blue colour were scattered through the colon. The femoral arteries were filled with *semicoagulated dark-coloured blood.* The cavity of the left femoral artery was completely obstructed by the clot.

The whole progress of this case, taking the symptoms observed during life with the appearances found after death, justifies us in concluding that the deceased was not destroyed by the directly corrosive action of the poison on the digestive organs, but indirectly by the coagulation of blood in the circulating system. We endeavoured to corroborate the evidence furnished by this case by some experiments performed on dogs; but we found it difficult to say on these occasions, whether death was owing to the direct or indirect action of the poison. When very diluted sulphuric acid is given to dogs, there is no more poisonous action than when any ordinary acid liquid containing sulphuric acid is given to a human being. In these instances the action of the acid is superficial, it does not appear to become absorbed.

*2d Observation.* In October 1835, a young woman presented herself at the Hôtel Dieu, complaining of having suffered much from some medicine which she had taken the day preceding. On examination, a dark yellow spot was seen on her face near the commissure of the lips, her tongue and the mucous membrane of the mouth were highly inflamed, and the act of deglutition was painful. She was strictly questioned as to whether she had attempted to poison herself, but this she positively denied. It was evident that she was in a state of pregnancy, but this she also denied. The prescription of the medicine which she had taken directed a mixture of four drachms of sulphate of magnesia in three ounces of chamomile water; but when the bottle was examined, it was found to contain two drachms of a liquid consisting of equal parts of sulphuric acid and water. Some gum-water was administered to her which she continued to swallow with great difficulty; but it was almost instantly rejected. Her pulse was small and frequent: there was scarcely any burning sensation in her stomach, and indeed her only complaint was of pain in her throat. After a quiet night she was found much more composed, and could swallow more freely. Her pulse, however, was depressed, and her extremities were cold. During the second night she suffered from violent cramps in the lower extremities and of an entire loss of feeling in her right leg, which was found to be extremely cold at its lower part. On the following morning the symptoms abated; but there was no return of sensation in the right leg, her pulse became weaker and weaker; and on the third night after her admission she died without appearing to suffer.

On inspection, the mucous membrane of the œsophagus was found of a yellowish-black colour: it was covered with a similarly-coloured liquid, which was so adherent to it as almost to resemble a false membrane. The lining membrane was thickened, but was easily detached in layers. About five ounces of a dark yellowish liquid were found in the stomach. The mucous membrane of the viscus was in the same condition as that of the œsophagus; and its surface was covered with a dark yellowish mucus, which was remarkably firm and strongly adherent at the pyloric extremity. On removing a portion of this, the membrane beneath was found as it were carbonized. The duodenum and the small intestines presented a similar appearance. The heart contained about *three ounces of coagulated blood,* the aorta was almost filled with *gelatinous clots;* and the femoral artery of the right leg was completely obliterated by a cylinder of *dark coloured* and *firm coagula.* [The liquid found in the stomach and the *coagula* of blood contained in the femoral artery were submitted to an elaborate analysis, which we must here omit. The result will appear from the observations which follow.]

This evidence of the presence of sulphuric acid in the blood is not very satisfactory; for there were only *traces* of sulphate of barytes, and merely the odour of sulphurous acid was perceived: but if to these results we add the symptoms observed during life, the coldness of the extremities, the death of the lower extremity before general death, and the presence of a coagulum obstructing the femoral artery, we have reason to conclude that, in both of the observations reported, death was to be ascribed to the coagulation of the blood by the action of sulphuric acid.

The above facts show that sulphuric acid may destroy life in two ways, 1, by its direct action: 2, by absorption. In the first case the membranes of the alimentary canal are deeply carbonized and corroded, death is preceded by an acute gastro-enteritis and peritonitis, the stomach and intestines are distended with gas, and death ensues in the midst of the acutest suffering. If the sulphuric acid were combined with indigo the colouring matter does not appear in the urine: such are the characters of *local* poisoning. In the *second* case when sulphuric acid is *absorbed*, the local suffering is inconsiderable and the abdomen is not distended. There is no peritonitis, the pain in the epigastrium diminishes, and there is an appearance of recovery; but the pulse sinks, the extremities become cold, painful cramps supervene, and death takes place at a time when the local symptoms would lead us the least to expect that event. If this view of the operation of sulphuric acid be correct, the treatment of poisoning by this body ought to undergo some modification. Thus, instead of administering calcined magnesia, it would be proper to employ the bicarbonate of potash or soda, either of which is rapidly absorbed. By their entrance into the circulation, these alkaline salts may have the effect of destroying the coagula which might have been produced by the acid.

[Remarks. We need hardly say that this paper is worthy of attention from the novelty of the views which are displayed in it. Sulphuric acid, like the other mineral acids, has been considered by toxicologists to operate independently of absorption, for the following reasons: 1. Dilution with water promotes the absorption and consequently accelerates the operation of many poisons, but dilution destroys entirely the effect of sulphuric acid in depriving it of its corrosive properties. 2. If absorbed, the acid would immediately obstruct the smaller vessels by coagulating the blood contained in them. The vital properties of a part are not sufficient to prevent this chemical action, since it is an effect every now and then witnessed in the gastric vessels as a result of the action of the acid through the coats of the stomach. 3. The acid has never been detected in the blood of persons poisoned by it. Strong as these reasons appear for the non-absorption of sulphuric acid, they cannot of course prevail against any new facts which may lead to a contrary presumption. Still the evidence against the present doctrine ought to be unequivocal; since, as the authors of the paper suggest, an alteration in our views respecting the operation of such poisons may materially modify the mode of treatment adopted. We must, however, maintain that the cases here detailed do not in our judgment present any such facts.

Although we do not pretend to deny the *possibility* of a mineral acid, like the sulphuric, entering into the blood by absorption, (indeed this may be necessary to its ordinary medicinal operation,) yet we contend the point still remains unsettled, and it will so remain, until analyses of a less equivocal nature establish its presence in the blood of persons who have taken it. Sulphuric acid forms a very intimate combination with albumen; and it would be by no means difficult to detect it in a coagulum where it really had existed in sufficient quantity to produce coagulation. In the mean time the hypothesis of the authors of this paper involves this paradox: the acid is capable of traversing the minute capillaries without coagulating their contents or blocking up their canals, while it recovers its coagulating power on the blood when it reaches the heart, aorta, and larger arteries. It will render the femoral artery impermeable by a consolidation of its contents, after having had its coagulating power suspended in the numerous vessels through which it must have passed before it could have reached that artery.]

*Annales d'Hygiène. Avril*, 1837.

## MATERIA MEDICA.

### *New Preparation of Ipecacuanha.*

M. Gay describes a new mode of preparing this medicine, which may have its advantages in certain cases. The following is the formula:

| | |
|---|---|
| Ipecacuanha, in powder . . | 1 part. |
| Rectified Sulphuric Æther . | 6 parts. |

Macerate for some hours, and filter. Dry, by exposure to the air, the powder remaining upon the filter till it has entirely lost the odour of æther; then triturate gently, and preserve for use.—Ipecacuanha thus prepared is administered in the same doses as ordinary ipecacuanha, having all the properties of the latter: it has only lost its nauseous odour and disagreeable taste.

*Bulletin Général de Thérapeutique. August,* 1837.

## ANIMAL CHEMISTRY.

### *Microscopical Researches on the Composition of the Vaccine Fluid.*

By M. Dubois, of Amiens.

[This is a memoir read before the Royal Academy of Medicine: we can only find room for the concluding part of it, which gives the summary of the results obtained.]

1st. The vaccine virus, whether liquid or dry, shows no signs of globules.

2d. The same virus, examined by means of the strongest magnifiers, shows no traces of animalcules.

3d. In its recent state, (that is to say, during the first hours that follow its removal from the pustule,) this virus is remarkably fluid and limpid: by degrees it takes a more solid form, and shows a kind of crystallization.

4th. In its state of desiccation, we observe two orders of physical arrangement: viz. 1, lengthened lines, both opaque and transparent, and very slightly interlacing; and, 2, a very minute network.

5th. These arrangements are essential to good vaccine lymph, and show themselves in all cases exactly in the same way.

6th. When these appearances of the virus are not to be found, the vaccine has lost its contagious properties.

7th. These material conditions may fail either from an anomalous development of the pustules, and consequently from a constitution previously vitiated, or from accidental causes.

8th. A high and low temperature (ebullition and congelation) hinder the establishment of these physical arrangements.

9th. When the vaccine virus, through the operation of these causes, has not been able to form itself in this way, it loses its contagious properties.

10th. It is not by killing the animalcules that a high and low temperature destroy the properties of vaccine, but by altering its material conditions.

11th. Microscopic examination of the fluid can assist in determining the existence or non-existence of its preservative properties.

*Bull. de l'Acad. Roy. de Med.* 30 *Avril,* 1838.

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### *On the Comparative Analysis of Healthy Bile and that containing Calculi.*

By Paulo Muratori.

The first experiments of the author were made upon biliary calculi, of which he found the white portions composed of pure cholesterine, the yellow of dried and altered bile.

In the experiments upon the bile, with or without calculi, he took care to use such re-agents as should not engender fresh products; as appears to have been the case in the analysis of Tiedemann and Gmelin.

A thousand parts of human bile are composed of

| | Healthy bile. | | With calculi. |
|---|---|---|---|
| Water | 832 | .. | 786 |
| Peculiar fatty matter | 5 | .. | 8 |
| Colouring matter | 11 | .. | 9 |
| Cholesterine with soda | 4; | without soda, | 35 |
| Picromel | 94.86 | .. | 96.5 |
| Fleshy extractive | 2.69 | .. | 5.25 |
| Mucus | 37 | .. | 46 |
| Soda | 5.14 | .. | 0 |
| Phosphate of soda | 3.45 | .. | 5.50 |
| Phosphate of zinc | 3 | .. | 5 |
| Chloride of sodium | 1.86 | .. | 3.75 |
| | 1000 | | 1000 |

From this analysis, and from other experiments, the author concludes that the cholesterine is not mechanically mixed with the bile, but that, combined with the soda, it forms a saponaceous compound, soluble in the bile. If, however, it be in excess, or the alkali be deficient, it is precipitated from the bile in a crystalline form. *Bullettino delle Sc. Med. de Bologna. Settemb.* 1836.

### *Analysis of Liquor Amnii at two different Periods of Pregnancy.*

By Dr. C. Vogt, of Bern.

| | Liquor amnii, at 3½ months; | at 6 months. |
|---|---|---|
| Water | 979.45 | 990.29 |
| Alcoholic extract, consisting of an uncertain animal matter and lactate of soda | 3,69 | 0.34 |
| Chloride of sodium | 5.95 | 2.40 |
| Albumen (as residuum) | 10.77 | 6.67 |
| By boiling (9.45). | | |
| Sulphate and phosphate of lime, and loss | 0.14 | 0.30 |
| | 1000.00 | 1000.00 |
| Specific gravity | 1.0182 | 1.0092 |

The first fluid was, in all its circumstances, more concentrated than the other. Whether this is a condition connected with the development of the fœtus, must be a subject of future investigation. The female, pregnant three months and a half, died of an inflammatory disease; the other in a state of cachexia. The fluids were drawn from the membranes by a canula, and thus obtained pure. This may explain the difference between the present analysis and that of Frommherz, who obtained the fluid as discharged in natural labour. *Müller's Archiv.*

### *New Analysis of the Blood.* By M. Beudant.

The following is an abridgment of the conclusions drawn by M. B. from an examination of healthy blood:

1. That the albumen and gelatin are but the same substance, and that the albumen is only liquid in consequence of its combination with a mixture of thirteen parts of neutral salts, soluble in water, and one part of soda, contained in the blood.
2. The central corpuscles of the coloured globules of the blood are formed of solid albumen or fibrin.
3. In its healthy state the blood always contains some of the yellow matter of bile, which is also constantly met with, both in it and in the tissues of jaundiced persons.
4. The composition of the serum is always identical in healthy individuals.

The same is the case with the globules; and the different kinds of blood differ only in the relative proportions of these constituent parts.

5. The substances of which the serum and globules consist are in a very simple numerical proportion: thus, the serum being 1000, the salts are 10; the neutral fatty matters, together with the yellow and blue colouring matters, 20; the albumen, 80; and the whole of these solid substances, relatively to the water, which is 900, constitutes a total of 100.

*Archives générales de Médecine. Janvier*, 1838.

### *On the Presence of Urea in Dropsical Effusions.* By R. MARCHAND.

IN Poggendorff's Annalen, (vol. xxxviii. p. 356,) I communicated the analysis of a dropsical fluid, which was remarkable by containing urea, in the proportion of 0.42 per cent. Since that period I have twice had occasion to analyze dropsical fluids, and each time I have detected urea; in the first instance in the proportion of 0.68 per cent., and in the second in the proportion of 0.50 per cent. I have no doubt that the urea existed in these fluids in considerably larger quantity, but its separation becomes exceedingly difficult, owing to the accompanying albumen. I may remark, that in all the three cases there was retention of urine; and in two, which proved fatal, examination after death showed that peculiar degeneration of the kidney which has been described by Bright. It gives me much pleasure to see these analyses confirmed in a paper by Nysten, which, although read to the Academy of Sciences as early as 1810, did not appear in the Journal de Chimie Médicale till June, 1837. In this paper several cases are quoted, and amongst others one in which urea was detected in the perspiration, and another in which both urea and uric acid were found in the fluid ejected from the stomach. In how far do these facts favour the view that urea is prepared in the blood, and is not a product of the kidneys?

*Müller's Archiv. Jahrgang*, 1837. Heft iv.

## Selections from American and Colonial Journals.

### PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

### *Extraordinary Case of Electrical Excitement.* By Dr. WILLARD HOSFORD, of Orford, New Hampshire, United States.

A LADY, of great respectability, during the evening of the 25th of January, 1837, the time when the aurora occurred,[*] became suddenly and unconsciously charged with electricity; and she gave the first exhibition of this power in passing her hand over the face of her brother, when, to the astonishment of both, vivid electrical sparks passed to it from the end of each finger. The fact was immediately mentioned; but the company were so sceptical that each, in succession, required, for conviction, both to see and feel the spark. On entering the room soon afterward, the combined testimony of the company was insufficient to convince me of the fact until a spark, three-fourths of an inch long, passed from the lady's knuckle to my nose, causing an involuntary recoil. This power continued with augmented force from the 25th of January to the last of February, when it began to decline, and became extinct by the middle of May. The quantity of electricity manifested during some days was much more than on others, and different hours were often marked by a light variableness; but it is believed that, under favorable circumstances, from the 25th of January to the 1st of the following April, there was no time when the lady was incapable of yielding electrical sparks.

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[*] Speaking of it, Dr. Hosford adds, that "the heavens were lighted with a crimson aurora of such uncommon splendour as to excite no ordinary emotions in every observer."

The most prominent circumstances which appeared to add to her electrical power were, an atmosphere of about 80° Fahr., moderate exercise, tranquillity of mind, and social enjoyment: these, severally or combined, added to her productive power, while the reverse diminished it precisely in the same ratio. Of these a high temperature evidently had the greatest effect, while the excitement diminished as the mercury sunk, and disappeared before it reached zero. The lady thinks fear alone would produce the same effect, by its check on the vital action. We had no evidence that the barometrical condition of the atmosphere exerted any influence, and the result was precisely the same whether it were humid or arid. It is not strange that the lady suffered a severe mental perturbation from the visitation of a power so unexpected and undesired, in addition to the vexation arising from her involuntarily giving sparks to every conducting body that came within the sphere of her electrical influence; for, whatever of the iron stove or its appurtenances, or the metallic utensils of her work-box, such as needles, scissors, knife, pencil, &c. &c., she had occasion to lay her hands upon, first received a spark, producing a consequent twinge at the point of contact. The imperfection of her insulator is to be regretted, as it was only the common Turkey carpet of her parlour, and it could sustain an electrical intensity only equal to giving sparks one and a half inch long: these were, however, amply sufficient to satisfy the most sceptical observer of the existence, in or about her system, of an active power that furnished an uninterrupted flow of the electrical fluid; of the amount of which, perhaps, the reader may obtain a very definite idea by reflecting upon the following experiments:—When her finger was brought within one-sixteenth of an inch of a metallic body, a spark, that was heard, seen, and felt, passed every second. When she was seated with her feet on the stove-hearth (of iron), engaged with her books, with no motion but that of breathing and the turning of leaves, then three or more sparks per minute would pass to the stove, notwithstanding the insulation of her shoes and silk hosiery. Indeed, her easy chair was no protection from these inconveniences; for this subtle agent would often find its way through the stuffing and covering of its arms to its steel framework. In a few moments she could charge other persons insulated like herself; thus enabling the first individual to pass it on to a second, and the second to a third.

When most favorably circumstanced, four sparks per minute, of one inch and a half, would pass from the end of her finger to a brass ball on the stove: these were quite brilliant, distinctly seen and heard in any part of a large room, and sharply felt when they passed to another person. In order further to test the strength of this measure, it was passed to the balls by four persons forming a line: this, however, evidently diminished its intensity, yet the spark was bright. . .

The lady had no internal evidence of this faculty,—a faculty *sui generis:* it was manifest to her only in the phenomena of its leaving her by sparks, and its dissipation was imperceptible, while walking her room or seated in a common chair, even after the intensity had previously arrived at the point of affording one and a half inch sparks. Neither the lady's hair or silk, so far as was noticed, was ever in a state of divergence; but, without doubt, this was owing to her dress being thick and heavy, and to her hair having been laid smooth at her toilet, and firmly fixed, before she appeared upon her insulator. . . .

This lady is the wife of a very respectable gentleman of this place: she is aged about thirty, of a delicate constitution, nervous temperament, sedentary habits, usually engaged with her books or needle-work, and generally enjoying a fine flow of spirits. She has, however, never been in sound health, but has seldom been confined to her bed by sickness even for a day. During the past two years she has suffered several attacks of acute rheumatism, of only a few days' continuance; but, during the autumn and the part of the winter preceding her electrical development, she suffered much from unseated neuralgia in the various parts of her system, and was particularly affected in the cutis vera, in isolated patches; the sensation produced being precisely like that caused by the application of water heated to the point a little short of producing vesication.

*Silliman's American Journal of Science. Jan.* 1838.

## *On Purulent Discharges from the Bladder and Rectum in Hepatic Diseases in India.* By J. Mouat, M.D. Surgeon H. M. 13th Dragoons.

This is an important paper, containing a detail of thirteen cases of disease of the liver, chiefly hepatitis, in all of which there was discharge of pus from some of the natural passages, generally with relief, and in most without any direct communication existing between the original seat of the fluid and its vicarious outlet. The following table gives a general view of the results:

*Table of Purulent Discharges.*

| Cases. | Names. | Pus passed by | | | | Remarks. |
|---|---|---|---|---|---|---|
| | | Urine. | Stools. | Expectoration. | Vomiting. | |
| 1 | J. Ward ......... | 1 | 1 | 0 | 0 | Recovered. |
| 2 | T. Rippen ....... | 0 | 1 | 1 | 0 | Ditto, but died the 2d attack. |
| 3 | J. Gibson ....... | 1 | 1 | 1 | 0 | Died. |
| 4 | J. Young......... | 0 | 1 | 0 | 0 | Recovered. |
| 5 | R. Mallalew..... | 1 | 1 | 0 | 0 | Died. |
| 6 | G. Kennedy .... | 1 | 1 | 0 | 0 | Ditto. |
| 7 | G. Munnings ... | 1 | 0 | 0 | 0 | Recovered. |
| 8 | M. Reddy....... | 1 | 0 | 0 | 0 | Ditto. |
| 9 | M. Smith........ | 1 | 1 | 0 | 0 | Died. |
| 10 | J. Thomas ....... | 1 | 0 | 0 | 0 | Ditto. |
| 11 | H. Jackson ...... | 1 | 1 | 0 | 0 | Recovered. |
| 12 | M. Leamy ....... | 1 | 1 | 1 | 1 | Died. |
| 13 | C. Pearson ...... | 1 | 1 | 1 | 0 | Under treatment. |
| | Total......... | 11 | 10 | 4 | 1 | |

We extract two of the cases, one of recovery, the other fatal.

Case viii. Private Michael Reddy, ætat. forty. In India fifteen years. Admitted 12th July, 1834, with acute hepatitis, attended with nausea, vomiting, general weakness, loss of appetite, fever, &c. On the following day the side appeared enlarged and very tender on pressure. Like the other cases, was actively treated, yet the pain and swelling continued unabated till the 17th, when his mouth became affected from mercury, and at the same time a deposition of pus took place in the urine, with great relief to the pain, &c. and the swelling appeared somewhat reduced. From this period he continued to pass matter in his urine to the extent of two to three ounces in the twenty-four hours, till the beginning of September, when a gradual decrease of both pain and swelling, and an improvement in his health and strength took place. During September, pus was observed in less quantity, and about 20th November it ceased entirely; and on the 26th of that month he was discharged to his duty quite well, and is now enjoying good health.

Case ix. Private Mark Smith, ætat. twenty-three. In India one year. Admitted 18th June, 1834, with acute hepatitis, complicated with dysentery, slight fever, nausea, general debility, &c.; copiously bled, and actively treated, yet on the fifth day of admission his side became much swollen and very painful, and the following day pus was observed in his urine, with evident relief; the swelling was now considerably reduced, and there was slight ptyalism from the mercury he had taken. On the 24th the pain returned with great violence, (though he continued passing matter largely both by urine and stool,) followed by difficulty of breathing, weakness, copious cold perspiration, dyspnœa and lowness of spirits, &c., and he died on the 26th, or eight days after admission, in great agony.

*Dissection* disclosed several abscesses of various sizes in both lobes of the liver, some running into each other, and others quite distinct and filled with pus, but no communication could be traced either with the kidneys or intestines, though the urinary bladder appeared filled with a mixture of urine and purulent matter.

[We entirely agree with Dr. Mouat in his remarks on these cases. He says,] The deposits by stool, urine, and expectoration were examined in all the patients in the usual manner by the most approved tests, particularly the sulphuric acid, &c.; and though these be of a negative nature, still, when taken together with the appearance of the matter voided, left no doubt of its nature. Beside which, the previous symptoms in some, the swelling of the side in others, and the dissection in all the fatal terminations, are confirmative of the circumstance. One or two cases, selected to illustrate particular views, might create doubt, but there can be none where there were so many instances, many of them protracted, all closely watched by the other medical officers of the corps, and many of them viewed with great interest by professional friends; and where the dissections in the fatal cases were made by the assistants attached to the regiment, with the specific purpose of tracing, if possible, any direct channel of communication between those abscesses and the intestines, lungs, and kidneys.

*Calcutta Quarterly Medical Journal. July,* 1837.

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## *Case of Extra-Uterine Pregnancy.* By ALLAN WEBB, Esq.

WHEN Mr. Webb was summoned to see this patient, a Bengali woman, sixteen years of age and married, she had been ill three days, vomiting frequently, and was supposed to be dying of cholera. She was restless, tossing about in bed; no pulse, or an extremely feeble one. Respiration 60°: no pain in the abdomen upon pressure: no cholera expression of face. The abdomen was, however, enlarged, but soft, and yielding, no resistance being offered by the muscles to pressure. To enquiries if she were pregnant, she, and those around her, replied in the negative. I could not feel either uterus or bladder enlarged.

Venesection was performed. The blood, instead of being black, was scarcely deeper coloured than common serum; flowed slowly at first, more freely afterwards, and improved in colour. In raising herself to vomit she nearly fainted, and no more blood flowed from the arm. Gave a little ether, and ordered a warm bath to be prepared. She seemed somewhat easier after the ether: but in the act of raising her, to enter the bath, she breathed her last.

The body was examined three hours after death.

On puncturing the abdomen immediately below the sternum, blood gushed out. Before proceeding further, I suffered an immense quantity to escape, as much indeed as filled two wash-basins On fully laying open the cavity of the abdomen, all the organs appeared to be in health, but having the peculiar bleached appearance observed in slaughtered animals. The omentum which was adherent in the hypogastric regions being raised, and the bowels turned aside; the pelvis was seen to be filled up with a large and tolerably firm coagulum of black blood. Removing this, the bladder was observed contracted to the size of a small apple. The uterus of the natural size, but an enlargement was observed in the right fallopian tube, about the size of a large walnut, with which the large coagulum before spoken of was intimately connected. This enlargement was exceedingly thinned, anteriorly, with one or two black spots, ready to give way; whilst, superiorly, it had burst and given exit to a small fœtus, in which the rudiments of a spine, head and limbs, could be distinctly made out through the membranes. The placenta had not wholly escaped, part of it being still adherent to the internal surface of the fallopian tube. The free villous surface seeming to consist of the torn mouths of an immense congeries of small vessels, whence the hemorrhage had come. No larger vessel could be found to account for it, yet both fœtus, placenta, and membranes were not bigger than a good-sized walnut. This rent fallopian tube is intimately connected with the firmest portion of the coagulum. Right ovary, size of a fig, containing cysts: left ovary and fallopian tube healthy: uterus closed with tenacious secretion, lined with membrana decidua: vagina bleached and corrugated.

*Calcutta Quarterly Medical Journal. July,* 1837.

## *On the Cure of Stammering by Moral Means.* By EDWARD WARREN, M.D., of Boston (U. S.)

[WE recommend the following very sensible observations to the attention of our readers. They are extracted from a well-written memoir on the general subject of stammering, which will well repay perusal. We regret that our limits do not admit of further extracts.]

I may be asked, if I maintain the importance of an experienced teacher: can parents who have children thus afflicted, do nothing themselves? I answer that they may do almost everything. With children, almost everything can be done by moral treatment; and according to the moral management they meet with, will the disease be confirmed or eradicated. The subjects are generally children of extreme nervous susceptibility and of feeble constitutions. The ordinary means for producing vigour and robustness, and for strengthening the nervous system must be resorted to. The muscular system must be developed as far as possible. If the chest is narrow and contracted, every means must be employed of bringing into action the muscles of the arms and chest. For this purpose gymnastic exercises, the use of dumb-bells, and various sports may be recommended. In this way, a great deal may be done to produce fulness of the chest. The child may be encouraged in the practice of these exercises as a means of acquiring physical strength, without his attention being called to his defect of speech. As soon as hfi is capable of reasoning, let him be driven as much as possible into the society of other children. If his defect is laughed at, let him be habituated to bear ridicule without flinching; let him be taught that those whose feelings allow them to ridicule defects or deformities, are much more worthy of pity than the subjects of those deformities. Let him be carried to the abodes of the deaf and dumb; teach him how much happier he is than they, how great a blessing he enjoys in the use of speech, even if his speech is imperfect. Carry him also into the presence of the blind, the lame, and the deformed. Let him be familiar with the sight of those who are greater sufferers than he. If you pursue an opposite course—if, because he is sensitive to the ridicule of other children, you let him remain at home, if you impress upon his mind the idea that he has a defect which must be removed, and which will be an insuperable bar to his progress in life, unless it is removed; if you allow him to perceive your constant anxiety for his care, he will get the idea that there is something peculiar in his case—that he is marked out from mankind, as if the seal of Cain were set upon his brow; and that until he is freed from his curse, he can never associate with his fellows without shame.

On the contrary, you should direct your principal attention to convince him that his fate is not an uncommon one; that defects and diseases are the common lot, and assigned for wise purposes. In as far as you direct his attention to his defect at all, let it be with this object—to convince him that it is not an evil. Teach him resignation to the will of God. Impress upon his mind above all things, that he is under the constant protection of a being who knew what was best for him, and who has placed him in the condition and under the circumstances best adapted for his welfare. Priestly attributes the greatest blessings he enjoyed to his impediment of speech; and others may in like manner trace to the same cause, their preservation from much evil, and their possession of much happiness. Our greatest felicity is often produced by what we regard at the time as our greatest misfortunes.

When he begins to feel the importance of a free use of speech (and he may feel the importance of it without being morbidly sensitive on the subject,) and disposed to enter upon a laborious course of discipline, seek out a person who has experience in the treatment of impediments of speech. Place him under his care, and if he is benefited, do not remove him and think to perfect the cure yourself. Three months is a very short time for him to remain under the superintendence of an instructor; six months is better, and where it is practicable he should remain a year. If this interferes with his other studies, it is of no consequence. He will derive benefit enough to compensate for the loss. The age I would fix upon for this trial, should be from eight to twelve. Some children, however, are as mature at the former age as others at the latter. At this period, the loss of a year's study may perchance be a gain. To a child of nervous habits, the time allowed from his instruction in speaking, may be much better employed in acquiring health and vigour, by play and exercise, than in study. But if the child is not disposed to enter into this course, if he is irritable and indocile, and regards it merely as an irksome task, it will be better to wait till a more advanced age shall convince him of its importance. Otherwise we run the risk of increasing his irritability and sensitiveness.

Moral management is, therefore, all important. In most cases it will alone be sufficient to effect a cure; and in cases where it does not, it will render the cure easy to a competent instructor. I would again urge the impropriety of subjecting the patient to a new trial if the first fails. I would urge most strenuously the necessity of leading him to the choice of that pursuit in which his defect shall afford the smallest obstacle to his progress. He is to be taught to look upon it as a necessary evil, and to shape his course accordingly. He is not to be led to bear it in his mind as the prime obstacle to his success, which must be removed before he can be happy. The molehill is thus magnified into a mountain. Whatever side he looks upon, his impediment rises up before him, shutting him out from the road to distinction. It comes to occupy so large a share of his attention that he becomes a monomaniac: on this subject, he is actually insane: there is this little diseased spot in his mind: fortunate will it be for him if it does not affect the whole; if the gangrene do not extend over all his feelings.

To the person whose age renders him the director of his own course, I would give the same directions. The same rules that must guide parents in the management of children, should guide him in the work of self-education. The first work the stammerer has to accomplish is the regulation of his mind; the acquisition of perfect self-command and of mental calmness. When this is done, the rest is easy. Until it is done, it is in vain for him to attempt by physical means to overcome his defect of utterance. The first embarrassment he meets with may cause its return. When he has brought himself to feel his impediment less keenly; to be less morbidly susceptible on the subject; then, if he is not already cured, let him apply to a person experienced in the treatment of stammering. If he meets, there, others who are afflicted as he is, it is all the better; he will no longer look upon his case as a peculiar one; and if he sees others whose impediments are worse than his, it will give him additional courage.

But great labour and perseverance are necessary in the employment of the physical means, in overcoming the perverse habits of the organs, and training them to articulate correctly. I would advise him, if it be possible, to pursue the method in the place of his usual residence, and while he continues his ordinary employments. An individual may leave his customary abode and pursuits, and go to a neighbouring town or city for his cure. His ordinary trains of association will be broken off, and the new mode of speech will be more readily adopted while he remains absent. But the moment he returns; the moment he resumes his former avocations, and is subjected to his customary objects of anxiety, his former mode of speech returns.

Whatever method may be employed for the relief of this affection, no permanent advantage will be gained, in the majority of cases, unless resolutely persevered in for one or two years. With this perseverance, it may be cured with as much certainty as any other chronic disorder, and this not by any new or patent method, but simply by attention to the course I have described.

*American Journal of the Medical Sciences.* No. 41.

## SURGERY.

### *A new Treatment in a Case of Anchylosis.* By J. R. Barton, M.D., Philadelphia.

[It is most gratifying to perceive, from the productions of the medical press of America, how completely the physicians 'and surgeons of that country are on a level with their brethren in Europe. In the boldness of their surgical operations they almost excel us, as the following most interesting case, as well as others which we have formerly cited, will evince.]

In the North American Medical and Surgical Journal for April, 1827, Dr. B. published an account of a new and successful operation at the hip, which had been undertaken for the twofold purpose of remedying a most serious deformity and lameness, and of *establishing an artificial joint*, as a substitute for the natural articulation, which had become obliterated by disease, terminating in true anchylosis.

In this case the neck of the femur was sawn through, and the distorted limb straightened. The wound of the soft parts was then healed, whilst the reunion of the divided bone was prevented by subjecting it, from time to time, to motion,—such as gentle rotation, flexion and extension, abduction and adduction. After continuing this treatment for a few weeks, the ends of the bone lost their disposition to unite, became obtunded and smooth, and were held attached to each other by provisional bands or ligaments, and in this manner forming an artificial joint, whose movements were regulated by all the principal muscles by which the original joint had been controlled.

The patient upon whom this operation was performed enjoyed the use of his artificial joint for six years; during which period he pursued a business (trunk-making) with great industry, earning for himself a comfortable subsistence, and a small annual surplus. Pecuniary losses, however, through the reverses of those in whose hands he had confided his means, sunk him into a state of despondency and desperation, followed by habits of intemperance. This, with all its train of evils, abuse of health, &c., was, no doubt, the cause of the change which afterwards took place in the artificial joint: it gradually became more and more rigid, and finally all motion ceased in the part.

The object of the treatment in the present case was different, but the means used were no less ingenious, and the results even more successful. The patient was a medical gentleman, Dr. Seaman Deas, now in active practice in Alabama. The following extracts give an abridged history of the case:

S. D., when a youth of about nine years of age, unluckily had his knee-joint involved in inflammation and suppuration so extensively as to occasion the destruction of the synovial membranes, the ligaments, cartilages, and, in short, every structure peculiarly appertaining to the joint. After a protracted suffering, he finally recovered with the loss of the joint; the tibia, femur, and patella having become united to each other in the form of a true anchylosis. The loss of the articulation of the knee, however, though a misfortune, did not constitute the *sadness* of his case: it was caused by the malposition of the limb; the leg having been flexed upon the thigh to a degree somewhat less than a right angle. Hence the only alternatives of which he could avail himself to aid in walking were, either to use crutches, or to employ a very high block-sole boot, and to lower his stature by flexing the sound limb, in order that both feet might reach the ground. The latter expedient he adopted. The long-continued pressure and weight of the body sustained by this defective limb, acting under such great mechanical disadvantages, had at length caused some projection of the instep, and other irregularities which it is unnecessary to particularize. This supposed irremediable condition of his limb, with all its ills, the young gentleman endured during the period of about sixteen years. In the meantime he graduated in medicine, and became a successful and highly respectable practitioner; but, as his professional labours increased, he found the condition of his limb to be an obstacle not only to his further success, but also a source of unceasing annoyance and vexation.

Dr. Deas having consulted Dr. Barton, and assented to the plan of cure proposed by him, the operation was performed on the 27th May, 1835, as follows:

Two incisions were made over the femur, just above the patella. The first commenced at a point opposite the upper and anterior margin of the external condyle of the femur, and, passing obliquely across the front of the thigh, terminated on the inner side. The second incision commenced also on the outer side, about two and a half inches above the first; and, passing likewise obliquely across the thigh, terminated with the other in an acute angle. By these incisions were divided the integuments, the tendon of the extensor muscles of the leg, at its insertion into the upper part of the patella, and some of the contiguous fibres of the rectus and crureus muscles themselves, a greater part of the vastus internus, and a portion of the vastus externus muscles. A flap, composed therefore of this structure, was elevated from the femur close to the condyles. The soft parts were next detached from the outer side of the bone, from the base of the flap toward the ham, by passing a knife over the circumference of it, so as to admit of the use of a saw. The flap then being turned aside, a triangular or wedge-like piece of the femur was easily removed by means of a small narrow-bladed saw, such as is used in the operation at the hip. This wedge of bone did not include the entire diameter of the femur at the point of section, so that a few lines of the posterior portion of the shaft of the bone remained yet undivided. By slightly inclining the leg backward, these yielded, and the solution was complete. This mode of effecting the lesion of the bone was designedly adopted, and constituted what I conceived to be a very important measure in the operation: important, because it rendered the popliteal artery free from the danger of being wounded by the action of the saw, and subsequently the interlocking of the fractured surfaces tended to retain the extremities of the divided bone in their positions until the harshness of their surfaces had been overcome, either by the absorption of their angles or by the deposition of new matter upon them; a change essential to the safety of the artery during the subsequent treatment of the case. Not a blood-vessel was opened which required either a ligature or compression. The operation, which lasted about five minutes, being thus ended, the reflected flap was restored to its place, the wound lightly dressed, and the patient was put to bed, lying on his back, with the limb supported upon a splint, *of an angle corresponding to that of the knee previous to the operation.* This position was maintained until it was believed that the asperities of the bone had become blunted, and were not likely by their pressure to cause ulceration of the artery beneath them. This first splint was then removed, and another, having the angle slightly obtuse, was substituted. In a few days a third splint, with the angle more obtuse than that of the second, supplied its place. Others, varying in degrees of angularity, in like manner came in their turn to support the limb, until it had attained a position almost straight: it was then unchangeably continued in that line until the contact surfaces of the bone had united, and securely fixed the limb in this the desired direction. . . .

At the end of about four months from the date of the operation, the patient stood erect, with both feet in their natural position, and the heels resting alike upon the floor, although a slight angle had been designedly left at the knee, in order that there might not be any necessity for throwing the limb out from the body in the act of walking, which is always the case when the knee is quite straight. After this period, the use of shoes of the ordinary shape was resumed, and the limb was daily exercised with increasing strength and usefulness.

[The sequel of the case is given in a letter from Dr. Deas himself, addressed to his accomplished surgeon, of which the following is the most important part:]

"I have the satisfaction and pleasure of saying to you now, that the operation you performed on my leg has been *completely* successful, and has more than realized my most sanguine anticipations. The small abscess which you dressed the day before we parted at Norfolk continued open, and threw out, from time to time, small pieces of bone until the August after, when the last piece was discharged; the orifice then closed, and I have suffered no material inconvenience from it since. From the January previous, however, I was going about and attending to my professional business; and early in the summer, when our sickly season commenced, I was on horseback daily, riding from thirty to fifty miles a day, without more than the ordinary fatigue or inconvenience. I am at present well, the wound sound, and I feel no other inconvenience in riding or walking than what arises from my knee-joint being stiff, which was the case before you performed the operation. I walk without a stick or other aid, with the sole of the foot to the ground, and, my friends tell me, with but a slight limp; and I have great pleasure in adding, that the leg and foot have increased considerably in size, so as now to be nearly equal to the other. When I think of what I was, and what I am; and that to your firmness, judgment, and skill, I am indebted for the happy change, I want words to express adequately all that I feel."

[We may almost envy the feelings of Dr. Barton on reading this letter. To save life, or, what is often more, to render a miserable life happy, by one bold and well-considered act; to have ourselves, and to permit to others no doubts as to the full amount of the benefit conferred; to be the object of such gratitude as only those can feel who have received so inestimable a blessing: these are the great and almost exclusive triumphs of SURGERY;—these are among the bright things in medical science which compensate for many of its difficulties, uncertainties, and cares; and ought to hallow its pursuit to the young as something conversant with man's diviner nature, and purify its practice from the low considerations which so pertinaciously cling to it as an art.]

*American Journal of Med. Sciences. Feb.* 1838.

## *Treatment of Varicose Veins in the Pennsylvania Hospital.*

CASE I. By Dr. NORRIS. George K——, a German, aged 57 years, was admitted into the wards on the 19th of July, 1837, for varicose veins, from which he had suffered for several months. He had had a large ulcer caused by the veins, for which he had been treated in the city by bandages, &c. The ulcer was much reduced in size when he entered, and, after appropriate treatment, healed. On the 12th of August, Dr. Norris introduced two acupuncture needles,—one behind the vein, and the other through and through it in a line oblique to its axis, and surrounded both by a figure-of-eight ligatures. Little pain was caused by the operation. The limb was then elevated in a fracture-box; lead-water cloths applied; and the antiphlogistic treatment directed.

August 15th. The patient complains of no pain; little inflammation has occurred: ligature tightened, and treatment continued.—17th. Slight inflammation at the sutures: same treatment.—19th. The needles and ligatures were removed: some inflammation around the part, but none to any distance above or below.—24th. Inflammation increased; slight ulceration at the points where the needles entered: a poultice to the part, and antiphlogistic treatment continued.—Sept. 4th. The ulcers have healed; the vein perfectly obstructed: bandages and compress applied along the course of the vein.—7th. Allowed to walk about; has slight porriginous eruption: treated accordingly.—15th. Vein obliterated entirely; patient walks without feeling any inconvenience from it.—17th. Discharged, entirely well.

Within a few weeks the patient was seen, having no return of his complaint, and continuing constantly at work.

CASE II. By Dr. HARRIS. The subject of this case was 66 years of age, and had suffered from leg-ulcers for five years, which had been preceded, for two years, by a varicose condition of the veins.

Saturday, 3d February, the man was introduced into the amphitheatre, and stood upon the table. A fold of skin over the saphena vein, where it passes over the knee-joint, was held up by an assistant, and a bistoury passed through, dividing the skin and cellular substance down to the vein. A needle, armed with a ligature, was passed under the vein, which was drawn out, and a piece of it, half an inch in length, removed. A firm compress was placed above and below the orifice, the edges of the wound drawn together by adhesive plaster, and a bandage applied from the toes up to above the knee. The man was put to bed, and the limb placed in a long fracture-box. The bandage was not removed for a week. At the end of that time the wound was lightly touched with nitrate of silver; and, Wednesday, 21st, it had entirely healed. The veins are still visible through the skin, though considerably reduced in size. Bandage still applied.

*American Medical Examiner.* Nos. 5 and 9. 1838.

## *A Statistical Account of Fractures, treated in the Pennsylvania Hospital, from its foundation in* 1751 *to* 1838. By J. M. Wallace, M.D., Resident Surgeon.

| I. Abstract of 197 Cases, treated from 1751 to 1800. — Description of Fracture. | Number admitted. | Number cured. | No. of days required for cure. | | | Died from immediate effects of injury. | Died subsequently. | Terminated by Amputation. | Removed while under treatment. |
|---|---|---|---|---|---|---|---|---|---|
| | | | Max. | Min. | Average | | | | |
| Leg, Simple | 55 | 41 | 205 | 10 | 87 | 2 | 1 | 2 | 9 |
| Compound | 8 | 7 | 201 | 53 | 104 | 0 | 0 | 1 | 0 |
| Complicated | 9 | 9 | 591 | 77 | 213 | 0 | 0 | 0 | 0 |
| Thigh, Simple | 31 | 23 | 245 | 29 | 96 | 2 | 2 | 0 | 4 |
| Compound | 1 | 1 | 199 | 199 | 199 | 0 | 0 | 0 | 0 |
| Complicated | 10 | 10 | 314 | 42 | 118 | 0 | 0 | 0 | 0 |
| Arm, Simple | 34 | 24 | 162 | 21 | 69 | 3 | 0 | 0 | 7 |
| Complicated | 2 | 2 | 123 | 32 | 77 | 0 | 0 | 0 | 0 |
| Ribs | 20 | 13 | 195 | 11 | 50 | 2 | 0 | 0 | 5 |
| Skull | 9 | 4 | 205 | 35 | 137 | 3 | 1 | 0 | 1 |
| Clavicle | 8 | 7 | 42 | 16 | 29 | 0 | 0 | 0 | 1 |
| Jaw | 6 | 4 | 192 | 40 | 91 | 1 | 0 | 0 | 1 |
| Patella | 3 | 3 | 151 | 79 | 114 | 0 | 0 | 0 | 0 |
| Vertebræ | 1 | 1 | 507 | 507 | 507 | 0 | 0 | 0 | 0 |
| Pelvis | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| Sternum | 1 | 1 | 10 | 10 | 10 | 0 | 0 | 0 | 0 |
| Wrist | 1 | 1 | 255 | 255 | 255 | 0 | 0 | 0 | 0 |
| Fingers | 1 | 1 | 31 | 31 | 31 | 0 | 0 | 0 | 0 |
| Toes | 1 | 1 | 26 | 26 | 26 | 0 | 0 | 0 | 0 |
| Not designated | 4 | 1 | 72 | 72 | 72 | 2 | 0 | 0 | 1 |
| II. *Do.* 868 Cases, from 1800 to 1829. | | | | | | | | | |
| Leg, Simple | 249 | 216 | 217 | 10 | 70 | 9 | 12 | 0 | 12 |
| Compound | 10 | 3 | 220 | 83 | 136 | 6 | 1 | 0 | 0 |
| Complicated | 21 | 9 | 518 | 51 | 204 | 5 | 5 | 2 | 0 |
| Arm, Simple | 192 | 168 | 181 | 11 | 48 | 3 | 3 | 0 | 18 |
| Complicated | 8 | 5 | 66 | 25 | 48 | 3 | 0 | 0 | 0 |
| Thigh, Simple | 130 | 106 | 210 | 32 | 110 | 8 | 3 | 0 | 13 |
| Compound | 2 | 0 | 0 | 0 | 0 | 2 | 0 | 0 | 0 |
| Complicated | 18 | 8 | 418 | 77 | 158 | 7 | 2 | 0 | 1 |
| Clavicle, Simple | 61 | 58 | 84 | 9 | 36 | 0 | 0 | 0 | 3 |
| Complicated | 2 | 0 | 0 | 0 | 0 | 1 | 1 | 0 | 0 |
| Ribs, Simple | 45 | 38 | 70 | 6 | 34 | 2 | 0 | 0 | 5 |
| Complicated | 7 | 2 | 70 | 43 | 56 | 5 | 0 | 0 | 0 |
| Skull | 29 | 9 | 133 | 10 | 50 | 16 | 1 | 0 | 3 |
| Jaw | 22 | 15 | 136 | 11 | 34 | 1 | 1 | 0 | 5 |
| Patella | 12 | 12 | 144 | 33 | 73 | 0 | 0 | 0 | 0 |
| Vertebræ | 10 | 0 | 0 | 0 | 0 | 2 | 7 | 0 | 1 |
| Fingers, Simple | 10 | 6 | 63 | 20 | 43 | 1 | 0 | 0 | 3 |
| Compound | 1 | 1 | 74 | 74 | 74 | 0 | 0 | 0 | 0 |
| Elbow | 8 | 6 | 106 | 28 | 51 | 0 | 0 | 1 | 1 |
| Fibula | 6 | 5 | 175 | 51 | 99 | 0 | 1 | 0 | 0 |
| Nose | 4 | 2 | 22 | 9 | 15 | 0 | 0 | 0 | 2 |
| Shoulder | 4 | 3 | 58 | 26 | 39 | 0 | 0 | 0 | 1 |
| Scapula | 3 | 3 | 131 | 41 | 71 | 0 | 0 | 0 | 0 |
| Pelvis | 3 | 2 | 54 | 30 | 42 | 0 | 0 | 0 | 1 |
| Foot | 3 | 2 | 128 | 121 | 124 | 1 | 0 | 0 | 0 |
| Toes | 3 | 2 | 130 | 9 | 69 | 0 | 0 | 0 | 1 |
| Ankle | 3 | 2 | 66 | 42 | 54 | 1 | 0 | 0 | 0 |
| Hand | 2 | 2 | 48 | 23 | 35 | 0 | 0 | 0 | 0 |
| Not designated | 1 | 1 | 19 | 19 | 19 | 0 | 0 | 0 | 0 |

III. **ABSTRACT** of 735 Cases, treated from 1829 to 1838.

| Description of Fracture. | Number admitted. | Number cured. | Number of days required for cure in adults. | | | Number of days required for cure in children under 18 years. | | | Died from the effects of injury. | Died subsequently | Ter. by amputation | Removed while under treatment. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | Max. | Min. | Aver. | Max. | Min. | Aver. | | | | |
| Leg, Simple ............ | 172 | 150 | 516 | 14 | 74 | 88 | 44 | 64 | 11 | 5 | 1 | 5 |
| Compound ....... | 37 | 21 | 374 | 54 | 141 | 103 | 79 | 91 | 8 | 5 | 2 | 1 |
| Complicated..... | 8 | 6 | 103 | 48 | 76 | 206 | 206 | 206 | 1 | 1 | 0 | 0 |
| Arm, Simple............ | 161 | 142 | 191 | 9 | 32 | 133 | 7 | 39 | 1 | 1 | 0 | 17 |
| Compound ....... | 11 | 4 | 381 | 17 | 156 | 54 | 54 | 54 | 4 | 2 | 1 | 0 |
| Complicated..... | 10 | 6 | 81 | 42 | 54 | 0 | 0 | 0 | 2 | 0 | 1 | 1 |
| Clavicle, Simple ....... | 75 | 63 | 68 | 8 | 31 | 39 | 15 | 23 | 0 | 0 | 0 | 12 |
| Complicated | 2 | 1 | 75 | 75 | 75 | 0 | 0 | 0 | 1 | 0 | 0 | 0 |
| Thigh, Simple ......... | 73 | 60 | 254 | 40 | 92 | 137 | 14 | 66 | 7 | 0 | 0 | 6 |
| Compound..... | 10 | 1 | 0 | 0 | 0 | 45 | 45 | 45 | 4 | 3 | 2 | 0 |
| Complicated.. | 12 | 8 | 174 | 102 | 138 | 221 | 45 | 98 | 3 | 1 | 0 | 0 |
| Neck of Thigh ......... | 4 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 4 |
| Skull, Simple ... ...... | 31 | 13 | 156 | 14 | 57 | 115 | 18 | 66 | 15 | 1 | 0 | 2 |
| Compound ..... | 3 | 1 | 0 | 0 | 0 | 69 | 69 | 69 | 2 | 0 | 0 | 0 |
| Complicated ... | 2 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 2 | 0 | 0 | 0 |
| Ribs, Simple....... ... | 35 | 29 | 116 | 4 | 29 | 0 | 0 | 0 | 3 | 1 | 0 | 2 |
| Complicated.... | 3 | 2 | 72 | 21 | 46 | 0 | 0 | 0 | 1 | 0 | 0 | 0 |
| Jaw, Simple.. ......... | 17 | 11 | 58 | 17 | 38 | 126 | 33 | 79 | 4 | 1 | 0 | 1 |
| Compound....... | 2 | 2 | 0 | 0 | 0 | 126 | 25 | 77 | 0 | 0 | 0 | 0 |
| Patella, Simple........ | 12 | 11 | 151 | 45 | 70 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| Compound .. | 1 | 1 | 140 | 140 | 140 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Fingers, Simple ....... | 8 | 4 | 108 | 17 | 52 | 0 | 0 | 0 | 1 | 0 | 2 | 1 |
| Fibula, Simple......... | 6 | 5 | 47 | 25 | 33 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| Complicated . | 1 | 1 | 73 | 73 | 73 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Elbow, Simple....... | 8 | 8 | 74 | 19 | 51 | 44 | 37 | 40 | 0 | 0 | 0 | 0 |
| Compound. .. | 3 | 3 | 336 | 55 | 197 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Scapula, Simple ...... | 6 | 4 | 106 | 16 | 46 | 0 | 0 | 0 | 0 | 1 | 0 | 1 |
| Vertebræ ................ | 4 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 3 | 0 | 0 |
| Pelvis, Compound .... | 1 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 1 | 0 | 0 | 0 |
| Ilium, Simple | 3 | 3 | 113 | 3 4 | 62 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Ankle, Simple ......... | 2 | 1 | 39 | 39 | 39 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| Compound . ... | 2 | 1 | 463 | 463 | 463 | 0 | 0 | 0 | 1 | 0 | 0 | 0 |
| Sternum, Simple....... | 3 | 2 | 26 | 15 | 20 | 0 | 0 | 0 | 0 | 0 | 0 | 1 |
| Nose, Simple. .......... | 2 | 2 | 19 | 15 | 17 | 0 | 0 | 0 | 0 | 0 | 0 | 0 |
| Not designated ........ | 5 | 4 | 225 | 50 | 107 | 0 | 0 | 0 | 1 | 0 | 0 | 0 |

*American Medical Examiner.* No. 2. 1838.

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*Elm-bark Surgery.* By W. A. M'DOWALL, M.D., of Fincastle, Virginia.

UNDER this title Dr. M'D. has given, in a recent journal, an account of the use of the bark of the slippery elm (*Ulmus fulva*) for bougies, tents, catheters, &c. It is not proposed that the new instruments should supersede the old, but to act as an adjuvant. Its advantages seem to be its ready fashioning by the surgeon to any shape or form, its pliability, smoothness, and great expansibility from imbibition of fluids. Dr. M'D. has employed it as a tent in abscesses and fistulæ, as a bougie in strictures, &c. He particularly recommends it in spasmodic stricture. As, probably, these instruments, or rather the materials for their fabrication, will reach this country, with other novelties, we must put on record the most important caution—and surely no less important drawback to its value—announced in the concluding sentence of this communication: "Some caution is necessary in using bougies or catheters of elm. Although this bark possesses a degree of tenacity surpassed by that of but few trees in the forest, yet, when seasoned and in a very dry state, it would be liable, in the hands of a careless or awkward operator, to break off in the urethra or bladder. To obviate this danger, it should be immersed in water, for a longer period when it is very dry, which will restore tenacity to its outer fibres." *The [Cincinnati] Western Journal of the Med. and Physical Sciences.* No. 43. *December*, 1837.

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## ADDENDUM *to the "American and Colonial Journals."*

[Two or three of the journals from which the preceding extracts are made have not been previously noticed by us. Indeed, the great mass of materials of a scientific and practical character that press on us from all sides, has forced us to curtail our notices of a merely literary and critical character within narrower limits than we could wish. In addition to the *India Journal of Medical Science*, formerly noticed by us, (Vol. III. p. 192,) there has recently appeared, at Calcutta, a *Quarterly Medical Journal*, edited by Drs. Goodeve and O'Shaughnessy, professors in the Medical College of that place. This, like its predecessor, is an excellent work, and cannot fail to be useful to the profession in Europe as well as in India. Emanating, as it does, from the Calcutta Medical and Physical Society, and intended, we presume, to supersede the *Transactions* heretofore published by that body, it contains more elaborate and longer papers than the *Indian Journal*, and deals much less in the lighter literature which distinguishes our weekly journals. It is, in every respect, a very valuable publication, and such as might be expected from the great learning and high respectability of the editors. In both the Indian journals, the Selections from the British and foreign Journals are most copious, judiciously chosen, and well arranged. It is not a little complimentary to us that so very large a share of the extracts from the continental journals, in both publications, is taken from the "Selections" in this Review. We gladly accord to the editors the same privilege we claim for ourselves, in borrowing from our fellow-labourers; but we would suggest to them the same care which we invariably take of indicating the exact sources of our information. To a large proportion of the articles taken from us, the names of the *original* journals are alone subjoined, while that which was at the trouble and expense of selecting and translating them is omitted.

The *Medical Examiner* commenced at Philadelphia with the present year, and promises to be a valuable addition to the already large body of medical journals published in America. It is published *fortnightly*, and consists of one large royal 8vo. sheet, of small print and double columns, like our weekly journals. The Examiner resembles these also in the general character of its contents, and particularly in giving full reports of lectures; which, we believe, had not been previously done by any journal in the United States. It is edited by Drs. Biddle and Clymer; and costs three dollars per annum.

*The Boston Medical and Surgical Journal* has been established eight years, and has now reached its eighteenth volume. It is published weekly, and consists of one 8vo sheet of the common size. It is edited by Dr. J. V. C. Smith; and costs three dollars per annum. It is a highly respectable publication, and contains a good deal of useful matter; but the greater part of this is taken from foreign journals.]

# PART FOURTH.

# Selections from the British Journals.

(FOR THE QUARTER ENDING MAY 31, 1838.)

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## PHYSIOLOGY.

### *Case of Paraplegia.* By WM. ELLIOTT, M.D., Carlisle.

THIS is an ordinary example of Pott's disease. The only point of any interest in it is the well-attested fact of motion being produced in one of the paralysed limbs by irritation applied to the part, and not exciting any previous sensation.

"On pricking, scratching, or pinching the skin of the left foot, indications of sensation of pain occur, and the girl asserts she feels pain: at the same time irregular movements of the left toes, left foot, and left leg, and sometimes of the thigh also, follow, over which she has no control. But, on irritating the integuments of the right foot in a similar manner, there is not, even when blood is drawn, the slightest indication of pain: the girl asserts she feels none; and irregular muscular movements of the right toes, foot, leg, and thigh are produced. These movements are sometimes those of flexion, sometimes those of extension, and are not those producing a withdrawal of the limb, or such as would appear the natural result of the sensation of pain in the part irritated."

*Lancet. April* 14, 1838.

## PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

### *Cases of Acute Inflammation confined to the Epiglottis.* By H. MARSH, M.D. M.R.I.A., one of the Physicians to Stevens' Hospital.

[THIS is a very valuable practical communication, such as might be expected from the experienced and skilful physician who is the author of it. We can only find room for Dr. Marsh's own cases, and a few of the remarks appended to them.]

CASE I. Mrs. T—, between fifty and sixty years of age, robust, and of plethoric habit and florid complexion, had been confined for some days to her bed by a feverish cold. The symptoms having rather abruptly assumed a more serious aspect, I was requested to visit her, having been informed by the medical gentleman in attendance that her case was an obscure one, and that he conceived her disease to be a nervous fever. I found her flushed, agitated, and complaining principally of being restless and sleepless: the moment her eyelids closed, she started suddenly, gasped for breath, and looked like one about to be convulsed. She sat up erect in bed, and would not for a moment venture to lie down. A continual accumulation of tenacious mucus obstructed the fauces, prevented sleep, and produced frequent and urgent paroxysms of dyspnœa; her skin was hot and dry; her pulse 120, contracted and resisting; and, to the enquiry whether she was distressed by thirst, she replied she was afraid to drink, the attempt to swallow was productive of so much pain and suffering. On handing her a glass of water, she seemed to dread to put it to her lips. On making an effort to swallow, a struggle so painful and convulsive, with protrusion of the eyes, ensued, that it gave rise in my mind to the suspicion that she had been bitten by a rabid animal. In attempting to swallow, the fluid was forcibly and convulsively rejected. On examining the interior of the mouth and fauces, I could discover no appearance indicative of inflammation or disease of those parts: the tonsils, uvula, and soft palate, presented a perfectly natural appearance; but the forcible depression of the tongue produced so much pain, and caused such convulsive movement of the muscles, that I was unable to obtain a satisfactory view of the parts which lie more posteriorly. The tone of her voice was scarcely, if at all, altered; the respiration, in the intervals between the paroxysms, was unimpeded, and it was not at any time in the least degree stridulous.

This case fell under my observation early in my professional life, and I felt very much at a loss to account for the symptoms: it occurred to me at the moment to pass my finger along the tongue, and endeavour to ascertain by the touch the cause of the symptoms. On passing the finger over the tongue, as far towards its root as I could reach, I felt a tense, rounded, prominent body, which, upon close examination, was evidently the epiglottis, considerably enlarged and erect. This at once revealed the seat of the disease, and the source of all the distressing symptoms. I had not previously witnessed an instance of this disease, nor had I, in the course of my reading, met with a description of it. My opinion was at once formed: it was now quite certain that I had to do with a case of acute inflammation of the epiglottis, accompanied with high inflammatory fever. The treatment was obvious: the patient was instantly and largely bled from the arm, and leeches numerously and successively applied, so as to maintain for some time a continued flow of blood; the thick tenacious mucus was removed from the fauces from time to time, by means of a sponge attached to a piece of whalebone. After a few hours the symptoms were considerably abated, and some rest was obtained; bleedings from the arm were repeated a second and a third time, and each repetition of the bleeding was followed by a still more evident abatement of the symptoms: she was now able, though with considerable pain and difficulty, to swallow. Calomel and Dover's powder were given in repeated doses; and, the symptoms having continued for some hours stationary, it was determined to introduce mercury by friction, so as to influence the system as speedily as possible. Half a drachm of mercurial ointment was rubbed in every fourth hour: moderate ptyalism was established; the remaining symptoms rapidly subsided, and after the lapse of a few days scarcely a trace of the disease remained. This lady has since enjoyed perfect health, no disposition to a return of the disease having manifested itself.

Case ii. The next very remarkable instance of this affection which fell under my observation occurred in an individual whose constitution and temperament were altogether the reverse of those which characterized the case just related. The disease manifested itself in a young person not twenty years of age, of delicate frame and feeble constitution, and arose in the following manner:—In consequence of exposure to cold, this young lady was affected with slight rigors; fever ensued, accompanied with some tumefaction and redness of the tonsils and uvula. The fever and inflammation yielded to the quiet of bed and to diaphoretic medicines. On the third day she was convalescent. On the evening of the third day, contrary to the injunctions she had received, she got up and exerted herself: on returning to bed, she felt herself chilly and uncomfortable, and spent a feverish and restless night. Next morning she experienced some pain and difficulty in deglutition, and, these symptoms continuing rapidly to increase during the day, I received an urgent message to see her. On visiting her in the evening, I found her in a state of great suffering and distress: she was incessantly harassed with a profusion of tenacious transparent matter, which was generated about the fauces, and every effort to rid herself of this was attended with very great distress; the act of protruding the tongue was extremely painful to her; she feared to lie down; there were frequent paroxysms of dyspnœa; she was agitated and apprehensive to an extraordinary degree; her friends around her were in a state of the greatest alarm; every effort to swallow was accompanied with violent pain, the liquid was rejected with force through the nostrils, and her distress and agitation were much increased. On examining the fauces there was scarcely a remaining blush, and the tumefaction which had existed on the first attack had entirely subsided: it was impossible sufficiently to depress the tongue to obtain a view more posteriorly, but, on introducing the finger, the epiglottis was found greatly swollen, tense, and smooth, and pressure with the finger produced severe pain; there was no external swelling; she experienced some uneasiness on pressure upwards at the highest part of the neck; the face was darkly flushed, anxiety and suffering were strongly depicted on the countenance, and the pulse was incalculably frequent, small, and feeble; the skin was hot, and covered with a clammy perspiration; the voice was natural; there was no cough, but the efforts to clear away the phlegm from the throat were almost unceasing; the respiratory and percutatory sounds were perfectly natural. Leeches were applied externally, as near as possible to the seat of the disease, and fomentations with the decoction of poppies, after the removal of the leeches, perseveringly employed. I visited her again very late at night, accompanied by Mr. Cusack, whose cooperation I was requested to obtain. There was no abatement, rather an increase, of all the urgent symptoms. Mr. Cusack, by sewing a dossil of lint to the end of the finger of a glove, and putting it firmly on his forefinger, was enabled to remove a large quantity of the adhesive mucus from the fauces, and thus procure considerable temporary ease; a short interval of sleep, in the erect position, was thus obtained: leeches were again applied, and fomentations repeated. No medicine could be given by the mouth: purgative injections were administered, and acted well. At our next visit, though no positive abatement of the symptoms could be said to have taken place, yet the patient on the whole seemed somewhat less agitated and distressed: the fauces were again cleansed in the same manner as before. It was now determined in consultation to apply, by means of the dossil of lint attached to the finger of a glove, a solution of the nitrate of silver (ten grains to an ounce of distilled water) to the inflamed epiglottis. At first some pain, afterwards marked relief, ensued from this application, which was repeated at every subsequent visit, the strength of the solution being gradually increased; and it appeared to me that its employment was always attended with benefit. We also determined to commence at once mercurial treatment, the urgency of the symptoms demanding its immediate application. A drachm of mercurial ointment was accordingly ordered to be rubbed in on the inside of the thighs, every fourth hour; and, in addition, to excite as speedily as possible mercurial action in the system, the surface of the body under the bedclothes was exposed to the continual contact of volatilized mercury: the bedclothes being retained in an elevated position, this was readily accomplished. The vapour arising from the hydrargyrum cum creta, heated, was generated abundantly, and retained between the bed-coverings; and, by perseverance in this, the entire surface of the patient's body was kept constantly involved in a mercurial atmosphere. On the third day from the commencement of this treatment, moderate but decided ptyalism was established; the occurrence of which was coincident with a marked and rapid abatement of every distressing symptom. Previously to the constitution having been placed under the influence of mercury, a mitigation of the symptoms had been effected. On the third day the patient was able, though with great pain, to swallow small quantities of fluids; and short but refreshing intervals of sleep were procured. It was not, however, until the end of the fifth day, when the decided action of mercury on the system became apparent, that a complete and permanent subsidence of the symptoms was manifested. After this period deglutition became easy, the paroxysms of dyspnœa no longer recurred, and the sleep was tranquil and refreshing. Towards the end of the third day the epiglottis felt rough to the touch, but still swollen and large: afterwards the feel was that of a body that was wrinkled and puckered, but not much swollen. After the lapse of many days, I examined again the epiglottis, and the touch could detect no abnormal condition of this organ. Convalescence was slow and tedious.

CASE III. The next case was that of a man aged forty, who, after exposure to cold, was affected with much pain and difficulty of swallowing, the liquids regurgitating through the nose in every effort. On looking into the mouth, the tonsils, soft palate, and uvula presented a natural appearance; but at the base of the tongue was seen a round, red, prominent substance, like a small ripe cherry. The attendant fever was considerable. For three days he was unable to swallow anything: he was relieved by frequent relays of leeches; warm baths and anodyne enemata having been used without apparent benefit. The constant oozing of blood from the leech-bites appeared to be the only effectual part of the treatment. On the third day the inflammation began gradually to subside, and the patient ultimately but slowly recovered.

*Remarks.* The anatomical characters and relations of the epiglottis fully explain the nature and course of the symptoms detailed. Being studded with glands at its root, the inflammatory irritation stimulates them to a greatly increased secretion of mucus: this adhesive mucus, in constant efforts to detach it, greatly harasses the patient, produces paroxysms of dyspnœa, and prevents sleep. The connexion of the epiglottis with the root of the tongue accounts for the pain felt when the tongue is moved or protruded, and its situation and the relation which it bears to the muscles of deglutition fully account for its essential symptoms,—pain, spasm, and difficulty in the act of swallowing. The loose attachment of the mucous membrane to its anterior surface will explain satisfactorily the great extent of the tumefaction, and also that the inflammatory distention should be situated on the lingual rather than on the laryngeal aspect of the organ, as well as for the puckered or wrinkled feel of the mucous membrane on the absorption of the effused fluid. . . . . . .

In the cases detailed, the inflammatory action was limited in extent, not extending probably beyond the anterior surface of the epiglottis. The circumscription of the inflammation was marked by positive as well as negative signs. The inspection of the fauces proved that it did not exist in the parts anterior to the epiglottis; the absence of dyspnœa, except in paroxysms; of stridulous breathing; the natural tones of the voice when the mucous matters were detached; the exemption from cough; the result of stethoscopic examination: all these negative signs prove that neither the 'glottis, larynx, nor bronchial mucous membrane shared in the inflammatory action. These cases are therefore examples of unmixed, circumscribed inflammation of the epiglottis.

*Treatment.* Slight cases of this disease are not unfrequent, but require little or no treatment. In cases of medium severity, the symptoms are not very urgent, and will, with mild antiphlogistic treatment, subside; but, in the more acute and intense examples of this disease, the urgency of the symptoms and the sufferings of the patient demand the most prompt and vigorous treatment. From the cases I have observed, and from those recorded, I am disposed to gather that the disease has a tendency to abate either on the third, fifth, or seventh days; but, if not combated by energetic remedies, it may end in suppuration, as it did in one recorded case; or it may extend downwards, involve the glottis, and thus produce a still more dangerous disease, œdema of the glottis; or, by causing repeated paroxysms of dyspnœa, it may give rise to pulmonary infiltration; or it may but partially subside, and leave behind a thickened, indurated, and permanently diseased condition of the epiglottis. To allay the urgent symptoms, and prevent these consequences, active treatment, regulated of course by the patient's constitution, is imperatively required. In one of the cases I have recorded, frequent and large bleedings, general and topical, with the active administration of mercury, were necessary to reduce both the concomitant fever and the local inflammation. In another, the delicate constitution, the feeble pulse, the state of the skin, induced me to restrict bleeding to the reiterated application of leeches as near as possible to the seat of the disease. To subdue inflammatory action, as well as to prevent lingering chronic disease, mercury is invaluable. Fortunately it can be applied as effectually, and I think as rapidly, by the skin, as when administered internally. In cases such as those now detailed, where the ability to swallow is lost, its external application, both by friction and by vapour, is of immense value; and it seems to me that, in the more severe cases of the disease, this part of the treatment should not be omitted. I think the application of the nitrate of silver, as suggested and practised by Mr. Cusack, was decidedly useful. Fomentations long continued, though a minor remedy, are not without their value. Blistering I did not think necessary, nor would I apply a blister till the inflammatory excitement were markedly reduced.

From the administration of tartar emetic at the period of commencing restoration of the power of deglutition, I abstained, fearful of exciting vomiting; which would, I conceive, be distressing, perhaps dangerous to the patient. Were, however, the fever to continue, and the symptoms not to yield satisfactorily, it might be given, guarded with opium in carefully regulated doses. The influence of this combination of medicine, in diminishing excitement and reducing inflammation, is established incontestibly in various conditions of disease; but the symptoms in the cases recorded yielded so completely to bleeding, mercury, and fomentations, that I did not deem it necessary to resort to other means; and in none of them has any trace of chronic disease been left behind.

*Dublin Journal of Med. Science. March*, 1838.

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### *On Tapping the Head in Hydrocephalus.* By T. J. CONQUEST, M.D.

[THIS is an important paper, and affords ample testimony of the great value of the practice, for the introduction of which into medicine we are all deeply indebted to Dr. Conquest. We give the more important parts of this communication in the author's own words; and must, at the same time, express our approbation of the modest and philosophical tone that pervades it.]

Nearly ten years having elapsed since I was first induced to attempt the cure of chronic hydrocephalus by withdrawing the fluid from the ventricles, the time seems to have arrived when the profession has a claim on me for some account of the results of these operations; and, indeed, this has become necessary in consequence of the numerous applications for information on the subject by practitioners, not only in Britain, but in many distant parts of the world. Still, it is a matter involving such important considerations, that, until experience has thrown much more light upon it, I do not feel justified in advancing anything beyond the mere statement of facts, such as the present position of my enquiries warrants, leaving to a future day a more methodical and full investigation of the origin, nature, and progress of this formidable disease, with its appropriate medical and surgical treatment.

The operation consists in passing a small and delicately constructed trocar into one of the lateral ventricles, and drawing off so much fluid as the powers of the constitution will admit of. The most eligible spot at which the trocar can be introduced is in the course of the coronal suture, about midway between the crista galli process of the ethmoid bone and the anterior fontanel, so that the danger of wounding the corpus striatum is avoided on the one hand, and the longitudinal sinus on the other. The instrument usually penetrates about two inches, and in most cases the serum has been colourless, but occasionally tinged with blood. In one instance, and that was in the last child operated on at St. Bartholomew's only a few weeks since, a large and alarming quantity of florid blood escaped; most likely from a branch of the meningeal artery. Sometimes, on withdrawing the trocar, the water will not flow until a probe has been passed along the canula, to remove portions of cerebrum which block it up. After taking away all the fluid that can be removed consistently with safety, the head, which should always be steadily compressed by an assistant during the operation, may be strapped with adhesive plaster, that it may retain its diminished size, and that the fearful consequences of suddenly removing long-continued pressure from the brain may be averted.

I have now tapped in *nineteen* cases, and of these *ten* were living when last heard of. Several of the children, before the operation, were reduced to the most deplorable condition, having frequent convulsions, with loss of sight, emaciation, &c.; but the diminution or disappearance of these symptoms has been very remarkable. In some cases the results have been triumphantly successful; in others, from the reluctance of the parents to have the operation repeated, only temporary relief has been afforded; but none of these children died either during or immediately after the operation; and those which, in the subsequent list, are reported as dead, survived weeks or months after the fluid was withdrawn.

All the operations were performed in the presence of many medical men, and most of them before large bodies of students, at St. Bartholomew's Hospital, and their progress has been watched by gentlemen who felt a deep interest in their termination; and, although exclusive dependence has been placed on the withdrawal of the fluid, without the auxiliary assistance of any pharmaceutical or other medical means, yet I consider much of the success to be attributable to the kind and able superintendence of medical friends.

| No. | Name. | No. of times operated on. | Quantity withdrawn. | Living. | Dead. |
|---|---|---|---|---|---|
| 1 | Catherine Seager .......... | 1 | ℥ xxxij. | 1 | 0 |
| 2 | William Honey ............ | 3 | xxxivss. | 0 | 1 |
| 3 | William Wilmer ........... | 2 | xxiv. | 1 | 0 |
| 4 | John Hall .................... | 5 | xlviijss. | 0 | 1 |
| 5 | Alfred Parman ............. | 4 | xlv. | 0 | 1 |
| 6 | Mary Ragon ............. ... | 3 | xxvj. | 1 | 0 |
| 7 | Charles Discomb ......... | 2 | xx. | 0 | 1 |
| 8 | John Ward .............. ... | 1 | viiij. | 0 | 1 |
| 9 | John Clauditt............... | 2 | xxij. | 0 | 1 |
| 10 | Charles Clarke .. .......... | 2 | xvij. | 0 | 1 |
| 11 | Elizabeth Forster ........ | 5 | lv. | 1 | 0 |
| 12 | Jemima Evans ............. | 1 | vijss. | 0 | 1 |
| 13 | Jane Brocken ............... | 1 | xiij. | 1 | 0 |
| 14 | Eleanor Mahoney ........ | 1 | ix. | 1 | 0 |
| 15 | Francis Chiddy ............ | 4 | xxxiij. | 0 | 1 |
| 16 | Thomas Norman .... ...... | 1 | vj. | 1 | 0 |
| 17 | Anne Armenio .............. | 3 | xxxjss. | 1 | 0 |
| 18 | James Thomson............ | 2 | xiv. | 1 | 0 |
| 19 | John Pratt.................... | 1 | ix. | 1 | 0 |
| 19 | | 44 | 455 | 10 | 9 |

*Lancet. March* 17, 1838.

## *Case of Empyema.* By T. Ogier Ward, m.d., Physician, Shrewsbury.

[This case presents some interesting features, which the excellent pathologist and auscultator who treated it so judiciously, did not fail to remark.]

Sept. 14th, 1837.—Joseph Hadduck, æt. two. In April last (four months ago) he was seized with cough and other symptoms of inflammation of the chest, for which he was treated by a druggist. . . .

Present state:—Great emaciation, but little fever; no night-sweats; pulse 160; tongue clean; appetite good; hacking cough, without expectoration; bowels regular. Sleeps only on left side: left side of chest distended, and dull on percussion, with evident fluctuation; no respiratory murmur over that side, except at the upper part and roots of the lung, where it is bronchial. Heart is felt to the right of the sternum. Respiration clear and puerile over right lung. I ordered mercurial friction to the side, and a mixture containing the iodide of potassium.

18th.—As no improvement had taken place, but the emaciation seemed increased, I requested my friend Mr. Crompton to puncture the chest; which he did, and drew off through a canula above a pint of creamy inodorous healthy pus, without the admission of more than a very minute quantity of air into the chest. During the operation the child coughed frequently, and at each effort the pus was projected in a powerful jet through the canula. After the operation, the left side of the chest was as much smaller as it had previously been larger than the right,—

that is, about three-fourths of an inch; but the heart retained its unnatural position, and the child continued to lie on the left side. No bad symptom followed the operation: on the contrary, there was marked improvement. Nevertheless, the chest filled again, and was again emptied on the 25th. On this occasion the pus was the same in consistence and quantity as before; but it had a greenish tinge, and the external air entered the chest freely, though some of it was withdrawn again by an elastic gum bottle. Percussion of the left side immediately became very clear, but no dashing of fluid was heard upon shaking the patient.

26th.—The child looks much better, and is gaining flesh decidedly; he is lively, and his appetite is keen. The pulse has fallen forty beats since the operation, being now 120 instead of 160. Cough slight; left side much smaller than right, and left shoulder lower, the spine being curved laterally. The heart is still to right of sternum; percussion clear; amphoric sound of dashing of fluid on succussion; respiration only audible at back and roots of lung. . . . The chest again became filled with fluid and air, that gurgled audibly upon moving him even slightly; and, on October 7, he was tapped for the third time, and a similar quantity of pus removed in my absence. . . .

12th.—Fluid increased in quantity, but, from its dash, is evidently thinner than before; cough still less, and sweating has ceased; respiration is clear at back of chest.

17th.—Much better; gains flesh fast. Fluid is less, and, judging from the sound, is of thicker consistence. Heart almost in natural position; respiration more extensive, with a little mucous rattle; no cough nor sweating; pulse 100. From this time the fluid became gradually less, and more dense, till it ceased to be detectible by percussion and succussion. The heart returned to its proper place, the spine became straight, and the ribs expanded: the only symptoms left at the end of October being a little obscurity of respiration, and occasional intermittence of the pulse. The child is now (March 10) in perfect health.

The medical treatment, after the first operation, consisted merely in regulating the bowels by occasional doses of hydr. c. cretâ, and in allaying the cough and arresting the perspirations by a linctus containing quinine and acid. sulph. dil. The skin of the chest was drawn aside at each operation, in order to form a valvular opening. . . .

I need not enlarge upon the utility of fluctuation as a diagnostic (I might almost say a pathognomonic) sign of effusion, although I am not aware of its having as yet been applied to the diagnosis of chest disease. In this case, owing to the thinness and elasticity of the parietes of the chest, the fluctuation of the fluid was very distinct on percussion, and in fact formed a useful guide for the introduction of the trocar; since, for want of knowledge of the exact height attained by the effusion, there was some risk of wounding the lung. Another circumstance that was of great use in determining the situation of the puncture in the second operation, was the fluctuation of the fluid caused by the heart's impulse, that was very evident over the intercostal spaces as high as the effusion existed, but there ceased. This sign I once saw exhibited in a remarkable degree in a case of gangrene of the lung, with destruction of the intercostal muscles, and hydro-pneumo-thorax. As this is the only case in which I have been able to detect the exact level of the fluid by this means, it would be presumptuous to offer more than an opinion that it may be of service in ascertaining the more moderate effusions of acute pleurisy and pericarditis; and I conceive its utility would be restricted to indicating the *presence* only of a fluid, notwithstanding that M. Piorry speaks of a gelatinous elasticity communicated to the finger by percussion over an hydatid cyst. Combined, however, with the next sign that I am going to mention, it affords an accurate notion of the *consistence* of the fluid also. This sign, the sound on succussion, has not, to my knowledge, been noticed by any author, except perhaps by Hippocrates, as indicative of the nature and consistence of the fluid effused into the chest. In the present instance, after the air had been admitted, upon shaking the child, a dash of liquid against the walls, and its fluctuation, were perceptible by the hand, as well as audible to the ear at a distance; and, as the fluid became thicker or thinner, augmented or diminished in quantity, so did the dash become smart or heavy, and the sound acute or dull; so that, after the last operation, finding that, as fluid decreased in the smartness of its impulse, the dulness of its fluctuating sound was increased, I concluded that it was becoming absorbed, and that no further operation would be required. It is difficult to describe these varieties of sound and impulse; but any person may convince himself of the possibility of drawing such distinctions, by shaking oil or treacle, and water or spirit, in separate elastic bottles, when not only the state of plenitude of the vessel will be readily ascertained, as Laennec has already pointed out in his article on Pneumo-thorax, but also the density of the contained fluid: indeed, the difference of sound between water and spirit is very apparent.

*Med. Gazette. March* 31, 1838.

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### *Case of Dropsy of the Pineal Gland.* By S. S. Stanley, Esq., Surgeon, Chelsea.

This case is somewhat curious, from the brief period of the disease and peculiar circumscription of the pathological changes.

Three weeks before her death, the patient, a little girl, æt. four, was affected with some fever and restlessness, and afterwards drooped and moped. Four days before her death, "she ran in a hurried manner towards her mother, to whom she clung convulsively, and then fell down suddenly in a state of insensibility, from which it does not appear she ever for a moment returned to consciousness, further than on one occasion giving her mother a kiss." When seen by Mr. Stanley, the day before her death, she was "in bed, lying quite insensible, with the eyes imperfectly closed, and the mouth slightly open; respiration natural; pulse sixty, small and irregular;" and she remained in this state until she died.

*Dissection.* "There was nothing peculiar seen until the brain was removed, when the pineal gland was found distended with fluid to the size of a large hazelnut. The tunica arachnoidea was separated from the gland without difficulty, leaving it entire: the colour of this body was lighter than natural; the peduncnli not apparently implicated; the sac was of various thickness, being thinnest at its upper part, where the density was about a line; in the inferior part, or the basis, it was about two lines in thickness. The enlarged body pressed in a conspicuous manner against the nates of the corpora quadrigemina, and laterally against the thalami optici. The tubercula quadrigemina, and pons varolii, were in a state of ramollissement, probably arising from the pressure described."

*Lancet. March* 24, 1838.

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### *An Account of two Cases of Bronchial Polypus; with Remarks.* By John North, F.L.S.

This paper is an interesting addition to our knowledge of the affection to which it relates. It professes to contain no fresh facts; but we do not recollect to have seen noted, in other cases of the same kind, the stethoscopic indications. The cases are well detailed, and the remarks show learning and sound practical sense.

*Med. Gazette. May* 19, 1838.

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### *Two Cases of Hydrophobia: remarkable Effect of Morphium administered endermically.* By John Burne, M.D., Physician to the Westminster Hospital.

The only thing remarkable in these two fatal cases was the effect of morphium in the first, that of a youth, aged seventeen. It may be well to observe, that although the symptoms were decidedly hydrophobic, there is no proof that they originated from the bite of a rabid animal. We give the conclusion of the case in Dr. Burne's own words:

"He was now materially changed for the worse, and fast approaching dissolution, of which he was fully conscious. He was frequently eructating and retching, throwing up bile and ropy mucus. He was sweating profusely, and struggling with intense crampy spasms of the upper and lower extremities, calling and hollowing out, as the spasms seized him, for three or four persons to rub him, one to each extremity. The eyes were staring, the face was frequently and suddenly contorted, and the head twisted round to the right: at times the trunk was arched suddenly backwards, (opisthotonos) by violent spasms of the dorsal muscles; and spasms of the diaphragm interrupted the respiration almost to suffocation, so that he could speak only in broken words or sentences. He begged to have his throat rubbed, it was so painful from perpetual, distressing, ineffectual efforts to swallow. Often he desired gruel, which, when presented to him, he dare not put to his lips, but would dip his finger into it, and carry this to the roof of his mouth, afraid to approach the muscles of deglutition. He was giddy, recollected with difficulty, moaned distressingly, and altogether presented a picture of suffering and distress such as I never witnessed. The pulse could be felt, but not counted, owing to its rapidity, as well as to the spasmodic twitchings of the tendons and perpetual movements of the body.

"In this state, Mr. White and I considered what could possibly be done to assuage the poor fellow's sufferings. Medicine could not be given by the mouth, not even put upon the tongue, nor by injection. I suggested the endermic use of morphium: accordingly, about ten grains of the acetate were sprinkled on the blistered surface along the spine, and we waited the effect. Scarcely had one minute elapsed when we observed the stare of the eyes and the dreadful alarm and anxiety of the countenance to diminish, then the violence of the spasm to abate, and the catchings in the respiration and the retching to subside; and to our astonishment this general amelioration progressed, till, in four minutes, the countenance had become placid and the respiration free; the retching had ceased, and the spasms vanished! Advantage of this repose was taken to give him some brandy and gruel, and afterwards some broth; of all of which he drank eagerly by a convulsive effort from his own hand, and said they warmed and comforted him. He then fell quietly back, as one fatigued and sleepy, lay down on the pillow on his right side, with his hand under his head and his knees drawn up in a most natural and comfortable position, free from pain and spasms. In this tranquil state we watched him for a quarter of an hour, and having shut out the light and directed the attendants to withdraw from his bed-side, in the hope that sleep might ensue, we retired; leaving instructions, that should the effect of the morphium pass away and the spasms recur, the morphium should be again administered, so as to keep him under its influence; also that a narrow blister should be applied in the region of the diaphragm, to facilitate the further use of the morphium, if requisite. So wonderful an effect from medicine I never saw. At eleven o'clock at night we visited him again. The influence produced by the morphium had continued, more or less, for three hours, when the spasms and other symptoms having recurred, the acetate of morphium was a second time applied, and a second time the symptoms yielded, though not so entirely as before, nor until nearly half an hour after the application. The influence of this second application had now passed away; the symptoms had returned, though in a mitigated form, especially the cramps of the extremities, which have scarcely troubled him since the first use of the morphium. The powers of life, however, are fast declining, the pulse is barely perceptible, and the face is growing sharp. We, nevertheless, did not think it right to abandon the case without another effort; Mr. White, therefore, raised the cuticle by passing a hot iron over wet silk laid upon the epigastrium, and to this fresh surface more morphium was applied, but in vain: the decline of the powers of life had probably rendered the body no longer susceptible. We left him about one o'clock, sinking, and he died in the course of two hours, having become outrageous and frantic some short time previous to dissolution. Death took place early on the sixth day of general indisposition, and in about forty-four hours after the supervention of the hydrophobic symptoms." *Med. Gazette. April* 14, 1838.

# SURGERY.

## *On Creosote, as a local Application.* By George Fife, M.D., Newcastle-upon-Tyne.

This communication contains ten cases of local disease, in all of which an ointment, composed of half a drachm or forty minims of creosote, and an ounce of lard or cerate, applied to or rubbed on the part affected, about twice daily, was productive of immediate and permanent relief. Four of the cases were of obstinate ulcer, in one of them assuming the form of lupus. "The above cases," says Dr. Fife, "will, I think, be received as tolerable evidence of the utility of creosote in not merely obstinate, but even malignant ulcers; and to these many others might be added. It is, however, only an useless expenditure of time to do so. I shall content myself with affirming, that in no case of ulceration in which I have yet tried it has it disappointed my expectations. I was first induced to try it from the perfect success attending its application to a case of cancer in the ear of a dog, about two years ago, a disease which I have no doubt many of the readers of the *Gazette* know to be most intractable; in fact, to defy every other means. In the case in question it was used in its concentrated state."

Two were cases of spontaneous neuralgia; two of neuralgic pains from severe sprain; one of the disease usually termed "rheumatic gout;" and one of ulcer of the throat. In this last case the creosote was used in the form of gargle. These cases are described with simplicity and commendable brevity, and deserve attention. *Med. Gazette. April* 7, 1838.

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## *Case of Scalded Glottis, in which Laryngotomy was successfully performed.* By F. E. Hicks, Esq.

This case, as the author observes, "illustrates well the efficacy of calomel in scalded glottis, as recommended by Dr. Wallace, of Dublin, and also the necessity of sometimes assisting it by the operation of laryngotomy, without which it was clear in this case the calomel would have had no time to produce its effect."

On Thursday, March 1st, 1838, I was sent for by my friend Mr. Richardson, surgeon, of Bayswater, to see with him a little girl aged five years, who a few hours before had taken a tea-kettle of boiling water from off the fire, and drank from the spout a portion of its contents. At first the symptoms did not appear to be very urgent, but at the time I saw her the difficulty of breathing had become so alarming, owing to the inflammation and tumefaction which had taken place about the upper part of the larynx, as to threaten immediate suffocation; indeed, it was clear that unless something were done to relieve the breathing the child could not survive many minutes. I accordingly (assisted by Mr. Richardson) proceeded to perform the operation of laryngotomy, opening into the larynx in the usual situation between the thyroid and cricoid cartilages. The child being exceedingly restless, it was not thought right to introduce any tube into the opening; neither did we find that it was afterwards required, as our patient continued to breathe very freely through it for three or four days, when the air began to find its way through the natural channel, and it healed. For the first week or ten days the child refused to swallow any food, and required to be nourished with injections of broth and milk. Small doses of calomel, however, mixed with sugar, and placed dry in the mouth, appeared to make their way into the stomach, as on being repeated at short intervals for three or four days the system became mercurialized. Under this treatment the sloughs in the mouth rapidly separated, and at the expiration of a little more than three weeks from the accident, both the sores in the mouth, and the wound made by the operation, have healed, and the child is nearly restored to health.

*Med. Gazette. April* 7, 1838.

## *On some Effects resulting from Wounds of Nerves.*

By JOHN HAMILTON, L.R.C.S.

[THIS paper contains some interesting facts. It reclaims for the nerves what many modern surgeons have been disposed to attribute to the vessels. We have only space for one of the cases, and the conclusions drawn from the whole by Mr. Hamilton.]

CASE II. Catherine Cantwell, ætat. twenty, apparently in good health, but somewhat hysterical, came to me at the South Eastern Dispensary, with a wound in the palm of the hand. While contending with her brother for an apple, she went to give him a slap, when the palm of her hand came on the point of a knife, which entered at the middle of the ball of the thumb at its inner side, and nearly penetrated through. There was little bleeding, and the wound healed in a few days; but then a train of symptoms arose of a most troublesome character; the palm and back of the hand became greatly swollen, the swelling of a pale, œdematous character, exquisitely tender, and extremely painful; the tenderness was greatest about the cicatrix, and exceeded anything I had seen in other diseases. She got no sleep at night, when the pain was worse. At the same time a red line sprung up on the front of the foramen, at its external side, like that from inflamed lymphatics, but without the hard and knotty swelling. These symptoms were, for a time, diminished by antiphlogistic means, but recurred again and again; and about two months after the wound, the swelling got so red and large, and the parts about the wound assumed so much the appearance of a collection of matter, that I should have thought there was one, but that I had so often seen nearly all the swelling and redness disappear in a night, and after remaining away a few days, would re-appear at night, in the course of an hour or so, and for days assume a periodical character, increased and red towards evening, and during the night, and decreased and pale in the morning. The pain, if possible, became greater, the tenderness extreme, a sensation of pins and needles was felt in the hand and fingers, most so in the fore-finger and thumb, with pain and shooting up the arm, numbness, and feeling of deadness, and loss of power. The fingers and thumb were somewhat contracted, and could not be extended without great agony, nor could they be completely flexed, so as to hold any body firmly. She suffered also from hysterical symptoms, globus, palpitations, and low spirits. Having tried the usual routine of antiphlogistic remedies, general and local, as also, latterly, bark and other tonics, with nothing more than a very temporary alleviation, I was desirous of putting her on the use of mercury, but previous to doing so, showed her to Mr. Crampton. He agreed with me as to the symptoms being the result of a wounded nerve, and as to the propriety of administering mercury, suggesting, also, the local application of belladonna, which, however, had been used. Direct salivation was established, the happiest effects were manifest: the great pain and swelling went away, leaving slight tenderness, slight swelling, of a pale colour, on the back of the hand, and an occasional feeling of pins and needles. These were a long time before going off, three months after there being still the slightest possible œdema on the back of the hand, and a trifling degree of tenderness about the cicatrix and palm. She also complained that at night the pins-and-needles' sensation came on, and the hand and arm, always moist, then became covered with large beads of perspiration. After a time, these symptoms only recurred every second or third night; but, though able to follow her usual employment at the end of nine months, it was at least a year before she was quite well.

From the cases here given, and many others that I do not wish to tire my readers with, I feel myself justified in stating, that a wound of a nerve, or any of its branches in peculiar states of the constitution, may be followed by most distressing local and constitutional symptoms, viz.:

1. Pain of the severest character in the injured part, or shooting from it in various directions in the course of the nerves to the extremities, or to the brain. That it is often periodical, and generally attended with the most exquisite tenderness.

2. That with the pain there is frequently redness and swelling, resembling a good deal the appearance of the skin in inflammation of the fascia or a deep collection of matter, but differing, in being of a paler colour and more œdematous character, in being subject to sudden increase and diminution, and at times being only periodical.

3. Contraction of the limb, and spasms of an unusually violent character; the spasms in some instances going into the genuine convulsion of epilepsy.

4. That the constitutional symptoms are those commonly called nervous and hysterical, viz.: depression of spirits, prostration of strength, globus, &c., but that hectic fever has also presented itself.

5. That all the symptoms, uninfluenced by every remedy made use of, have gradually declined after a long interval, having apparently undergone a natural cure; but that in a few instances the termination has been fatal.

It is not easy to account for these symptoms being present in only a few cases of wounded nerve; for in the greater number of injuries daily occurring, in which the nerves must be wounded, they do not ensue. They are most frequently observed in nervous and hysterical women, but not always.

*Dublin Journal of Med. Science. March,* 1838.

## *Case of Talipes Varus Verus, affecting both Limbs.*

By W. J. Little, M.D., *London.*

[This is a striking example of the great value of the operation revived by Dr. Stromeyer, and so successfully practised by Dr. Little and other surgeons in this country. The patient was a lad, aged sixteen, and the deformity was congenital. The following extracts give the more important parts of the case, and also show Dr. Little's mode of operating.]

*Condition of the Patient when seen by Dr. Little, (April 5th,* 1837.) The first circumstance which strikes the observer is the imperfect development of the whole organization of the lower extremities, which are disproportionately short, compared with the length of the trunk of the body, and with that of the upper extremities; the muscles of the pelvis and thighs are, although well cased in adipose tissue, much below the usual standard, while those of the leg are still less developed; indeed, there is a total absence of the calves of the legs from atrophy of the gastrocnemii. The shortness of each limb depends less upon deficiency of the femur than of the tibia and fibula; the shafts of the latter can also be distinctly felt to be more slender than natural. The feet are also stunted in their growth; both heels are held up two inches from the ground by the shortness of the gastrocnemii, and the toes are drawn inward, and the inner margins of the feet upwards, by the shortness of the tibialis posticus, so that the great toes are in contact with one another, and the patient touches the ground by that part only of the plantar surfaces of the feet, called the ball of the little toe. This part, which is an oval of an inch and a half in length, is covered by a painful corn, through having had to bear all the weight of the body. Of the remainder of the soles of the feet, the ball of the great toe, the heel and under part of the instep, no part has ever come into contact with the earth. There is very little mobility of the anklejoints, particularly of the left, scarcely sufficient to enable the surgeon to render the Achilles' tendon tense; the ligamentous tissues are very rigid, and those of the inside of the feet (lig. deltoidea), as the form of the feet indicates, are particularly contracted; the legs are both œdematous, and the skin purplish, from languor of the circulation in them; their temperature, also, is unusually low.

The patient's mode of progression, when he has boots on, which, having heels and broad soles, are accommodated to his lameness, is extremely laborious and difficult; his arms maintain the equilibrium of his body, as without useful gastrocnemii, and with so extremely small a point of support upon the earth as is afforded by the ball of the little toes, he would be in constant danger of falling were it not from the agility the constant practice from infancy has afforded him; his arms are almost constantly in motion, until his body has found the perpendicular; like the dancer upon the tight or slack rope, he raises the one foot over the other alternately, giving the limb, whilst it is moving, a peculiar semicircular swing, first outwards, then forwards, and, lastly, inwards. Without his boots, unaided by the arm of his mother, or by two sticks, he cannot make a single step. The peculiar swing I have described, arises from the endeavour to avoid touching the toe of one foot against those of its fellow, and to bring as large a part of the sole against the ground as possible; for if such a patient were to place his feet perpendicularly on the ground, the point of the foot would turn gradually still more inwards, and the ankle outwards, the tendency of which would be to cause him to tread upon the dorsum pedis, in which case, although the deformity is increased, progression is less laborious to the patient.

*Operation*. April 11, 1837. I divided both the Achilles' tendons. The knife used is a very narrow, slightly curved, sharp-pointed bistoury, the blade one line in width at the widest part, and an inch and a half long, ground to a slightly concave cutting edge, in half only of its length, the remaining half (that connected with the handle) being left blunt. The operation I perform as follows,—the patient being seated in a chair, one assistant supports the knee firmly, whilst another, grasping and drawing downwards the patient's heel with his left hand, and pressing upwards the toes and front of the foot with his right, renders the tendon to be divided as tense as possible. After feeling with my left thumb and forefinger the outline of the tendo Achillis (enveloped in this patient in much adipose tissue, owing to which no relief of the tendon was visible externally), I pass the small bistoury through the skin, one or two fingers' breadth, above the malleolus internus, with one of its sides turned towards the tendon and immediately above it (the patient sitting), the other directed towards the deeper muscles and the tibial vessels and nerves; as soon as I know that the point of the knife has been passed beyond the external edge of the tendon, and has nearly reached the skin of the opposite side, I turn the knife so as to bring its cutting edge to press against the tendon, which is divided generally at one stroke, in the act of withdrawing the knife from the limb. The complete division of the tendon is known by the sensation of the immediate cessation of the tense resistance, and by feeling, before the knife is wholly withdrawn, that nothing remains undivided except the flaccid integuments. (The operation does not occupy a quarter of a minute.) After the withdrawal of the knife, there remains a single puncture in the skin of less than a line in length, from which seldom oozes more than a single drop of blood; such was the case in both legs in the present instance. A few strips of adhesive plaster, and a roller, were applied, and each leg and foot secured to a pasteboard splint, adapted to their deformed shape, to prevent motion of the joint, as it is a wise principle of the Stromeyerian method to make no attempt to draw down the heel, or improve the form of the foot (which would be prematurely interfering with the effusion of lymph between the ends of the tendons, and with the closure of the external punctures), until the latter heals, which it uniformly does by first intention.

Everything proceeded favorably, and the latest report, dated, September 1, 1837: At the present time, he informs me that he can walk about all day with comfort, ten or twelve miles without difficulty, in the capacity of errand-boy. It could not be known that he had formerly had two congenital club-feet; the only remains of them are a slight turning in of the toes when he is fatigued, and, consequently, careless about his gait, and an evident want of the natural freedom of action in the ankle-joints. The mass of muscle has evidently increased. He did not wear the Scarpa shoes after the tenth week, and has worn a common pair of lace-up boots ever since, stiffened on the inner side of the ankle with an additional piece of leather.

[In three subsequent Nos. of the Lancet (March 31, May 19 and 26,) there are three more cases of distortion by Dr. Little. The cure in all was equally prompt and complete, and by the same means.]

*Lancet. March* 17, 1838.

## *Remarks on Lenticular Glaucoma.* By WILLIAM MACKENZIE, M.D., Surgeon-Oculist to the Queen, for Scotland.

THE object of this paper, which is written with the clearness and deep knowledge of the subject, observable in all the author's communications on diseases of the eye, is to prove, contrary to the general opinion of the most distinguished oculists, "that by far the greater number of glaucomata depend essentially on the state of the lens." "I am inclined to think (says Dr. Mackenzie) that there is never any very distinct glaucomatous appearance (that is to say, cloudiness of a greenish hue,) except what is caused by the amber or reddish-brown colour of the central and posterior laminæ of the lens. In lenticular glaucoma, the lens has become, in a certain sense, dichromatic, being of a deep amber when allowed to transmit the light, but appearing green by reflected light; the green hue being probably the result of the absorption of the extreme prismatic rays of the light entering the eye, while the middle prismatic rays are but little affected."

The author also shows that the *catoptrical examination* of the eye, in the manner proposed by M. Sanson, (noticed at p. 231 of our present Number,) "confirms, in the most satisfactory manner, the doctrine that glaucoma is, in general, an affection of the crystalline lens." *Med. Gazette. April* 14, 1838.

## *A Mode of relieving Patients labouring under Enlargement of the Veins of the Testicle.* By THOMAS WORMALD, Esq.

WHEN cases of varicocele are allowed to proceed without any active means being adopted for their relief, the patients may experience much inconvenience from pain in the loins and spermatic cord, and frequently are incapacitated from walking any considerable distance.

P. W., aged nineteen, applied to me in the year 1832, in consequence of a circocele of very large dimensions, which had existed two years, and had been progressively getting worse. The veins were distended to the size of a large apple; so much was he inconvenienced, that a walk of half a mile produced great pain in the back and spermatic cord. After a consultation with Sir A. Cooper, cold lotions, suspensory bandages, &c., having been employed without affording the slightest relief, Sir Astley recommended the removal of a portion of the scrotum. To this proceeding the patient would not consent. I therefore adopted the following mode of treatment:—A ring, about an inch in diameter, made of soft silver wire, of a suitable thickness, was padded, and covered with wash-leather. Through this I drew the lower part of the scrotum, whilst the patient was in the recumbent position, and the veins comparatively empty. I then pressed the sides of the instrument towards each other with sufficient force to prevent the scrotum escaping. The use of this instrument every morning before the patient rose from his bed, enabled this gentleman to walk nineteen miles on the third day after the first application; and although he has for six years worn an instrument of this description, he has never experienced the least inconvenience. And I may add, that other patients (and amongst them medical friends) labouring under varicocele, have found the greatest relief from this simple contrivance.

*Med. Gazette. April* 28, 1838.

## *Treatment of Intussusception by Inflation.* By S. MITCHELL, Esq. Surgeon, Kingston.

[ALTHOUGH this is not a new practice, it deserves notice; the more so as we know that it has succeeded in other instances.]

The case presented all the usual symptoms: intolerable restlessness, the most obstinate sickness, the singularly distressed state of countenance and shrunken features. The usual remedies were had recourse to,—viz. warm baths, clysters, anodyne frictions over the abdomen, &c.; but without avail. As a forlorn hope, I made trial of inflation by means of a clyster-pipe attached to a common pair of bellows, with the most happy result: the sickness immediately ceased, the child within an hour passed a natural stool, fell into a sleep, and in the morning was almost without ailment. *Lancet. March* 17, 1838.

# PART FIFTH.

# Medical Intelligence.

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## ON THE PRESENT STATE OF MEDICINE AND MEDICAL EDUCATION IN HOLLAND.

By C. J. Nieuwenhuijs, m.d., Member of the Provincial Medical Commission for the Province of North Holland, &c. &c.

### A. OF THE MEDICAL INSTITUTIONS.

Holland contains three universities,—those of Leyden, Utrecht, and Gröningen. Harderwyke and Franecker were formerly the seats of universities, but, owing to the small number of students at each, were disfranchised early in the present century. Linnæus for some time studied, and ultimately graduated, at Harderwyke; and here was published his Amænitates Academicæ. There are also three subsidiary colleges, or Athenæums,—one at Amsterdam, another at Franecker, and the third at Deventer. Each of these has five Faculties; and the medical Faculties are subjected to arrangements in accordance with the general system of instruction established in Holland. Wardens or directors (curators) are appointed to preside over each; and these functionaries are selected from the most respectable classes, and generally with reference to their literary acquirements. Every university has its rector-general, secretary, five deans and secretaries; one of each of the latter to each faculty. The rectorship is an annual office, held by the professors in rotation. The different faculties elect annually their own deans; but the secretaryships are for the most part life-offices, and are generally held by the senior professor: thus, Sandifort is secretary to the medical faculty in Leyden.

### I. THE UNIVERSITIES.

#### 1. THE UNIVERSITY OF LEYDEN.

Among the universities, that of Leyden still holds the first place, on account of the excellency of its arrangements and institutions. It was founded in the year 1575, and contains the following professors: Of the Faculty of Physics and Natural History, nine; of the Faculty of Literature and Philosophy, eleven; of the Faculty of Medicine, five.

The following are the names of the professors, and the subjects of the lectures relating to medical science delivered in the Faculties of Physics, Natural History and Medicine, in the session of 1837-8:

| | |
|---|---|
| C. G. C. Reinwardt | Chemistry and Botany. |
| J. G. van Breda . | Natural History, Anatomy, Physiology, and Comparative Anatomy. |
| J. van der Hoeven, (Prof. Extr.) | Zoology, Comparative Anatomy and Physiology, Anthropology, Entomology. |
| A. H. van der Boon Mesch, (Prof. Extr.) | Chemistry, Practical Chemistry, and Pharmacology. |
| G. Sandifort . . | Anatomy, Physiology, Dissections. |
| M. J. Macquelyn . | Pharmacology, Dietetics, Clinical Medicine. |
| J. C. Broers . . | Surgery, Clinical Surgery, Midwifery, Forensic Medicine. |
| C. P. van der Hoeven, | Pathology, Practice of Physic, Clinical Medicine, History of Medicine. |

This university is well known, both on account of the distinguished men who have flourished in it, and for the excellence of its scientific museums and other arrangements. The Museum of Natural History is in no way inferior to that of Paris, and in many respects even excels it, especially in the beauty, variety, and extent of its osteological department. It has lately been augmented by a complete and systematically arranged collection of birds, which has been presented to it by the present director, Themmink. The Museum of Antiquities has been but recently established, and was not, in July 1837, open to the public: it is under the immediate superintendence of that learned antiquary, Dr. Lamans, and is augmenting daily both by purchases and presents. The anatomical and pathological collection, which is already very rich, is also constantly increasing, through the zealous perseverance of Professor Sandifort, who follows the track of his excellent father. The professor of anatomy is, *ex officio*, director of this collection. In it we find some of the most successful of Ruysch's injections, preparations of Albinus, father and son, Bonn, Rau, Brugmanns, Sandifort, sen., and others. The preparations from which the drawings were taken for Sandifort's large work, entitled "Observationes Anatomico-pathologicæ," are to be seen here. An extensive Japanese collection is under the direction of Dr. de Siebold, its principal collector, and is open to the public twice in the week. The University Library is extensive: it is very rich in old medical books and manuscripts, and is not behind any continental library in its collection of modern standard works of all languages. The Botanical Garden forms one of the great attractions of the town, of which the inhabitants may well be proud. It is situated at the back of the university, and extends from hence down to the Rhine, a branch of which entirely surrounds the town. The plants have all been arranged according to their natural order: this arrangement, with many other improvements, having been effected by the late and present directors, Brugmanns and Reinwardt. The lectures on botany are delivered in a theatre within the garden; and, on the conclusion of the lecture, the professor proceeds with the pupils to that division occupied by the natural family he has been discussing, and then enters somewhat more into the analogies and other particulars of the whole group: he is also ready to answer any enquiries that may be put to him by the pupils. The hothouses are large and extensive, and are well stored with oriental and other plants. A fine specimen of the Fraxinus ornus, planted by Boerhaave himself, when professor of botany and chemistry, is to be seen in the upper garden.

The lectures are (with the exception of the clinics) all delivered in Latin; some at the houses of the respective professors, others at the hospital: among the latter are medicine, surgery, midwifery, and the clinics.

The hospital is well arranged: it contains about fifty beds, nearly equally divided between medical and surgical cases, including among the latter midwifery; for it is also a lying-in establishment. The physicians are Professors Macquelyn and Pruijs van der Hoeven; the surgeon and obstetrician, Professor Broers. The pupils have the privilege of dressing and of prescribing for the patients, under the direction of the medical officers.

These are the principal of the public medical institutions. Our view of Leyden, as a seat of learning, would, however, be scarcely complete without referring to the private collection of Professor Broers, and to the magnificent library and museum of Professor Reinwardt. The specimens of diseased bone in the former may, from their beauty of preparation, their extent and rarity, be considered as unique. The latter collection is particularly rich in botanical works. The esteemed owners of them are always ready to make them as serviceable as possible. The emolument of the professors is derived from two sources, from government and from the pupils. Nothing but an adequate and extensive medical and surgical clinic is wanting to render the university complete.

When we consider that the expenses of the university amount yearly to the sum of 118,000 Dutch guilders,* (nearly 10,000*l.*) we cannot refrain from expressing our astonishment that it should be incomplete in any department, or leave anything to be wished for.

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* A Dutch guilder, or florin, is in value about 1*s.* 8*d.* English.

### II. THE UNIVERSITY OF UTRECHT.

This university was founded in the year 1636, and celebrated, in June 1836, the jubilee of its two hundredth anniversary; of which a description has been published; and which will obviate the necessity of any further elucidation on our part.

In the year 1835, there were 519 pupils at this university.

The curators are six in number.

The professors of the Faculty of Physics and Natural History are seven in number, and of Medicine nine.

The following are the names of the medical professors, and the subjects taught by them:

T. L. C. Schroeder van der Kolk, Anatomy, Physiology, Pathological Anatomy, Dissections.
B. F. Suerman . . Pathology, Surgery.
J. T. Wolterbeck . Nature and Treatment of Diseases, General Therapeutics, Pharmacology, Semeiotics, Clinical Medicine, Clinical Midwifery.
N. C. de Fremery . Pharmacy (in the Dutch language), Chemistry, Mineralogy and Geology, Medical Police.
P. T. S. Fremery . Animal and Organic Chemistry.
C. A. Bergsma . . Medical and Economical Botany, Anatomy of Plants, Botanical Excursions.
T. Kops . . . . Botany, Physiology of Plants.
T. G. van der Ledth de Jeude, Natural History, Zoology, Comparative Anatomy.

Utrecht possesses a botanical garden, an anatomical theatre, and a physiological and pathological museum, which formerly belonged to Professor Bleuland, and was ceded by him to the university.

This university costs the government 70,000 florins yearly; notwithstanding which, it is but in a very incomplete state.

The Veterinary Medical Institution is unconnected with the university: it is situated outside of the gates of the town, is under the direction of Professor A. Numann, and is provided with a good hospital and other requisites for diseased animals. This school is said to have collected, by its own exertions, a very fine physiological and pathological cabinet; of which an account has lately been published.

### III. UNIVERSITY OF GRÖNINGEN.

This university is situated in the eastern part of Holland: it was founded in 1614. In the year 1835 it had only 276 students.

The curators are six in number.

The following are the professors of the Faculties of Medicine and Natural History:

S. E. Strating, J. Boast de la Faille, A. A. Sebastian, Physiology.
H. Woltherr . . Clinical Medicine, and Medicine.
W. Verschuir . . Clinical Surgery, and Surgery.
H. Jonxis . . . . Prosector of Anatomy.
J. C. Hoffman, S. Strating, Chemistry.
C. H. van Hall . Botany.

Although the university of Gröningen is of such little utility, its expenses are the same as those of Utrecht, namely, 70,000 florins a year. It is deficient in many requisites; the enumeration of which does not, however, belong to this place.

## II. SUBSIDIARY COLLEGES, OR ATHENÆUMS.

### I. ATHENÆUM OF AMSTERDAM.

This Athenæum was founded on the 8th of January, 1632, and has consequently existed 206 years. It is celebrated for the distinguished men that have adorned it; as a proof of which, we need only mention the names of Tulp, Ruysch, and A. Bonn in former times, and that of G. Vrolik in the present.

The curators are four in number.

The medical professors are: G. Vrolik, F. van der Breggen, W. Vrolik, W. H. Vriese, W. S. Swart, C. B. Tilanus, and G. C. B. Suringar.

These last two professors have been appointed honorary professors at the Athenæum, because they were professors at the Clinical School, an institution in some measure connected with the Athenæum, and which we shall therefore notice in this place, as well as all the other institutions in Amsterdam connected with medical science.

### THE CLINICAL SCHOOL, HOSPITALS, &c.

By a royal decree, dated January 6, 1823, a so-called clinical school was established in the principal town of each province, for the education of surgeons, apothecaries, rural surgeons (Plattelands Heelmeesters), men-midwives (Vrowmeesters), and midwives (Vrowvrouwen). In accordance with this decree, there is now a clinical institution connected with the Athenæum, though the union between the two institutions is not so close as might be wished for the advancement of medical science. It was originally intended that the superintendence of the clinical school should be committed to the Medical Board in North Holland; but several circumstances, unconnected with this statistical account, have caused this intention to be modified, and at present the superintendence has been intrusted to the minister of the interior, who elects a board of commissioners, consisting of the governor of this province, or a member of the provincial States, as president, three members of the above-mentioned Medical Board, one member of the town-council, one curator of the Athenæum, and one director of the hospitals.

In this school the students are instructed in the following branches of science: Anatomy, Natural History, Physiology, Botany, Chemistry, Pharmacy, Materia Medica, Pathology, Therapeutics, Surgery, and Obstetrics. The instruction in all the Athenæums is conveyed in Dutch. Formerly the lectures at the Athenæum were delivered in Latin, but at present they are given in Dutch; and, by this arrangement, the two institutions stand in closer connexion.

The following are the names of the professors, and the subjects on which they lecture:

| | |
|---|---|
| F. van der Breggen, | Pathology, Nature and Treatment of Diseases, Forensic Medicine, Pharmacology (if required). |
| W. Vrolik . . . | Anatomical Dissections, Zootomy, Physiology, Pathological Anatomy. |
| W. S. Swart . . . | Chemistry, Mathematics, Physics. |
| W. H. Vriese . . | Botany. |
| C. B. Tilanus . . | Surgery, Midwifery, Clinical Surgery. |
| G. C. B. Suringar . | Institutes of Medicine, Practice of Medicine, Clinical Medicine in St. Peter's Hospital. |

In this school, rural physicians and surgeons, (allowed to practise in both capacities, as well as in midwifery,) town surgeons, apothecaries, and midwives, are qualified. The male pupil must have attained his sixteenth year; the female, qualifying for midwife, the age of between twenty and thirty, and possess the requisite abilities and capacity, to be admitted by the Provincial Board. This admission costs the pupil three florins; in addition to which, he pays fifteen florins for every course he attends.

The surgical pupil is required to attend the following courses:

First Year:—*First Course,* Anatomy, Natural History, Chemistry; *Second Course,* Anatomy, Physiology, Botany.

Second Year:—*First Course,* Anatomy, Physiology; *Second Course,* Anatomy, Physiology, Pathology.

Third Year:—*First Course,* Materia Medica, Pathology, Surgery; *Second Course* Materia Medica, Clinical Surgery.

Fourth Year:—*First and Second Course,* Clinical Surgery.

The student who intends practising both medicine and surgery must attend the following courses:

FIRST YEAR:—*First Course*, Anatomy, Natural History, Chemistry; *Second Course*, Anatomy, Physiology, Botany, Pharmacy.

SECOND YEAR:—*First Course*, Anatomy, Physiology, Pharmacy; *Second Course*, Anatomy, Physiology, Materia Medica.

THIRD YEAR:—*First Course*, Materia Medica, Pathology, Chemistry; *Second Course*, Therapeutics, Chemistry, Surgery, Clinical Medicine.

FOURTH YEAR:—*First Course*, Therapeutics, Clinical Medicine and Surgery; *Second Course*, Clinical Medicine and Surgery.

No student is admitted to the lectures given on Midwifery till he has attended, for the space of two years, the preparatory lectures on anatomy and pathology; and no licence is granted for giving instruction in this art until the last year of study, and after the pupil has diligently attended the lectures on the state of pregnant and puerperal women and new-born children.

Students in Pharmacy are required to attend lectures on Natural History, Chemistry, and Botany, during the first and second year after their admission.

Females, studying the Obstetric Art, are required to follow the course of study indicated by the professor; but this course is always required to be in accordance with the royal regulation of the 6th of January, 1823.

The professors hold yearly a general examination of all the pupils in the Clinic, in presence of a member of the Provincial States, a member of the town administration, and one of the Board of Direction of the Hospitals. After the examination, the professors are required to forward to the Provincial Medical Board a report on the progress of each pupil.

Students at the Universities, or at the Amsterdam Athenæum, (having been admitted as *Candidates*, or taken their degree of *Doctor*,) may, if they choose, participate in the instruction given at the Clinical School: and, indeed, there are better opportunities for the acquirement of practical skill at the hospital of this place (*St. Peter's Gasthuis*,) than at the Athenæums, as the professors at the Clinic are also physicians at the hospital.

All the patients of the hospital are at the disposal of the professor, who may remove any of them, men or women, to the Clinic attached to the hospital, and allow them to be treated by the *Candidates*, agreeably to the method prescribed in his lectures. We do not, however, approve of the plan of choosing male subjects one year, and female the next. The professor of surgery selects subjects for his clinical instruction from among all the patients under his care, as well as from the pregnant women and those recently confined.

Besides the hospital of St. Peter's, there is another, called the *Pest-house* (Pesthuis), in many respects very superior to it, at a few minutes' walk from the city. Into this, as in the former, patients requiring medical or surgical aid are admitted; with the exception, however, of pregnant women. Lunatics and venereal subjects are also received into this hospital, which are wanting in the Clinic, to render the course of instruction complete. This hospital is under the direction of a physician and a surgeon: the former resides in the hospital, the latter in the city, where he attends to his private practice, but visits the hospital every morning.

If the beautiful *Workhouse* (Werkhuis), which is too expensive for this city, and might easily be dispensed with, were converted into an hospital, which might be done at a small expense, the two hospitals might be united into one useful establishment. By this arrangement, the *Pesthuis*, situated outside of the gate, might be made useful as a lunatic asylum, in which the unhappy citizens would be better attended, and a greater probability of their recovery be entertained. It is a disgrace to our country that we possess no general lunatic asylum. Those now existing can only be considered as private madhouses, generally defective, and established more for private advantage than general good.

THE BOTANIC GARDEN occupies a rather extensive piece of ground in the *Slantage*, a walk within the city, adorned with gardens and garden-houses. This garden has been rendered much more effective and beautiful by the late improvements, and we hope to see it still more complete. All the students who attend, or have attended the botanical lectures, have free admission to this garden, in which the medicinal plants are classified in a separate piece of ground. This garden is also distinguished by a large and rare collection of exotics.

THE ANATOMICAL THEATRE is in a very imperfect condition, and stands in general disrepute, because culprits are executed, and other public punishments, inflicted in front of it. To the want of a good anatomical theatre, we may add that of a public anatomical and physiological museum.

A few years ago, the king offered to present our city with the beautiful anatomical and physiological cabinet of Professor Reimer; but the authorities of the city declined the offer, (as we are informed,) because they had no building fit to deposit it in. This, however, we may be allowed to doubt; as, if the authorities had applied to the Provincial Medical Board, they would very probably have provided, not only a suitable place for the reception of the cabinet, but also funds for the care of so valuable a bequest.

The excellent and extensive Physiological and Pathological Museum of Professor G. Vrolik is so celebrated that we can add nothing to its praise: a detailed catalogue of its contents would be very interesting.

The Medical and Surgical Clinic is also now occupied in making a collection of pathological specimens; and, considering the shortness of the time, a very interesting collection has already been made. This school, which has been established only eleven years, reckoned, in 1836, forty-one students, exclusive of those entered a the Athenæum.

We now entertain some hope of having a new anatomical theatre, in which room might be made for the much-wanted pathological cabinet.

### II. III. DEVENTER AND FRANECKER ATHENÆUMS.

The other two Athenæums, at Deventer and Franecker, (the latter of which was formerly a university,) are of too little importance in reference to medicine to require any further notice; though the Franecker costs the country 20,000 florins a year, and the Deventer probably very little less.

The total number of students in the universities, in 1834, was as follows:—In Leyden, 761; in Utrecht, 540; in Gröningen, 296: total, 1597. The number has been steadily increasing since this period.

It must strike every reflecting reader, that the number of institutions is disproportioned to the wants of a country containing a population of only two millions and a half; for, in the first place, the expenditure of 258,000 florins (21,500*l.*) yearly for the universities alone, independently of the cost of the Athenæums and clinical schools, is too great for a small territory, already oppressed with other taxes. In the second place, the great number of institutions prevents the extension and perfection of them individually. At present, each department of a science can be superintended by only one professor, and from this will arise that want of emulation in teachers, as well as in pupils, so necessary in universities. From this circumstance must also result partial views of a subject, and blind belief in the dicta of the professor, so that the system of the teacher being known, there can be no difficulty in determining that of the pupil. The minds of the pupils would be better developed if different professors lectured on the branches of a science; the difference of opinions, particularly in medicine, would be smoothed down, and one tend to elucidate the other; excitement would be kept alive among the pupils, and a stronger desire for enquiry and reflection created; the consequence of which must be greater accuracy of judgment: besides which, the professors would endeavour to increase the number of their auditors by the improved delivery and selection of the matter of their lectures. These lectures are delivered at the universities in Latin, and at the Athenæums and clinical schools in Dutch.

## B. OF THE ORDERS AND QUALIFICATIONS OF THE MEDICAL PROFESSION.

The universities alone can confer the degree of Doctor. The individual desirous of this degree must have matriculated at the university, and have undergone the *examen philosophicum*; previously to being admitted to which, he must adduce certificates of attendance on lectures on the Greek and Latin languages, mathematics, physics, chemistry, and botany; on all which subjects he is tested at this examination. He next matriculates as a medical student, and attends the lectures on anatomy and physiology, zoology and comparative anatomy, general pathology, and pharmacy. On these points he is examined at the *examen pro gradu candidatûs*; having passed which, he receives the denomination of *Candidatus medicinæ*, a class already referred to as admissible to the clinical schools. This trial generally takes place from twelve to eighteen months after the *examen philosophicum*, and at it the bones and dried preparations are placed before the student for demonstration. He now proceeds, by attending lectures on materia medica and therapeutics, medicine, surgery, midwifery, forensic medicine, clinics and hospital practice, to qualify himself for the *examina pro gradu doctoratûs*. These are two in number: at the first, his proficiency in the above subjects is tested; at the second, he has a written question and aphorism from Hippocrates,—the former of which he has to answer, the latter to write a commentary on. This being completed to the satisfaction of the professors, he obtains the rank of *Doctorandus;* a grade analogous to the bachelor of medicine of the English universities. He may now immediately proceed to the degree of doctor, for obtaining which he has to print an inaugural dissertation, and defend it in public against the professors, or any individual who may choose to oppose him. Immediately after this he proceeds to the senate-room, takes the academic oath,* and receives his degree from one of the professors, who is termed his *Promotor*. Previously to each of these trials, he undergoes a preliminary private examination before one of the medical professors, who advises him to, or dissuades him from presenting himself for the public examination, according as he finds him possessed of the requisite qualifications or not. The examinations for M.D. are oral, (with the exceptions above mentioned,—i. e. the question and aphorism,) and in the Latin language. Practical examinations at the sick-bed are not required. The examinations of candidates for the degree of Doctor of Surgery are also held in Latin; and no operations on the dead body are required. The examination for the degree of Master or Doctor of Midwifery alone requires some proofs to be given of practical experience in that department of the art.

Few candidates take up the three degrees at the same time, as such a qualification would be of no advantage in obtaining a professorship, or other official appointment; of which latter there are almost none for medical men. Moreover, a doctor both of medicine and surgery can practise only in one capacity, under heavy penalties, and is required to make his election for practice between the two. It is remarkable that a doctor of medicine and of midwifery is not allowed to practise in both capacities, but that a doctor of surgery, being also a doctor of midwifery, may do so. As these branches cannot be separated, it would certainly be advisable to confer the degree of doctor on no candidate who could not produce testimonials of, at least, a theoretical knowledge of surgery. Government ought also to give or secure some advantages to those who should take the three degrees.

The degree of Master is conferred by the Provincial Medical Board on surgeons and on rural doctors, who are permitted to practise medicine, surgery, and midwifery; also on apothecaries, midwives, druggists, dentists, and oculists, who unfortunately flourish still in great numbers. The examinations held by the Provincial Medical Board, are not very strict. The surgeons, rural doctors, and apothecaries are required to know sufficient Latin to translate the Pharmacopœia Belgica: this forms the first part of the examination.

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* The following is a copy of this oath:—"Testor Deum omni potentem (sancte promitto) me in curandis ægris, diætam aliaque remedia, quantum ingenii viribus assequar, ex artis regulis ad ægrotantium salutem et commodum commendaturum: nec prece, nec pretio aliave de causâ pharmacum noxium cuiquam propinaturum, nec gravidæ abortum procuraturum. Audita vel visa inter curandum, nisi Reipublicæ ea efferri intersit, silentio suppressurum: in examine autem forensi ad judicem fideliter relaturum; quid actum, quid repertum sit, et de indole mali ex animi sententiâ religiose pronuntiaturum. Et in his omnibus pietati, honestati et conscientiæ integritati operam daturum. Hæc si sincere præstitero, nec sciens fefellero, felix mihi per Deum vita et ars esto. *Ita me Deus juvet!*

There is in every province one, and in large provinces two Provincial Boards, appointed by the king, consisting of doctors of medicine, surgeons, and apothecaries. Of these there are in all ten, in direct communication with the minister of the interior, receiving his directions, giving their opinion on proposed questions, and giving explanations when required. Their office is also to watch over the health of the province, to notice the epidemics and diseases that make their appearance, to enforce the regulations for medical practice, to examine the apothecaries' shops and surgical instruments in the country and towns in which there is no local Board, &c.

In towns in which more than two doctors of medicine reside, local Boards are appointed by the town administration: these are under the control of the Provincial Board, and their duty is to watch over the medical police, and examine the apothecaries' shops and the surgical instruments, and to make a yearly report to the Provincial Board on all medical occurrences that have come under their cognizance. These local Boards are, however, not authorised to punish infractions of the law, quackeries, &c., but merely to receive information of such occurrences, which are afterwards to be forwarded to the courts that have to decide on such cases. The Provincial Boards also report, every year, to the ministry whatever has occurred concerning medicine in their province, accompanied by such remarks as they may deem fit.

Doctors, who have graduated at one of the universities, are allowed to practise in any part of the kingdom; but surgeons and others, who have received their degree from the Provincial Boards, are allowed to practise only in the province in which they have been examined; and must undergo a new examination, if required, by the Board of the province in which they are desirous of practising. Dentists and oculists are excepted from this regulation, and may practise their profession in any part of the kingdom.

### MILITARY MEDICAL ESTABLISHMENT.

The military physicians receive their medical education at the Royal Hospital at Utrecht, and are admitted as pupils after they have given proofs, in a preliminary examination, that they know the Dutch, French, and the elements of the Latin languages, and have some knowledge of arithmetic, history, natural philosophy, &c. They are divided into three classes, and are maintained partly at the expense of government, and partly at their own. They are required to study four years, or until they have acquired sufficient knowledge of the sciences enumerated in the prospectus to pass the examination. Those who apply themselves diligently receive a salary; they board themselves, but are submitted to military discipline. The means of instruction are ample, and directed by teachers of the first and second class. The course of instruction is as follows:—*The first year,* they are instructed in general and particular anatomy, chemistry, languages, drawing, botany, and pharmacy. During *the second year,* instruction in the above branches is continued, with the addition of physiology, materia medica, and surgery. *The third year* is occupied with the study of general pathology and chirurgical operations; and *the fourth* with clinical instruction in medicine, surgery, &c: they may also attend lectures at the university, and must attend the Chemical and Pharmaceutical College and the Botanical Garden.

When the candidate has given proofs of sufficient knowledge of the different branches of science on which he is to be examined, he is admitted to take his examination for the rank of officer of health of the third class, and is examined on the following subjects:—theoretical and practical anatomy, physiology, medicines and their combinations, botany, the elements of chemistry, a part of pharmacy, general pathology, general treatment of the sick, the particular pathology and treatment of diseases which suddenly threaten life, systematical description of general and particular surgical diseases, and their medical treatment, bandaging, appropriate applications in cases of operations, the practice of minor operations, &c.

This examination having been satisfactorily passed, the candidate is promoted to the rank of officer of health of the third class, with a salary of 600 florins (50*l.*), which is equal to the military rank of second lieutenant; his studies are continued in the hospitals and the military corps to which he is attached, under the superintendence of the inspector-general of the medical staff.

To be promoted to the second class, which can be done only after some years' service, a new examination is necessary: this embraces the higher branches of surgical anatomy, physiology and its literature, a more extensive enquiry into simple and compound medicines, chemistry and its application, botany, antidotes as applicable in cases of poisoning; pathology, particularly with regard to military cases; surgical diseases, particularly such as have reference to soldiers; surgical instruments, operations, dissections as referring to juridical medicine. Practically, the examination comprehends a demonstration on a dead subject or a preparation; the treatment, for fourteen days, of four patients affected with acute or chronic diseases, the drawing up the *Historia Morbi*, and the performance of one or more operations on a dead or living subject. The rank of first lieutenant, and a pay of 900 florins (75*l.*), are given to officers of this class.

To be admitted to the first class, and become surgeon-major, a third examination, embracing a greater variety of subjects, must be passed; and only after a service of some years. This examination includes anatomical demonstration relating to surgical practice; the treatment of eight patients during a period of four weeks, with an analysis of the different methods of treatment. Officers of this class hold the rank of captain, and receive a salary of 1600 florins (133*l.*)

Besides these, there is one still higher rank to be obtained; namely, that of first officer of health, which gives the rank of staff-officer.

The five oldest surgeons-major receive, after they have served for ten years, a salary of 1800 florins (150*l.*); after twenty years' service, they receive 2000 florins (166*l.*) a year, and have then the same privileges as physicians and surgeons to practise in towns.

The apothecaries of the army are also divided into three classes, and at every promotion must undergo a new examination, on natural history, botany, chemistry, pharmacy, &c. The subjects on which the candidate is to be examined are named by the inspector-general, who has the privilege of being present at the examination: this is held at the Royal Hospital at Utrecht, and by the resident professors. At the head of this military medical administration is the inspector-general of the land and sea medical service, who, at this time, is M. Bernard.

## MEDICAL LITERATURE.*

Owing to its contracted territory, and its intimate connexion with other countries, of which the languages are more generally studied and more extensively known, and from which, particularly Germany and France, books are continually imported, the medical literature of Holland is at present rather circumscribed. Still it can boast of many distinguished authors, whose works are well known throughout Europe; and, among others, the distinguished friend to whose kindness we are indebted for the foregoing sketch.† Several of these works are noticed in our Lists of Books received for Review, and we have now before us a catalogue of the principal ones which have appeared in Holland since 1834: of these, twenty in number, twelve are in the Dutch language, and eight in Latin, and they appeared at the following places: at Amsterdam, 12; Leyden, 3; Gröningen, 2; Deventer, 1; Franecker, 1; Tiel, 1. Among the publications of the present year we observe, in Dutch, one on Percussion and Auscultation, and another on Lithotripsy. The following is a list of the journals on medicine and the allied sciences at present published in Holland, which, it will be observed, amount precisely to the same number as are published in Great Britain.

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* By one of the Editors.

† We regret to learn, from the public prints, that the respected author of this Report is recently dead.—For the principal part of the account of Leyden in this Report, and also for the mode of examination for medical degrees, we are indebted to our friend, Dr. William Munk, a recent graduate of that learned university.

1. Practisch Tydschrift voor de geneeskunde in al haren omvang, &c. &c. Verzameld door A. Moll en C. van Eldik. (Medicine.)
2. Tydschrift voor natuurlyke Geschiedenis en Physiologie. Uitgegeven door J. v. d. Hoeven en W. H. de Vriese. (Natural History and Physiology.)
3. Natuur en Scheikundig Archief. Uitgegeven door G. J. Mulder en W. Wenckebach. (Natural History and Chemistry.)
4. Nieuwe Schei-, Artsenymeng- en natuurkundige Bibliotheek. Verzameld door B. Meylink. (Chemistry, Pharmacy, &c.)
5. Hippocrates. Magazyn toegewyd aan den geheelen omvang van de geneeskunde, &c. (Medicine.)
6. Aesculap. Een Vaderlandsch Tydschrift voor theor. en pract. bydingen in het gebied der genees-, Heel-, en verloskundige wetenschappen. (Medicine, &c.)
7. Tydschrift voor genees-, Heel-, Verlos- en Scheikundige Wetenschappen. (Medicine, &c.)

## SKETCH OF THE PRESENT STATE OF MEDICINE IN PORTUGAL.

THE medical institutions of Portugal, for the last two centuries, have participated in the general decline of that unhappy country, and only those students who have visited foreign schools have kept pace with the progress of science of other nations. But now, under the present dynasty, the medical practitioners of Portugal exert their best efforts to acquire that comprehensive knowledge which is necessary for the adequate exercise of their profession; a change which has been happily effected by the more general diffusion of education, that has sprung from the enlarged knowledge and liberal principles of foreign institutions, which the banished patriots brought with them from exile.

COIMBRA is the only university in Portugal where medicine is taught: it confers degrees in medicine and in surgery. It was founded in 1291, by King Dennis, and had at that time fifty professors. At Coimbra, as elsewhere, theoretical principles were those chiefly insisted on in medicine; and, even when a better system was adopted by other universities, Coimbra, in character with the inhabitants of the country, failed in emulating the improvements, and consequently fell into disrepute. There were also other causes, more directly lessening its importance, arising from the scanty facilities presented in the town for the study of anatomy, or any of the practical departments of medicine, and chiefly from the interference, both of professors and students, in the political disturbances of the day. These causes sufficiently explain its decline; and the recent proposal to transfer the medical school to Lisbon, if carried into effect, will prevent its ever reacquiring its former celebrity, at least as a school of medicine.

Besides the university of Coimbra, there are schools of surgery at Lisbon and at Oporto. These schools were established in 1824, by Don John VI.; but only since 1825 has any regular course of instruction been attempted. That in Lisbon is connected with the hospital of St. Joseph, and here the school of anatomy, and also of surgery, is excellent. Many eminent men have already commenced and finished their education under its teachers. The supply of subjects for anatomical and surgical purposes is most ample; and foreigners meet with every encouragement in prosecuting this important branch of study.

Before the establishment of the schools of Lisbon and Oporto, the great bulk of medical men in Portugal were very ignorant, knowing little of either anatomy or practical surgery: indeed, many surgeons were in practice who had never studied at any school. These men practised on the attestation of other surgeons, after an examination which was not conducted before professors or teachers, but before surgeons chosen in all parts of the country, as suited convenience, by the chief surgeon of the kingdom. Matters went even further than this; surgery being sometimes practised by men who had never received any authority at all for doing so: and of this class there are still some examples to be found.

Much is still wanting to perfect medical education in Portugal; and, indeed, the only plan of sustaining the stimulus now existing among the Portuguese medical men is by carrying into effect the design, which has been for some time entertained, of establishing a regular college at Lisbon, where every facility may be commanded for the furtherance of study.

The following is a list of the hospitals at present established in Lisbon:

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|---|---|
| Hospital of St. Joseph | 1000 beds. |
| Marine Hospital | 150 .. |
| Military Hospital, at the Castle | 300 .. |
| at Belem | 100 .. |
| Hospital of St. Lazarus | 30 .. |
| Victoria (for aged Women) | 12 .. |

In addition to the foregoing, it must be understood that each religious house has an hospital attached to it.

Of the above, that of St. Joseph is the only civil hospital. The number of patients in this establishment, it is obvious, gives ample scope for clinical teaching and anatomical research; but the city of Lisbon, if under proper regulations, might present a field far more extensive than exists at present for these studies; for it is notorious that the demand for admission into the hospitals is greater than can be complied with: even under present circumstances, the opportunities are great. The number of patients admitted into St. Joseph's, during the spring and summer quarters of 1835, (six months,) were 14,066; of which number, 2,077 died: and, during the autumnal quarter of 1836, we perceive, from a report in the Lisbon Medical Journal, that the numbers were—admitted, 3,725; cured, 3,281; died, 444.

The arrangements of the Foundling Hospital are much complained of; and when we consider that on such institutions the population of the city depends in a considerable degree, we are not surprised to find that this subject is much talked of at present in Portugal. There is no well-regulated Lunatic Asylum; and the classification of patients, if understood at all, is not practised in Portugal.

In Portugal there is no prevailing system of medicine which can be termed national: the mode of study, the favorite theories, and the practice, are, in general, more similar to the French than those of any other. The doctrines of Stahl, Hoffmann, Boerhaave, and Cullen, have alike had their day in Portugal, as in other European countries. The celebrity of Broussais, and the apparent clearness and simplicity of his doctrines, soon led the great body of Portuguese medical men to become his followers. They still continue to be so; but there were, from the first, a good many physicians, who, while they adopted many of his views, yielded to none of his extravagance, and followed, and still follow, his tenets with a reasonable and prudent moderation. This will appear from the following extract from a recent essay by Dr. F. A. Barral; which will serve, at the same time, to give a general outline of the ordinary course of practice in Portugal.

"We consider fevers as local irritations of different organs; we devote much attention to the state of the stomach and intestines previous to the administration of medicines; and we do not neglect the prudent use of emetics and purgatives in functional disorders and subacute irritations of organs; the advantages of which have been proved by time and experience. We employ bloodletting and the antiphlogistic treatment with sufficient frequency, but we are not reduced to this mode of treatment only—despising all others which physicians, in all times, have represented as beneficial. So also, in obscure cases, we employ substances of less evident power, but which, at various periods, have had ascribed to them many and different properties by practical physicians. Camphor, calomel, tartar emetic, and all the antimonial preparations, have been much employed, and with good results, in inflammatory affections, after the most violent symptoms have been combated by more direct antiphlogistic and revulsive means; when the disease seems to resist these means, and when the digestive apparatus permits their exhibition. The use of tartar emetic in acute pneumonia, in repeated minute doses, was known and approved of in Portugal, long before the period at which it was introduced first into Italy, and more recently into France. The homœopathic doctrine has been known in Portugal for the last twelve years, but has not been followed with much enthusiasm."

The employment of the bark of the pomegranate root in tænia is claimed as a national discovery, although, we believe, it has been employed for ages in India for this purpose.

Portugal abounds in mineral waters, chiefly chalybeate and sulphureous; but as yet we have no accurate scientific account of them. The "Caldas da Rainha," the most celebrated, are near Lisbon, and are sulphureous.

In Surgery, particularly operative surgery, the Portuguese are very well advanced; the opportunities during the late French war, and the recent civil contest, gave them ample practical experience, which has not been thrown away. Their military surgical establishments are, however, still at a low ebb; and this inefficient state was much felt, and much to be deplored, during the war.

A striking proof of the impulse which has, of late years, been given to medicine in Portugal is the greatly increased activity of the medical press. In the summer of 1835, two medical journals made their appearance, and have continued to be regularly published since that time. They both originate from Medical Societies, and they are both very creditable productions. *The Society of the Medical Sciences of Lisbon* (Sociedade das Sciencias Medicas de Lisboa,) was instituted in May 1835, and consists of physicians, surgeons, and apothecaries, foreign as well as native. *The Pharmaceutical Society of Lisbon* (Sociedade Pharmaceutica de Lisboa,) was instituted in July of the same year, and consists also of members from all the classes of the profession; although its views are more especially directed to objects of pharmacy, natural history, and therapeutics. The *Jornal da Sociedade das Sciencias Medicas de Lisboa* appears monthly: it is of the octavo size, and each number contains four sheets of letterpress. The contents are—copious reports from the Lisbon hospitals, an account of the business transacted at the meetings of the Society whence it emanates, original papers of considerable interest, extracts from foreign journals, and notices of foreign publications. In our last Number we have given some extracts from this journal, and hope to be indebted to its pages for matter still more interesting.

The *Jornal da Sociedade Pharmaceutica de Lisboa* is conducted on the same general plan, but contains much less information on subjects of general interest to the practitioner. It seems, however, well conducted, and indicates an active state of the Association from which it springs.

Remembering, as we do, what Lisbon was some twenty-five years ago,—a sink of filth, ignorance, sloth, and superstition, and the medical profession partaking largely of these attributes of the general population,—we feel convinced, on the mere evidence furnished by these journals, that no country has, within a short period, made more rapid strides in real civilization than Portugal. In these journals we discover no indication that Portugal yet possesses an original medical literature; nor was this to be expected. Such a literature is necessarily a plant of slow growth, and requires a civilization of some duration. We find, however, sufficient indications in them of a vigilant observation of what is passing abroad, and of an intellectual exertion at home, which will in time produce indigenous fruit. The physiological system of Broussais, as we have already remarked, is manifestly the reigning doctrine in this country: it mingles in almost all the reasonings of their writers, and imparts a tinge to the nomenclature of their reports. We observe, however, that some of the original communications in the journals display a strong spirit of independent observation, and that the writers (as stated by Senor Barral) rather cull from the doctrines of Broussais what they deem to be valuable than fulfil implicitly all his mandates.*

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* For the above sketch we are principally indebted to the kindness of Dr. P. S. FRASER.—EDS.

## THE PROVINCIAL MEDICAL AND SURGICAL ASSOCIATION.

The steady progress of the affairs of this Association, from the period of its institution, six years ago, to the present time, serves most undeniably to evince that its founders made a correct estimate of the existing desire among the cultivators of medical science residing in the English provinces to combine together, so as to make their labours more instrumental than they had formerly been in the advancement of the healing art. Every anniversary has shown a great increase of members; and the influence which has arisen from this additional strength has been directed to the accomplishment of the important objects which were, at the commencement of the undertaking, specified as most worthy of regard: consequently, there can be little doubt that each returning year will afford fresh proofs of the advantages that must result from the continuance of an Association which has in view the promotion of harmony and good feeling among its members, and the collection and diffusion of facts which may serve to render medical science more signally beneficial to mankind.

The members now amount to at least eleven hundred, and the number is progressively increasing. The happy project of establishing *District Branches* has been a fruitful source of the Association's prosperity, and promises to extend its benefits, in a very short time, over the whole kingdom. In addition to the Branches formerly noticed, two new ones have been formed during the last and present year; viz. one at Newton, in Lancashire, called the *Newton Branch*, comprehending members residing in Manchester, Liverpool, Warrington, and several other towns in the north of England; the other at Shrewsbury, called the *Shropshire and North Wales Branch*. These branches, in imitation of the parent Society, hold local and annual meetings for the discussion of medical subjects, reading reports on the progress of medicine and surgery, and watching over and promoting the interests of the profession generally. The place of the annual meetings of the Branch-Societies, like that of their parent, is changed on every occasion, for the convenience of the members. Thus, the *Southern Branch* held its general meeting last year at Winchester, this year at Salisbury, and it will hold it next year at Portsmouth. The meeting at Salisbury took place on the 14th of last month, and was attended by from sixty to eighty members. Dr. Fowler was president. Much valuable matter was brought forward, which led to many interesting discussions. The members afterwards dined together. On this occasion, a prize of twenty guineas was announced for the best essay on Vaccination, the gift of one of the members, Mr. Newnham, of Farnham. The essays are to be transmitted to the secretary three months before the annual meeting in June, 1840. Another prize, of fifty pounds, was offered last year, at the Cheltenham meeting of the Parent Association, by Dr. Thackeray, of Chester, for the best essay having for its object the investigation of the sources of the common Continued Fevers of Great Britain and Ireland, and the ascertaining of the circumstances which favour the diffusion of these diseases, and also those circumstances which may have a tendency to render them communicable from one person to another. The prize, like Mr. Newnham's, will be open to the competition of the members of every accredited school for medicine and surgery in the united kingdom, and the essays are to be sent to the secretaries of the Association on or before the 1st of January, 1840.

The annual meeting of the Association takes place this year at Bath, on the 18th and 19th of this month (July), under the presidency of Dr. Barlow. A very large reunion of members is expected; and it is understood that many topics of great interest will be discussed; among others, the present state of vaccination, and the best means of putting a stop to the lamentable ravages of small-pox. The Retrospective Address will be delivered by Dr. Malden, of Worcester.

# OBITUARY.

## THOMAS BLIZARD, ESQ.

On the 7th of May last, at his house in Cumberland terrace, Regent's Park, aged sixty-six. Mr. Blizard commenced his professional life by being apprenticed at Surgeon's Hall, in 1789, and he obtained the greater portion of his education at the London Hospital, to which his cousin, Sir W. Blizard, was attached. Having received his diploma, he was elected assistant-surgeon to that hospital in 1795, and he at once commenced the system of teaching anatomy in its school. In 1796 he was appointed professor of anatomy to the corporation of surgeons, and delivered lectures in that capacity for three years. In 1797 he became surgeon to the London Hospital. He now resigned his anatomical lectureship; for his time was already fully occupied in the duties of the hospital and his increasing private practice. To these he applied himself with the most unremitting zeal, daily learning in the one the means, and earning in the other the reward, of bestowing benefits on both. From this time his practice increased every year; and, in 1816, finding it occupy every hour that he could, consistently with health and comfort, bestow upon it, he resigned the surgeoncy to the London Hospital. In the same year he was chosen one of the council of the College of Surgeons; and in the following, when his practice in the preceding six months had produced him a larger sum than is recorded of any medical practitioner, he resigned every professional occupation, and retired to the continent. The cause of this sudden retirement was the bad health of Mrs. Blizard, to the care and solace of whom the whole of his future life was devoted, with a tenderness and vigilance almost unexampled. In 1837, death at last deprived him of his only care, and broke almost the last tie which had held him to the earth. But this tie, painful as it had been, seemed necessary to his life. From the time of Mrs. Blizard's decease, one might see an evident alteration in his manner and appearance: he was no longer anxious to live; and, when his last illness seized him, he rapidly, and as it were involuntarily, succumbed to it.

Mr. Blizard was equally distinguished as a scientific and practical surgeon, and as a member of society. One who was long a pupil at the London Hospital says, that the opinion which he formed of him from observing his practice was, that he saw further, saw clearer, and acted more energetically than any surgeon he ever knew. His surgery was indeed purely practical: his anatomical knowledge was so exact, that no anticipated difficulty of manipulation deterred him from an operation when he deemed it necessary; nor did any unexpected accident embarrass him in its performance. Nay, even after twenty years of retirement, his anatomy was sitll good and clear; as easily producible as ever in discussing a case, or examining a preparation; no rust had accumulated on its polished and well-tempered, though long-neglected surface. The excellence of his lithotomy, which he always performed with the long and slender-beaked knife which still bears his name, has always been enthusiastically described by his old pupils: yet he was not fond of operating, and so little anxious to display his own powers, that he was always as ready to assist others in their difficulties as to take the chief part himself. Mild, and even affectionate, as his manners were towards his patients, and in the extensive circle of his friends, his spirit was truly indomitable when he was opposed in the pursuit of what he deemed to be just and honorable. His success was as little the result of servility as of private interest; he fairly and honestly worked his own way, and often against the tide; repelled by no obstacle, disturbed by no fear, retarded by no discouragement or indolence; entirely by his own industry he gained the knowledge which first brought him into public notice; and, by the unremitted exertion with which he pursued every advantage when once attained, he had earned by his profession, and at the age of forty-six, an ample fortune.*

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* Med. Gazette, June 9, 1838.

### PROFESSOR FERGUS.

ON the 3d April, in London, in the twenty-eighth year of his age. Dr. Fergus was professor of forensic medicine in King's College, London; and, although only recently elected to this chair, he showed himself possessed of talents that would have soon raised him to eminence as a teacher. He was greatly respected in private life. Dr. Fergus was born in Fife, and was the son of a minister of the church of Scotland.

### DR. EDWARD HARRISON.

ON the 6th May, at Marlborough, while on his way to visit a patient. Dr. Harrison was born in 1766, and was consequently in his seventy-second year when he died. He was descended from an ancient family in Lancashire, where he was born. He studied at Edinburgh, where he took the degree of M.D. in 1784; his thesis being "De Opio." After graduation, he prosecuted his studies both in London and Paris, and finally settled at Horncastle, in Lincolnshire, where he practised thirty years. While here, he made himself conspicuous by strenuous efforts at medical reform, which originated in the best motives, but were not carried into effect with much discretion. He, however, left behind him more permanent memorials of his activity and benevolence in the Horncastle Dispensary, the Lincolnshire Benevolent Medical Society, and the Horncastle General Book-Club; of all which he was the founder. In 1817 he came to London, and from that period devoted himself almost exclusively to the treatment of spinal affections; on which he published, in 1827, a work entitled "Pathological and Practical Observations on Spinal Diseases." His views were peculiar, and not generally sanctioned by pathologists; and his practice was obnoxious to the charge of attending too much to the local deformity, and too little to the constitutional debility in which this originated: the consequence was, that many of his patients who became straight, under his manipulations and prescribed recumbency, soon relapsed into their original crookedness, on taking their accustomed exercise. Dr. Harrison died rich, and bequeathed considerable sums to various charities.

### RASORI.

GIOVANNI RASORI was born at Parma, in 1766. After an enlarged and varied education, both at Italy and in foreign countries, including England, where he remained a considerable time, he was appointed professor of medicine, first in the university of Pavia, and afterwards at Pisa. He went from this to Milan, where, in the year 1799, he published his first work, "Analisi del preteso genio d'Ippocrate." He was in Genoa during the siege of that city; and, in 1801, published his celebrated account of the fever which prevailed there, and in the treatment of which he shewed so much zeal and obtained so much success. This is his greatest work, and that on which his fame chiefly rests, as it was in this that he first announced the new practice of giving emetic tartar in large doses, which has since become so famous. It is entitled "Storia della Febbre epidemica di Genova, degli anni 1799 e 1800." After the conquest of Italy by Napoleon, he returned to Milan, and soon acquired great reputation, both as a practitioner and clinical teacher. Rasori continued to enjoy great reputation and consideration at Milan during the reign of Napoleon; but, upon the return of Lombardy to the Austrian dominion, he was dispossessed of his appointments, and imprisoned for several years. On being restored to liberty, he resumed his practice and his clinical instruction at Milan, where he died in April, 1837. In the earlier part of his life he translated into Italian Brown's "Elements of Medicine" and Darwin's "Zoonomia." In 1830, he published two volumes on clinical medicine, under the title "Opuscoli di Medicina Clinica;" and, at the time of his death, he was engaged in printing what he regarded as his greatest work, and in the preparation of which (he tells us in his preface,) he had been engaged the greater part of his life. This has since been published in two volumes, under the title "Teoria della Flogosi," of which we hope to give an account in an early Number.

It is well known that it is to Rasori that we are indebted for the introduction into general practice of emetic tartar in large doses, as a remedy in febrile and inflammatory diseases, although it had been previously employed, by Dr. Marryat in this country, in similar doses; and we are of opinion that his name will be handed down to posterity, more by this circumstance than by his writings. Rasori was greatly esteemed at Milan, where a statue is about to be erected to his memory at the public expense.

### BARON DESGENETTES.

**RENÉ DUFRICHE DESGENETTES** was born at Alençon, in 1762. He was descended from an old family, whose members have for several centuries illustrated the magistracy and the bar of that city. His earliest instruction was received at Alençon, but he was soon transferred to Paris, where he studied at the College of St. Barbe. At the age of eighteen, he made choice of the medical profession; and, after having attended the principal schools of Paris, he visited those of England and Italy. He has recorded the remarkable events of this period of his life in his "Souvenirs," a book which was the production of his latter years, and which shews the extent and variety of information which he had acquired by reading, observation, and the intercourse which he had enjoyed with the first men of his time. Returning to France in 1789, he received his degree of doctor at Montpellier, where he enjoyed the friendship of Fouquet and Barthez. He wrote his thesis on the Lymphatic System; a subject which, being then entirely new, attracted universal attention; and he demonstrated the anatomy of the absorbents from drawings, which he had received from his friend Mascagni. In 1793, he was attached professionally to the army of Italy, and from that time he continued to follow the fortunes of Napoleon to the close of his career. His celebrated experiments on the contagious nature of the Plague are well known; but it was not less his great professional talents than his unbending integrity which recommended him to the Emperor, who could appreciate his upright and elevated mind, which never stooped to low flattery or cowardly complaisance. He directed his attention particularly to the hygienic branch of military medicine, and it was this portion of the science which he professed at the School of Medicine in Paris. Political events deprived him of his professorship, which was restored to him after the revolution of July; but the caprices of fortune could not take from him that just consideration which was his due, nor that peace of mind and self-respect which his conduct had procured for him.[*]

### DR. DECKMANN, OF KIEL.

**C. G. DECKMANN** was born at Rendsburg, April 8, 1798, of parents in the lower class of society. He early distinguished himself at school and at the university. He commenced his medical studies at Kiel, and prosecuted them still further at Copenhagen. He returned from Copenhagen to Kiel, and, in March, 1824, took his degree of doctor, publishing a thesis under the following title: "Notæ quædam chemici præcipuè argumenti in aquas ophthalmicas." He then established himself in practice in the city of Schleswig, where he enjoyed good practice and the consideration of all his brethren, until 1829, when he was appointed extraordinary professor of anatomy and surgery in the university of Kiel. He published, in 1830, an interesting prospectus to his course of lectures, under the following title: "Studium Anatomiæ et Physiologiæ omnibus singularum artium cultoribus probat et ad prælectiones anthropologicas invitat C. G. Deckmann." Dr. Fischer having died in 1832, Deckmann was nominated ordinary professor, and took charge of the surgical clinic in the Frederick's Hospital. Of this clinic he published interesting reports in "Pfaff's Mittheilungen" for 1833. While in the flower of his age, and in high and increasing reputation, he was attacked with hæmoptysis and phthisis, which carried him off, after long suffering, on the 25th February, 1837, in the thirty-ninth year of his age. Professor Deckmann was an excellent surgeon and teacher, and most faithful and zealous in the performance of all his duties.[*]

[*] Eloge par M. Pariset. Bulletin de l'Academie, 28 Feb. 1837.

[*] From a notice, by Dr. Pfaff, in Sachs's Almanach, for 1838.

# BOOKS RECEIVED FOR REVIEW.

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ENGLISH.

1. A short Sketch of Animal Magnetism: intended to direct attention to the Propriety of practically examining the Question. By a Physician.—London, 1838. 8vo. pp. 63.

2. A Practical Treatise on Fractures. Illustrated by sixty Woodcuts. By E. F. Lonsdale, Surgeon, Demonstrator of Anatomy at the Middlesex Hospital School of Medicine.—London, 1838. 8vo. pp. 536. 16s.

3. Velpeau's Anatomy of Regions. Translated from the French, by Henry Hancock, Lecturer on Anatomy at the Westminster Hospital School of Medicine, &c.—London, 1838. 8vo. pp. 565. 16s.

4. Libamenta Praxeos Medicæ; or, a Manual of the Practice of Medicine, for the use of Students and junior Practitioners. By D. Spillan, M.D.—London, 1838. 48mo. pp. 192. 2s.

5. A Collection of Medical Formulæ, selected from the Writings of the most eminent Physicians. By D. Spillan, M.D.—London, 1838. 48mo. pp. 111. 1s.

6. The Final Report of the Committee of the Philadelphia Medical Society on the Construction of Instruments, and their Mode of Action in the radical Cure of Hernia, &c. By Heber Chase, M.D.—Philadelphia, 1837. 8vo. pp. 243.

7. The Philadelphia Practice of Midwifery. By C. D. Meigs, M.D., Lecturer on Midwifery, &c.—Philadelphia, 1838. 8vo. pp. 370.

8. Guy's Hospital Reports, No. VI. April, 1838. 8vo. pp. 267. 6s.

9. Manual of British Botany; in which the Orders and Genera are arranged and described according to the Natural System of De Candolle, &c. By D. C. Macreight, M.D. &c.—London, 1837. 12mo. pp. 296. 7s. 6d.

10. Remarks on a Case of Suicide published by Dr. P. D. Handyside. By J. R. Cormack, M.D.—Edinburgh, 1838. 8vo. pp. 16.

11. On the Differences of the Laws regulating Vital and Physical Phenomena. By W. B. Carpenter, M.R.C.S. &c. (From the Edinburgh Philosophical Journal.)—Edinburgh, 1838. 8vo. pp. 27.

12. Second Report of the New Lying-in Hospital, Dublin. By T. E. Beatty, M.D. Master of the Hospital.—Dublin, 1837. 8vo. pp. 43.

13. General View of the Proximate Causes of Diseases, organic and dynamic. By William Thomson, M.D. (From the Encyclopædia Britannica.) — Edinburgh, 1838. 4to. pp. 20.

14. The Responsibilities of Medical Students: a Sermon, preached in the Chapel of Guy's Hospital. By the Rev. F. Maurice, A.M.—London, 1838. 8vo. pp. 42.

15. The Medical Portrait Gallery, with Biographical Memoirs of the most celebrated Physicians, Surgeons, &c. who have contributed to the advancement of Medical Science. By T. J. Pettigrew, F.R.S. &c. —London, 1838. Nos. I. to III. Royal 8vo. 3s. each.

16. Experiments and Observations on the Gastric Juice, and the Physiology of Digestion. By W. Beaumont, M.D., Surgeon in the United States' Army. Reprinted, with Notes, by Andrew Combe, M.D. &c.—Edinburgh, 1838. 8vo. pp. 319. 7s.

17. Practical Observations on the Preservation of Health and the Prevention of Diseases; comprising the Author's Experience on the Disorders of Childhood and Old Age, on Scrofula, and on the Efficacy of Cathartic Medicines. By Sir Anthony Carlisle, F.R.S., President of the Royal College of Surgeons, &c.—London, 1838. 8vo. pp. 154. 8s.

18. A Series of Anatomical Sketches and Diagrams, with Descriptions and References. By T. Wormald, Assistant Surgeon, and A. M. M'Whinnie, Teacher of Practical Anatomy at St. Bartholomew's Hospital.—London, 1838. 4to. pp. 16; Five Plates. 4s.

19. On the Evidence of the occasional Contagious Propagation of Malignant Cholera. By J. Y. Simpson, M.D.—Edinburgh, 1838. 8vo. pp. 54. (From the Edinburgh Journal.)

20. A Dissertation on the Causes and Effects of Disease, considered in reference to the Moral Constitution of Man. By Henry Clark Barlow, M.D.—Edinburgh, 1837. 8vo. pp. 79. 3s.

21. On the Improvement of Medicine. The Oration delivered before the Medical Society of London, at their Sixty-fifth Anniversary, March 8th, 1838. By Theoph. Thompson, M.D.—London, 1838. 8vo. pp. 23.

22. Buxton and its Waters: an Analytical Account of their Medical Properties and general Effects. By W. H. Robertson, M.D.—London, 1838. 12mo. pp. 147. 2s. 6d.

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14. Forelæsninger over den legale Medicin af Dr. MICHAEL SKJELDERUP, Professor i Lægevidenskaben ved det Kongl. Frederiks Universitat, &c.— Christiania, 1838. 8vo. pp. 212.

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THE
BRITISH AND FOREIGN
MEDICAL REVIEW,

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# PART FIRST.

## Analytical and Critical Reviews.

### ART. I.

1. *Influence de l'Anatomie Pathologique sur la Médecine, depuis Morgagni jusqu'à nos Jours.* Par RISUENO D'AMADOR, Professeur à la Faculté de Médecine de Montpellier, &c. &c. 4to. pp. 181. (*Memoires de l'Academie de Médecine*, tome vi.—*Paris*, 1837.)
*Influence of Pathological Anatomy upon Medicine, from the time of Morgagni to the present day.* By RISUENO D'AMADOR, Professor, &c. &c.

2. *Mémoire en Réponse à cette Question: " Quelle a été l'Influence de l'Anatomie Pathologique sur la Médecine, depuis Morgagni jusqu'à nos Jours?"* Par CONSTANT SAUCEROTTE, Docteur en Médecine, &c. &c.—4to. pp. 111. (*Ib.*)
*Memoir in Reply to the Question: " What has been the Influence of Pathological Anatomy upon Medicine, from the time of Morgagni to the present Day?"* By CONSTANT SAUCEROTTE, M.D. &c.

THE two dissertations the titles of which are above transcribed, were submitted to the Royal Academy of Medicine in the *Concours* of 1836 for the prize founded by the late Baron Portal, for the encouragement of pathological anatomy. One of these, (that written by M. Saucerotte,) received the approbation of the academy, while the other obtained the prize. This last is a production of very considerable merit. The question proposed by the academy, was to determine the influence of pathological anatomy upon the science of medicine from the time of Morgagni to the present day. M. D'Amador in replying to this question, divides his subject into two parts, the first of which is devoted to the consideration of certain laws laid down as those under which he conceives the development of morbid anatomy to have proceeded; the second, to the solution of what he terms the three anatomico-pathological problems in their application to the science of disease. These problems respectively relate to the organic change, to the symptoms, and to the causes, as will hereafter be seen. From the mere enunciation of the primary divisions of this treatise, we are at once led to expect an attempt at something like precision in the general conduct of the work, an expectation which a more extended examination of its pages confirms; and, indeed, a closer attention to the principles of the inductive philosophy will be found throughout than is customary in writings devoted to the elucidation of medical science.

It has ever been found advantageous to take, from time to time, a retrospect of the progress made in the acquisition of knowledge,—to pause, as it were, on some eminence gained, and to contemplate at once from this vantage-ground the steps successively attained, and the vast field which lies yet untrodden before us. In the darker periods of the science of medicine, the several theories which successively sprung up, and, for a time, held possession of the minds of those who were but too readily disposed to follow the leading of a great name, seem to have conduced to this important end; while the more modern eras have been similarly marked by the researches of observers devoting their time and talents, in a more cautious spirit of observation, to the elucidation of parts of the great whole, leaving it to some future master-mind, which may prove the Cuvier or Jussieu of our science, to combine all these into a complete and harmonious system.

The leading principle, however, "*l'idée mère,*" that which gravitation is to astronomy, or the doctrines of proportion to chemistry,—yet remains to be developed in the medical sciences; and, until the *special* laws of life and organization shall have been ascertained, and the combination of them into more *general* laws effected, we must still remain in the present unsatisfactory state of hesitation and doubt, with respect to numerous questions of vital import to the true comprehension of the phenomena of health and disease. The essay of M. D'Amador contains materials which, considered in this point of view, are applicable to the illustration, not only of pathological anatomy, but also of the science of medicine taken as a whole.

The first part of the essay, is devoted to the elucidation of what the author terms *the Laws of Development of Pathological Anatomy*. In the attempt to establish these laws, M. D'Amador traces the progress of pathology in each of the leading systems which have at various times obtained the ascendant. In the earliest periods to which our records extend, when, amidst much uncertainty, some important truths had already been appreciated, the germ of these several systems may be detected; but with the gradual increase of knowledge came also the attempts at explanation of all known phenomena upon some general and exclusive principle, and hence sprung up the successive and rival codes of the vital, humoral, and organic pathologies, each, in its turn, professing to trace every aberration from the physiological state to an alteration in the solids, in the fluids, or in the vital forces.

The progressive advance of these systems, however, does not appear to have been made in accordance with any general rule, as, from the more gross to the more refined material agent, and thence again to the more subtle agencies of the vital system; but seems rather to have proceeded in a series of oscillatory gradations, to have vacillated, as it were, between the extremes, each successive period being enriched by the acquirements of that which had preceded it. It is thus that the doctrines of the schools, having been in succession exclusively vital, and exclusively humoral, became at a subsequent period exclusively organic, until at length it has been again acknowledged that the fluids must also challenge for themselves a due consideration in a true and extended system of pathology; and, more recently, that the vital influence—the influence arising from the presence of the principle of life, and under the regulation of the laws which govern the action of that principle, must pervade the whole. There is this especial difference, however, in the code of pathology professed by the modern, or, as it has been termed, the eclectic school,—that, at the same time that it recognizes the fluids and vital powers as necessarily bearing an important part in the theory of disease, the alterations in the tissues and organs constituting the solid parts of the body are not overlooked nor disregarded.

The first law of development proposed by M. D'Amador (although, according to our view of the matter, the proposition contains the statement of a *fact* not a *law*;) is, that *Pathological Anatomy, as well as the other Elements of Medical Science, already existed in the germ at the commencement of Medicine.* In illustration of this principle, which will scarcely be questioned if we are to receive the term medicine as intended to apply to the science, the author instances the description of the development and progress of tubercles by Hippocrates, and the circumstance that Aretæus was aware that paralysis occurs on the side opposite to that on which the brain is affected; and he might have referred to the still earlier period, in which the sacred historian so admirably describes the distinctive characters of the Jewish leprosy and some allied forms of cutaneous disease. But the epoch of pathological anatomy can scarcely be said to have really commenced until the time of Morgagni, and, in tracing the history of medicine, we can scarcely date its influence much anterior to that period. During the long period in which the Galenical doctrines of the four humours held undisputed sway, medicine, as a science, remained absolutely stationary, and it was not until the examination of diseased structure was generally had recourse to that it can be said to have made any real progress. At first, however, it was only the more extraordinary cases which were thought worthy of investigation or description, and with respect to the alterations produced by diseases of frequent occurrence, it was, as M. Saucerotte remarks, much too common an affair to require looking into. Hence we find Sylvius, Bartholin, Renealmus, Tulpius, Panarolus, and many other writers of that period detailing cases of disease and morbid changes, bearing about the same relation to the descriptions of the modern pathology as the romances of the troubadours, and the fairy tales of the nursery, do to a correct historical or biographical memoir. It is to Morgagni that we especially owe the impulse to a more rational employment of this mode of studying disease, and his immortal work *De Sedibus et Causis Morborum* will ever remain a practical illustration of the important truth, that the more frequent a disease, the more necessary it is that its phenomena should be carefully investigated and thoroughly understood. Without enquiry into those alterations of structure which take place during the course of morbid actions, or are otherwise connected with them, it is impossible to comprehend the symptoms by which they are indicated, and the mode of operation of the causes to which they owe their rise. In reference, therefore, to the organic changes which takes place internally, and to the rationale of disease consequent thereon, pathological anatomy may be said, in the somewhat obscure terms of the second law, to represent in the history of medicine "*le developpement interne, reflechi, et logique de la science.*"

The third law proposed by M. D'Amador requires a rather more extended consideration. It professes, as we understand it, that *pathological anatomy in the course of its own progress has given a character ("a revêtu les formes") to the general systems of medicine which have successively prevailed.* There seems, however, to be some ambiguity in the enunciation of this law, since many of the observations which follow —to which, we may remark, the facts of the science themselves accord— imply just the reverse, viz. that pathological anatomy has in the course of its progress assumed to itself the characters of the systems which have successively prevailed.

At the time when surgical or external diseases became the especial objects of attention, the chief application of pathological anatomy was directed towards the exposition of changes taking place in the organization of the exterior organs or parts of the body; and subsequently when extended to the elucidation of internal diseases, the same spirit of adaptation to existing views would seem to have prevailed. Accordingly, we find Bonetus and the writers of that period, who were occupied rather in the endeavour to account for the fatal termination of a malady than in tracing its rise and progress, seeking the cause of death in the changes discoverable in the internal organs, and recognizing as such, alterations of structure very inadequate to the effect, provided they were of sufficient magnitude to attract attention.

It is sufficiently evident that the long exclusion of changes in the fluids, and in the distribution of the more subtle agents connected with the phenomena of life from their due place in the doctrines of disease, has been mainly owing to the exclusive direction given by the study of pathological anatomy towards the solids; in other words, that the predominant system of a purely organic doctrine of disease has been invested with an importance by the cultivation of morbid anatomy to which, considered as an exclusive system, it is by no means entitled. At the same time, it seems clear that an impulse was given to the cultivation of pathological anatomy by the existing doctrines of solidism, rather than that these doctrines took their rise from the attention given to the study of diseased structure. This however, after all, is a question upon which there may be probably some difference of opinion, and at the same time one which, considered apart from the terms of the law of development laid down by M. D'Amador, is now of little importance. A more correct expression of this law, or fact, as it appears to us, is, that pathological anatomy in its progressive development has borne an intimate relation to the doctrines of pathology which have successively prevailed. However this may be, there can be no doubt that the researches of Morgagni in the first place, and subsequently those of Carmichael Smyth, Pinel, Bichat, Laennec, Dr. Abercrombie, M. Broussais, and others, which have contributed so much towards a correct knowledge of the changes induced by disease in the organization of the human body, have in this respect, by their very importance, tended to impede the study of those changes in the condition of the fluids, or in the action of the vital forces, which are of at least equal importance in a true system of pathology. Some of the errors thus resulting from the exclusive attention paid to changes of structure in the solids are well pointed out by M. Saucerotte.

> "Not only has this assiduous investigation into those diseases which, as it was thought, admitted of explanation from certain structural lesions, caused the consideration of the vital phenomena to which they are subordinate to be neglected; not only has it accredited the false idea that every functional disturbance is necessarily accompanied by sensible material changes; but it has also caused us to lose sight of the constitutional origin of a host of local affections, themselves only the crises of diseases; it has turned aside our attention from those epidemic influences which despotically govern disease, rendering necessary the most different modes of treatment when the organic lesions are the same, (facts well adapted to show with what distrust the opinion that anatomical observations should form the sole basis of pathology is to be received): finally, since morbid anatomy has been limited to the study of alterations of the solids, it has contributed to confirm the absolute dominion of that exclusive solidism, which leaves without its narrow limits so many unexplained facts." (p. 544.)

The great error of the organic pathology would however seem to be, that the distinction between the seat and the cause of disease has not been sufficiently borne in mind. It is thus that the alterations in the organization which, at one time, were regarded as indicating the cause of death, were subsequently considered as, in their initial state, constituting the cause of the disease. Really, however, these changes are in themselves no more than links in the chain of diseased action which is, as it were, thereby localized, but the original cause of which is to be sought elsewhere. It is thus also, that the luminous views of Bichat, and his predecessors, Carmichael Smyth and Pinel, have been distorted by the one-sided speculations of more recent writers, until the last finish was put to the organic pathology by the wild reveries of M. Broussais, who, with the most perverse ingenuity, has contrived not only to give a local organic cause to almost every disease, but actually, in a great measure, to limit the seat of this cause to a part of one particular system of the human frame, seeing in a *gastro-enteritis* the fertile origin of every disease which flesh is heir to.

M. D'Amador correctly observes, that the progress of pathological anatomy has, for some years, taken a direction towards the humoral pathology, and traces this change to the publication of the *Clinique Médicale* of M. Andral in the year 1825. Had M. D'Amador been as well acquainted with the writings of British authors, as he has shown himself to be with those of his own countrymen, he would not only have discovered similar traces of this change for some time before the appearance of the work of M. Andral, but also, that the pathology of the British school has never been so exclusively organic as that of the French; and, putting out of the question John Hunter, in whose writings will be found many passages tending to show that that master of our science has anticipated much of what is now brought forward by M. Andral and his followers, he would also have discovered that Drs. Prout, Burne, Carswell, Sir James Clark, and many others, of whose existence even he seems not to be aware, have done more towards the elucidation of these great questions than even the great chief of the *école eclectique* himself, to whose writings he attributes so powerful an effect. We have said that the origin of this change is referred by the French pathologists to the year 1825, and this period seems at least that from which we must date its progress amongst themselves. In 1829, we find M. Cruveilhier saying, "the more we study diseases, the more we seek to discover their immediate seat, the more are we led to think that the liquids are the vehicle of a great number of morbid causes, that a complete system of pathology should embrace the lesions of both liquids and solids." These opinions, which we are told were at that time confined to the brighter geniuses (*esprits élevés*,) were subsequently spread throughout all ranks of the profession, and thus, as M. D'Amador observes, the insufficiency of the solids has led to the study of the liquids, as the insufficiency of the seat has led to the study of the cause, and the insufficiency of local disease to the study of general disease.

The recognition of the agencies of vital forces soon followed, or was rather perhaps contemporaneous with the progress of the humoral code, and is equally attributed by M. D'Amador to MM. Andral, Lobstein, and Cruveilhier.

> "We may say, then, that by a long and painful path, the science of pathological anatomy has at length itself attained to the confirmation of the aphorism of the school of Hippocrates—'that we must consider in man not only the solids or containing parts, and also the liquids their contents, but above all the active forces, or those which give rise to motion.' Organic science commencing with the solids, concludes with vitalism, that is to say, with the Hippocratic doctrine properly comprehended; whilst, on the other hand, the school of Hippocrates commencing in vitalism, concludes in the course of ages by associating itself to the science of organic pathology: a fatal circle run by these doctrines, which by their ultimate concurrence prove at once both their individual insufficiency and their reciprocal dependence. For be it remarked that, at all times and in all cases, the fact—the practice—that which in the mass is called the history, commences by preceding the theory." (p. 333.)

These progressive steps which are, in the first place, brought forward as matters of historical demonstration, are then shown to be in accordance with what might have been reasonably anticipated. The organic pathology, being that which, by the more evident alterations of volume and consistence, the increase or diminution of organs, their hypertrophy or atrophy, reveals itself at the first glance when attention is directed to what is passing in the interior of the body, would naturally be the forerunner of those systems in which the more subtle though no less evident changes in the fluids or vital forces, are allowed their due place. But when the more refined operations of the body, whether in the state of health or disease are scrutinized into; when the molecular actions are investigated, the insufficiency of the solids to account for the phenomena, whether physiological or pathological, becomes instantly apparent. It is then that the blood, which Bordeu, in the spirit of the organic pathology, had asserted to be nothing more than liquid flesh (*la chair coulante*,) is recognized as being possessed of innate powers, and as materially influencing, and not unfrequently causing, by changes in its component parts, the series of diseased actions. The study of alterations taking place whether in the blood, or in the other fluids of the system, is but a more refined species of pathological anatomy; the mechanical process of the scalpel being here inadequate, the more refined processes of chemistry are called to our aid, and variations in the nature of the fluids which had otherwise escaped our notice are thus detected. It is obvious, that, whether these variations are to be considered as proximate or ultimate links in the chain of disease, they are deserving of our most attentive consideration. The enquirer into the pathology of typhoid fevers, for instance, who should content himself with merely ascertaining that congestions in the capillaries of the gastro-enteric mucous membrane are present in certain forms, while in others an injected state of the arachnoid tissue may be detected, overlooking the loose dissolved consistence and black colour of the blood, and the changes which take place in the proportion and qualities of the urine, perspiration, and other excreted fluids, will form a very imperfect idea of the nature of this family of diseases. And yet, not only are these fevers localized in one or other of the sites specified by high authorities, but their very essence is, on the one hand, asserted to be cerebral inflammation, on the other a gastro-enteritis.

It is not, however, our purpose here to refute these exclusive doctrines of the organic pathology. We are desirous merely of enforcing what M. D'Amador throughout his treatise ably points out, that the province of pathological anatomy is rather to detect the changes which have taken place, in the course of morbid actions, in the structure of the tissues and organs of the body, to demonstrate from such changes the seat of local alterations, to investigate their nature, and to determine their relation with the general progress of the disease and its symptoms during life; but that with respect to the nature of the proximate cause, the essence of the disease, the *primum mobile* of the organic aberration from the state of perfect health, it reveals absolutely nothing. Pathological anatomy will teach us that the lungs, for instance, may have been the particular organ in which the diseased action has been going on; that the changes thus induced are consolidation of texture, a distended state of the capillaries of the mucous membrane, or tubercular deposition in the bronchial tubes and cells. It may show that inflammation, or tubercular disease has existed therein; it may point out that the alterations of texture are in strict accordance with the physical signs afforded during life by auscultation and percussion; but to determine the peculiar aberration from the state of health which constitutes inflammation, tubercle, rheumatism, or any other morbid process, is beyond its powers and foreign to its true objects. The irregularity in the molecular actions which constitutes the essence of these maladies, may probably for ever elude our search, though it is the aim of the modern pathology to seek to appreciate this.

The theories of disease have in succession been explained, according to the vital and humoral systems, by the fanciful doctrines of animal spirits, of the four humours, of the presiding Archæus of Stahl, and the more rational humoralism of Boerhaave, Van Swieten, and Gaubius; according to the organic system of Hoffman and his followers, by alterations taking place in the actions of the solids, whether accompanied or not by changes of structure. "Where," asks M. Saucerotte, "was the concealed flaw which paralysed the productions of so many eminent men? It is, that from the consideration of the exterior phenomena of diseases, they proceeded at once to the consideration of the primitive forces impressed upon the organism, without stopping at the organic lesions by which they are necessarily connected; the intermediate link without which the two extremities of the chain could never have been united." (p. 501.) Between the disease itself, then, and the symptoms by which it is manifested, there is a link, or series of links, wanting, and it is in the filling up of this chasm that pathological anatomy has, according to M. D'Amador, followed the same course as the theories which have successively had the ascendant.

The fourth law declares that "*pathological anatomy, in each of its systematic transformations, has exerted a powerful influence upon the other branches of medical science, and has favoured their progressive development.*" It is unnecessary here to enter at any length upon the investigation of this law. It is sufficient to refer to what has been already said, to show the progress made in the doctrines of disease since the more assiduous cultivation of morbid anatomy. The several systems of the organic, humoral, and vital pathologies have been successively preferred, and the necessity of their combination in an extended and comprehensive theory of disease recognized through the insufficiency of any one to afford a satisfactory explanation of the phenomena. Among the other results springing from the investigation of organic changes, M. D'Amador enumerates the general improvement of the spirit of medical science, the impulse given to the methods of discovering and demonstrating medical facts, the improvement of organic chemistry and of comparative anatomy, &c. Our limits will not permit us to enter upon the illustration of these remarks supplied by our authors.

The fifth law is, that "*pathological anatomy concludes by exaggerating its own principle, thus constituting itself into an exclusive system.*" This is evidently the natural tendency of its cultivation upon a system of pathology essentially and exclusively organic, and therefore, as M. D'Amador observes, this law is but the abridged formulary of the history of medicine. The subsequent pages of this part of the treatise are accordingly devoted to a rapid summary of the progress of pathological anatomy itself. In this sketch we cannot follow our author, neither is it necessary for the due comprehension of the subject that we should make the attempt. It may however be stated, that after referring to Hippocrates and Aretæus, as having shown a remarkable knowledge of some diseased processes, the principal writers mentioned as contributing in an especial manner towards the progressive advance of this branch of medical science are Bonet, Morgagni, Bichat, and Andral, each of whom, more especially the three last, are to be considered as having constituted by their observations and writings a new era in this science. Here, as throughout, M. D'Amador appears to be lamentably ignorant of English and German authors. He enumerates, however, the worthies of the "*école anglaise,*" who follow, according to him, in the steps of Morgagni; and these are J. Hunter and Home (!) as the founders of the school, and *Sthear* and *Abernety* (sic) as its most recent cultivators! In the identification of Abernethy we have not much difficulty, although we may demur as to his claims to the place assigned him; but, as to who Sthear may be, we confess our utter ignorance. This school, however, he admits is rather than Morgagni to be considered as the forerunner of Bichat, and indeed that it almost anticipated him; and he might have added, had he been really acquainted with the writings of J. Hunter,—Andral, also. We cannot resist transcribing the following exquisite morsel in which the author of the *Anatomie Générale* is introduced: we will not lessen its force by attempting to clothe it in an English dress. "Or, trente ans après qu'il fut achevé, (the elevation of pathological anatomy into a science by

Morgagni,) c'est un Français, c'est Bichat, dont le génie trouve ce que le génie de tous les médecins anatomistes n'a pu encore trouver. C'est Bichat qui enseigne à l'Europe entière les principes et les lois des faits, et les regards étonnés se fixent sur l'auteur de l'*Anatomie Générale*, qui réalise si bien la prophétie qu'en avait portée Sandifort." (p. 387.)

Proceed we now to the second part of the essay, which professes to be a solution of the three anatomico-pathological problems in their application to the science of diseases. These problems are: 1st, that of the organic change; 2d, the relations of this change to the symptoms; 3d, that of the cause or of the diathesis.

"Every disease when fully formed," says M. D'Amador, "consists of external phenomena called symptoms, of organic changes, and of modifications in the vital forces which properly constitutes the affection itself, the morbid condition or diathesis." It is the middle term, the organic alterations, which forms the subject of the first enquiry. The two leading sections of the ancient school, the dogmatists and the empirics, were both deficient in this point, and the latter of these was content with the study of the symptoms alone. The former attempted to investigate the morbid condition, as well as the symptoms to which this condition ultimately gave rise; but the connexion between these two could not then be detected, and it is the school of pathological anatomy which has supplied the link wanting by the gradual development of the intermediate organic changes. The progress made by this school has been in accordance with the laws of induction; it has traced the relations existing between the external manifestations of disease and the organic alterations which have arisen in its course, and though subsequently the principle was carried out too far, yet when accumulated observation has demonstrated that residual phenomena exist, for the explanation of which the leading idea of organic change is insufficient, the modern school of pathology seeks a more general law under which their phenomena may be comprehended, and is thus led to discriminate between the organic changes as the effects of diseased actions, localized in the organs and tissues, and the actions themselves. This then is the province of pathological anatomy, not to form a system of pathology, but to develope the alterations arising in the tissues and organs of the body in the course of disease, and to determine the laws of their formation. "Le problème médical," says M. D'Amador, "a souvent changé de face." It has been stated in these terms,—A disease being given to find the remedy:—this is the purely practical view, and will recommend itself to the mere utilitarian. Pinel changed the question,—A disease being given to give it a character and to determine its place in a nosological chart: this is the view of the systematist. The pathological anatomist, again, says,—A disease being given to discover its seat and to determine the organic change in which it consists:—this is the view of the man of science, but it is imperfect and insufficient, inasmuch as, although a correct pathology always contemplates the three elements of disease, the external symptoms, the organic change, and the cause from which these spring, yet the existence of each of these elements is not necessarily implied in every disease; it is the last only, the cause, which is essential and without which the other elements cannot exist.

It has been observed, that it is the province of pathological anatomy to determine the seat of diseased action; and it is instructive to trace the progressive refinement which has been attained in effecting this object. In the earlier stages of this enquiry, the question involved little more than the organ affected: gradually, however, it was perceived that the various parts of which the organ consisted were liable to peculiar changes; as, for instance, the air-tubes of the lungs to catarrhal discharges, the cavities of the pleura to serous accumulations and adhesions, &c. The study of the membranes by Pinel, Carmichael Smyth, and Bichat led to the generalization of these facts; and the law was established, that catarrhal affections appertain to the mucous membranes especially, and effusion of serum to the serous membranes. It may be readily perceived that there are reasons for this in the general conformation and distribution of these tissues; but the occult causes, which must be sought in the molecular structure of the parts, have not as yet been revealed. A necessary result of this progressive advance in the investigation of morbid products has been a material change in the mode of contemplating disease. Many affections, which were formerly separated from each other, have been ascertained to be mere modifications of the same morbid condition; while others, again, have been separated from those with which they were formerly confounded, and distinguished by appropriate signs. The precision at which we have thus arrived in diseases of the chest may be adduced as affording illustration of the preceding remarks. The external signs of cough, expectoration, dyspnœa, various degrees of pain, &c., are common to almost all; and in many instances are altogether insufficient for the distinguishing of affections very different in their nature. Pleurisy, pneumonia, peripneumonia notha, catarrh, hæmoptoe, phthisis, asthma, dyspnœa, and hydrothorax constitute the list of pulmonary diseases formerly received; and how ill these affections were defined, and how utterly impracticable it was to distinguish one from another, need not here be insisted upon. Pathological anatomy, however, establishes some of these upon just grounds; indicates the existence of many which had not been previously recognized, or had been confounded under the vague terms of asthma and dyspnœa; and points out those differences in the organic changes which afford distinguishing characteristics: but it is a matter of vast importance to be enabled to ascertain these differences in the diseased state during life. New methods of investigation are required; and it was reserved for the admirable sagacity of Laennec, working still upon the foundation of pathological anatomy, to devise and perfect those methods of auscultation and percussion which leave in this department little to be desired.

In proportion to the advance of pathological anatomy, then, are we enabled to connect particular symptoms with particular organic lesions, and thus to ascertain the *proximate causes* of disease. In the same proportion, it appears to us, will it be found that the term *disease* will naturally be applied to the abnormal condition thus recognized, and not to the mere external manifestations of it. The improvements in diagnosis to which we have just alluded have effected this in a very remarkable degree as to complaints of the chest; and, when the diseases of other parts of the system shall be as well understood, we venture to predict that the Cullenian definitions, admirable as they are if looked upon simply as *descriptions* of groups of phenomena, will no longer be regarded as characterizing the morbid states whose cognizable effects they so graphically delineate. Let us draw an illustration of our meaning from mechanical science. A steam-engine may be compared in many respects to the living body: its different parts are *organized* to perform different functions, all tending to one end; and, for the maintenance of these functions, it is dependent upon the continued supply of external *stimuli*,—fuel to the fire, water to the boiler, and cold to the condenser; moreover, it possesses, by means of its *governor*, a certain degree of power of self-adaptation to changes in its external conditions. Now, from the excessive or deficient supply of these, irregularities in the operations of the machine will manifest themselves. Thus, an additional quantity of fuel may have been laid on the fire, or the water in the boiler may be low so that steam is generated too rapidly, or the quantity of work required may be suddenly diminished. The effect of either of these causes will be the same, acceleration of the motions of the engine. The latter, then, according to the old method of describing diseases, would be the sole account given of the disorder of our machine, although it might have arisen from either one of three different causes. It is obvious that no engineer would rest satisfied with such a symptom, but would, before attempting to apply his remedy, set himself to enquire what is the cause of the irregularity; on what change of the conditions of the machine it is dependent. This, however, is a very simple case. We will push our analogy a little further. We will suppose that, after violent irregularity of action, the machine suddenly stops. The engineer will, of course, do his best to restore its motion, by the alteration of the conditions on which it depends; but, if unsuccessful, he will search into the internal structure of his machine, with the view of discovering the cause of his failure. There he may find the displacement of a valve, the fracture of a rod, or the abrasion of the packing by which the piston is adapted to the cylinder, which may in some instances have been the original *cause* of the irregularity of action which preceded the cessation, or in others it may have been itself the *result* of an irregularity dependent upon some of the causes formerly mentioned. We hope that this illustration may set in an evident light the relation between pathological anatomy, pathology, and practical medicine.

These observations naturally lead us to the consideration of the second problem,—that of the relations existing between the organic change and the external symptoms by which it is accompanied.

In a perfect symptomatology, says M. D'Amador, this problem should be proposed as follows: 1. To determine, by the symptoms, the seat and nature of the organic change; 2. To explain, by the seat and nature of the organic change, the symptoms observed. The first question should be resolved during life, the second can be resolved only after death. The complete solution of these questions, however, implies, first, that every disease shall leave such marks of its progress as are appreciable to the senses, and that these alterations shall be always of the same nature; secondly, that the changes induced by death shall be sufficiently distinct from those produced by the disease; and, thirdly, that these last shall never disappear with the cessation of life, to give place to phenomena purely cadaveric. It is not necessary here to point out that, in the present state of our knowledge, these objects are unattainable; but that much may be effected by careful observation and study, in unravelling the intricacies which involve this question, is manifest from what has been already said in respect of diseases of the chest. That these difficulties, however, are altogether insurmountable, as M. D'Amador seems to apprehend,—at least in respect to many of the instances which he cites,—we are by no means prepared to admit. It is not because no proportion is found to exist between the extent of certain morbid alterations and the symptoms by which we are at this time accustomed to seek for them, that we are to draw the conclusion that such a proportion may not hereafter be discovered. Future researches may open to us other methods of investigation which shall sensibly indicate these changes in proportion to their extent, although, perhaps, the relative importance of the organ affected, or its want of sensibility, or its situation in the innermost recesses of the body, may prevent the manifestation of the changes of structure which it may be undergoing, in a corresponding amount of physical suffering, or other readily recognized signs. The pain or inconvenience experienced by a person labouring under very extensive disorganization of the lungs, for example, is sometimes comparatively trifling, and in slighter cases may be, and frequently is, altogether overlooked; and in such instances we cannot doubt that the true nature of the disease, as well as its extent, would formerly have long escaped detection. But, since the introduction of new methods of investigation, percussion and auscultation, the most intimate relation between the organic changes and the symptoms by which they are accompanied is found to exist. Dr. Bright's admirable researches upon disease of the kidney in connexion with an albuminous state of the urine, followed up and rendered more definite in their application by Drs. Christison, J. C. Gregory, and Osborne, afford another excellent illustration of the remarks we have just made. It may possibly be objected that the signs of disease afforded by these methods of exploration are not symptoms in the usual acceptation of the term; but to us the dull sound beneath the clavicles elicited on percussion, or differences in the respiratory murmur in the same region, perceived by the application of the ear, are as clearly symptoms of disease as the pain or tumefaction, detected only on close and accurate manual examination, of parts deeply seated in the abdomen or pelvis. If, however, we are to understand by the term symptoms, merely those evident signs which are of such magnitude as to attract the notice of the sufferer, or to give rise to much personal inconvenience, then it must be granted that no relation can ever exist between the internal change and the symptoms attending it; and, further, that, thus limited, the study of symptoms, so far from aiding in the discovery of disease, reveals nothing but a tissue of contradiction and fallacy.

It would be an instructive task to examine the instances brought forwards by the author, as well as numerous others which might be adduced in illustration of these observations; but we are reminded that another problem, that of the causes or diatheses, yet remains for consideration.

This problem, says M. D'Amador, embraces three principal questions, to be resolved by pathological anatomy: 1, To prove, by pathological anatomy, the existence of diatheses or general conditions, tending to disease; 2, to show the influence of these as occasional causes in the production of organic changes; 3, to study the influence of inflammation, first considered as a diathesis, and subsequently as an occasional cause of change of structure. To these might have been added with advantage the study upon similar principles of the tubercular, the carcinomatous, and other no less marked diatheses, which, especially the tubercular, are of equal importance with that of inflammation.

These questions, in the present state of pathology, would require a very extended discussion for their complete elucidation: it will be desirable, therefore, here rather to give the opinions of the author than to enter into any abstract speculations concerning them. At the same time it will be observed, that many of the views brought forward as peculiar to the French eclectic school are sufficiently familiar to British pathologists, although, from the ignorance of M. D'Amador of the writings of our countrymen, he has been led into the error of considering them as of very recent date.

"One of the errors of the exclusive anatomico-pathological school was the rejection of internal predisposing causes, and the exclusive study of occasional causes. The insufficiency of the alterations of the solids in the theory of disease has necessarily led to the study of the liquids; and thence has arisen the whole system of the anatomical humoral pathology (*l'humorisme anatomique*). Each liquid globule lives, assimilates nourishment, secretes, and is susceptible of passing through every degree of organization; and it is upon this fact that the modern system of humoralism is really based. Now, the condition of a single globule may communicate itself to all those which circulate with it, and the participation of all the molecules in a diseased modification experienced by one only, or a small number, constitutes in fact a diathesis. This general condition, or diathesis, is revealed by pathological anatomy and by organic chemistry. What is better known than the great accumulations of serous, fatty, or lymphous depositions, effused into the cavities and infiltrated in the tissues, and bathing, as it were, every organ of the body; or than those primitive *cachexies*, as recognized by the ancients and confirmed by the investigations of the moderns; cachexies really *essential*, for the morbid alteration passes directly from the forces to the liquids, leaving the solids unaffected?" . . . . "The quantity alone of the liquids, either by augmentation or by diminution, may constitute a disease. Observers of all ages and of every school have admitted the existence of plethora,—that is, the superabundance of the sanguineous fluid,—as a fact established by experience; while, at the present day, the liquids take so essential a place in the theories of pathological anatomy, that the most marked and distinctive characters of almost every alteration of the solids is now attributed to them. It is pretty generally considered that cancerous matter is but the production of a morbid secretion, an opinion first announced by M. Cruveilhier; and the same view is taken of tubercle. The eclectic school has deprived the organization of that property with which the school of Laennec had invested it, of creating everywhere new tissues, the type of which does not exist in the normal organization. Medullary sarcoma, melanosis, cirrhosis, &c. were, according to Laennec, so many tissues of new formation. According to the recent views of organic science, these products are to be regarded rather as altered secretions; but it will always remain to be ascertained whether these morbid secretions may or may not have their analogues among the normal secretions. This view has acquired such importance that M. Andral makes it the foundation of his anatomico-pathological classifications. According to him, every degeneration of the solids resolves itself, in tracing the origin and mechanism of its formation, into an alteration of the circulation, of nutrition, or of secretion; that is, into an alteration of the liquids." (p. 449.)

According to the eclectic school, then, the organic tissues are in themselves incapable of degeneration. They are only liable to atrophy or hypertrophy,—to a diminution or increase in their nutrition. It follows that every alteration of texture arises, without exception, from the deposition of secreted matters in the meshes of the cellular tissue. These secretions accumulate, become developed and organized; thus gradually encroaching upon the normal tissues, and ultimately altogether destroying them by inducing a state of atrophy. It results from these speculations, which profess to be founded on the facts of pathological anatomy, that there are diseases which consist in general conditions, as they are termed by the author; conditions of disease existing throughout the whole body, before any localization of the morbid process shall have determined the explosion of the disease upon any particular organ or tissue.

The second question proposes for consideration the influence of these diatheses, viewed as occasional causes in the production of organic alterations. Bichât was inclined to suppose that each tissue has its own peculiar diathesis; but a more comprehensive view fully establishes the fallacy of this opinion. It is indisputable that certain diseased actions manifest themselves more commonly in certain organs or tissues than in others: thus tubercle, though found in every part of the body, is far more frequently deposited in the lungs than in any other site; carcinoma in the mamma and uterus, &c. M. D'Amador says, that it would appear that certain lesions are more prone to occur in one or other organ, according to the more or less intimate relation of the diathesis giving rise to them with the intimate structure of the parts. This may possibly be an expression of the fact; but it affords no explanation of it, no more than the substitution of one arbitrary symbol for another, in an algebraical formula, would lead to the detection of the unknown quantity which it is intended to represent. There is little in this part of the treatise which need detain us, since it consists chiefly in the exposition of what is already well known. The following are the conclusions at which the author arrives:

> "Diatheses, then, should be considered as peculiar states, either of the solids or especially of the liquids, and, above all, of the blood, constituting a kind of vitiated nutrition, sometimes obscure and latent, sometimes characterized by its effects upon various parts of the economy. Hence it is that local treatment of these affections is never sufficient to eradicate them from the system; that, when they disappear from one part, they reappear at another. It is by modifications of the general influences alone that the constitutional state can be changed and the disease destroyed." . . .
> "We conclude, therefore, that, if the knowledge of the seat of the affection is advantageous, the knowledge of the diathesis is indispensable; for, possessed of this last, there is a much better prospect of the removal of organic disease already existing, and of its prevention where it has not already been formed." (p. 459.)

The third question, referring to the influence of inflammation as a diathesis and as an exciting cause of change of structure, we pass over, contenting ourselves with quoting the conclusion, of which, indeed, the remarks preceding it are but the amplification; viz. that inflammation is to be looked upon only as a particular diathesis, and is by no means to be considered as the sole and universal cause of diseases.

Previously to bringing this notice to a conclusion, we must make a few observations upon the Essay of M. Saucerotte. Like that to which we have already devoted so much attention, it is divided into two sections. The first section comprehends a brief sketch of the state of medicine, considered as a science, previous to the epoch of Morgagni; and treats of the influence which pathological anatomy has had upon the knowledge of the seat, nature, and treatment of diseases; its influence upon theory, and the philosophical spirit developed in consequence of its cultivation; the methods employed in the study of pathological anatomy, in relation to the influence which these methods have exercised upon medicine, and the errors introduced by it into the science. The second part treats of the influence which pathological anatomy has exercised on the knowledge of diseases considered in detail. These are grouped together under the several heads of diseases of the cerebro-spinal apparatus, diseases of the circulatory system, of the respiratory system, of the digestive tube and its dependencies, and general diseases affecting the whole frame or certain tissues only. Some remarks upon the influence of pathological anatomy on certain specialities of pathology, and its application in general to the medical sciences, follow; and a brief retrospect concludes the essay. M. Saucerotte, notwithstanding the tenor of the observations which we have previously quoted from him, is a localist, as the following extract sufficiently evinces: "To know a disease," he observes, "is to know its seat and its nature. We can no more form a conception of any diseased affection without a seat, than we can of digestion without the stomach, or of vision without the eye." He would also seem to be a decided advocate of the organic pathology; for he asks, "Is it not, in effect, in the organs that it (disease) exists; and, if we know not the organs diseased, how can we know the diseases?" Nevertheless, in the subsequent discussion of these principles, and in attempting to illustrate them by an appeal to observed facts, the author is obliged, in part at least, to swerve from his position. Some of these passages we had marked for quotation and comparison with the opinions expressed by M. D'Amador; but the length to which this notice has already extended compels us to forego our intention. Our opinion of the merits of these dissertations may be gathered from what we have already had occasion to observe respecting them; but it would scarcely be just to M. Saucerotte, were we not to add that he seems to have spared no pains in the investigation of his subject; and, although his treatise perhaps is not equal, as a philosophical production, to that of M. D'Amador, it yet possesses very meritorious qualities, affords a good view of the present state of our knowledge, and evinces an acquaintance with the medical literature of other countries beside his own; a quality in which, we regret to say, the essay of M. D'Amador is deficient.

## Art. II.

*Remarks on the Treatment of Fractures of the lower Extremities without the Aid of Splints.* By J. F. Burke, M.R.C.S.—*London*, 1837. 8vo. pp. 39.

*A Practical Treatise on Fractures, illustrated with sixty Woodcuts.* By Edward F. Lonsdale, Surgeon, Demonstrator of Anatomy in the Middlesex Hospital School of Medicine.—*London*, 1838. 8vo. pp. 536.

We do not propose to give an analysis of the pamphlet of Mr. Burke, because it contains nothing which is not, to some extent, a matter of everyday practice; to as great an extent, indeed, as the principle can be safely applied. We apprehend the author's experience in the treatment of fractures must be very limited, or he would not have imbibed so great a horror of the judicious application of splints; a horror so intense, that he appears to deem any sacrifice light by which they may be superseded. By splints he can conceive nothing but an obdurate piece of wood or iron, which, if it enter not into the patient's soul, may at least very much inconvenience his limb. Now, in our innocence, looking at the subject in an enlarged sense, we had conceived that any apparatus by which the necessary resistance to displacement was offered, whether it were a piece of pasteboard, a junk, or any similar contrivance, was, in fracture-language, just as much a splint as a piece of board or iron so many inches long and so many wide. On this point we would adopt Mr. Lonsdale's sentiment:

> "It is the fact of many fractures getting well by position only that, I believe, has led some surgeons to adopt the erroneous opinion that splints are altogether useless, or any mechanical contrivance that tends to keep the ends of the bone in apposition. How far such an opinion is correct, any one who has had much experience in treating fractures must be able to judge; for there are many cases that absolutely require mechanical means to steady the portions of bone, while the majority of fractures are benefited by splints of some kind. I am inclined to think that those who advance the sweeping assertion of splints being useless, have formed their opinion upon very limited experience, and judge of single cases that may have got well under their care without their employment, instead of taking the majority of cases, which are always benefited by their use, and many of them absolutely requiring it." (p. 51.)

The impression we derive from the perusal of the second work, from which we have just quoted, is, that Mr. Lonsdale is a careful observer, that he possesses ingenuity in mechanical contrivances, and that he has a competent knowledge of the routine treatment of fractures in the hospitals of this country; but, if we had been consulted, we are not sure that we should have advised him to publish the work before us. It would require a better reason than the difference in price to convince us that, whilst we possess Sir A. Cooper's work, the profession required the present. This most distinguished surgeon, in the plenitude of his experience, felt it was unnecessary to give any other than his own ideas: but our author is a much younger man, his experience comparatively small, and, to render his work acceptable, he should have made it complete. We cannot say that this is exactly the case, although it contains much, and may be safely consulted by the young surgeon in his difficulties. We, in particular, desiderate some account of fractures of the cranium and of the vertebræ; subjects of the highest importance. We also regret that no allusion is made in this work,—nor indeed, we may say, in the generality of works upon Fracture,—as to what should be done when there is a vicious consolidation. Should an attempt be made to rupture it? Some persons have come to the conclusion that such cases should be left to themselves; but we think the facts adduced by Fabricius and Purmann, and more recently by Œsterlen, seem to prove that the rupture of the callus may be often done successfully. Œsterlen gives eight cases which establish the possibility of straightening the limb by extension made at the time pressure was applied at the deformed joint, even at the end of the fifth week. For the particulars of these cases, and the mode of operating, we refer to the original work.* Various modifications of the same general mode have been employed by other men, and with considerable success.

We also have some reason to complain that Mr. Lonsdale has not used sufficient diligence in ascertaining modes of practice even amongst ourselves. Much time and space are occupied in proving that, in fracture of the forearm, the palm of the hand should not be placed against the chest. This was scarcely necessary: the common practice is not so: the dictum of our own quondam instructor was, "bring the hand so that you can spit into it." Again, he should be aware that, although luxations of the wrist are uncommon, yet that they may exist. In our own country, up to a very recent period, luxation of the wrist was supposed to be not an unfrequent accident; fracture of the lower portion of the radius being mistaken for it. In France, for many years, a directly opposite opinion was maintained, based upon the great experience of Dupuytren: he, as well as Marjolin, regarded all cases of luxation of the carpus, described by observers, as cases of fracture of the inferior extremity of the radius. We think it excessively doubtful whether the accident be possible without fracture, as a consequence of a fall upon the palm of the hand; because the repulsion of the ground is entirely directed upon the inferior extremity of the radius, and not upon the ulna: neither do we believe that the fracture of the radius is likely to occur as a consequence of a fall upon the dorsum of the flexed hand. In the work of Cruveilhier (*Anatomie Pathologique*, 9ᵉ livraison,) may be found conclusive evidence of the occasional occurrence of such an accident.

It must not be supposed, from these strictures, that we do not think the work of Mr. Lonsdale creditable to him, or that it does not contain much that is good. The ordinary modes of treatment practised in our country are plainly described in it; its woodcuts afford considerable assistance towards comprehending points which descriptions might leave doubtful; and several ingenious apparatuses, of easy application, of the author's own invention, are given in it,—especially one for fractured jaw: finally, it is published at a moderate price. We extract from it the following interesting table, showing the comparative frequency with which certain fractures occur. It will be interesting to compare it with a table of the same kind, and on a still larger scale, and containing more particulars, extracted from the records of the Pennsylvania Hospital, published in our last Number.

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* Ueber das Kunstliche Weiderabbrechen fehlerhaft, &c.—Tubingen, 1828.

*Table of the comparative Frequency of Fractures in the different Bones, observed at the Middlesex Hospital, from Sept.* 1831 *to Sept.* 1837.

| | | Out-Patients. | In-Patients. | Total. | |
|---|---|---|---|---|---|
| Hand | Phalanges | 63 | 3 | 66 | |
| | Metacarpus | 50 | — | 50 | |
| Fore Arm | Both bones | 85 | 8 | 93 | 386 |
| | Radius, singly | 193 | 4 | 197 | |
| | Ulna, singly | 63 | 1 | 64 | |
| | Olecranon | 29 | 1 | 30 | |
| | Coronoid process | 2 | — | 2 | |
| Humerus | Shaft | 78 | 11 | 89 | 118 |
| | Neck | 13 | — | 13 | |
| | Condyles | 16 | — | 16 | |
| Clavicle | | 260 | 13 | 273 | |
| Scapula | Body of the bone | 3 | 5 | 8 | |
| | Acromion process | 8 | — | 8 | |
| | Neck of the bone | 2 | — | 2 | |
| Face | Ossa Nasi | 10 | 3 | 13 | |
| | Upper jaw | 1 | — | 1 | |
| | Lower jaw | 9 | 23 | 32 | |
| Thorax | Ribs | 270 | 87 | 357 | |
| | Sternum | — | 2 | 2 | |
| Pelvis | | — | 7 | 7 | |
| Thigh | Adults | — | 144 | 144 | |
| | Infants | 37 | — | 37 | |
| Patella | | — | 38 | 38 | |
| Leg | Both bones | — | 197 | 197 | 289 |
| | Tibia | — | 41 | 41 | |
| | Fibula | — | 51 | 51 | |
| Foot* | Os Calcis | — | 2 | 2 | |
| | Astragalus | — | 1 | 1 | |
| | Toes | 4 | 7 | 11 | |
| Skull | | — | 48 | 48 | |
| Spine | | — | 8 | 8 | |
| Total number, during six years | | | | 1901 | |

With these few remarks we dismiss Mr. Lonsdale's work, and shall devote the remainder of this article to the consideration of one particular mode of treating fractures, which has been of late years extensively used in other countries, and which, we doubt not, will, ere long, be largely tested in our own. We are the rather led to confine our notice to this point because the more common treatment employed in England is presumed to be familiar to all our surgical readers, and because we foresee that this treatment is about to undergo important modifications, from the greater or less adoption of the treatment *by the permanent or immoveable bandage*, which we are now about to consider.

We are most ready to admit that, carefully and judiciously employed, the treatment by splints, such as are in common use, ensures the integrity of the limb; but at the same time we cannot conceal from ourselves that serious inconveniences attach to this method: the patient is long confined to his bed; he becomes extremely fatigued by the position which it is necessary to preserve; the digestive and other functions are impaired; and the general health may, and often does, greatly suffer. To obviate these pressing evils, and to enable the patient to change his position as often as it becomes fatiguing, or indeed to rise from his bed, a variety of means have been employed; and it is singular that, with the exception of Amesbury's apparatus, and certain modifications of it, such as that in use at the North London Hospital, which have, from time to time, been, to a limited extent, introduced in practice, no means of enabling the patient to rise early from his bed have been much employed in this country. Better success attended the attempts which were made in other countries to escape the inconveniences alluded to, and several forms of permanent immoveable apparatus have been among the means employed for this purpose.

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* "Fractures of the small bones of the tarsus and carpus are not inserted as distinct fractures in the hospital books, but come under the head of severe lacerations or bruises, the injury of the soft parts being the most important point: I have, however, seen many cases."

When or where this system of treatment originated, it is not easy to determine; nor can we be sure that the idea, suggested to the mind of Geoffroy by an inspection of some relics found in Egyptian monuments, —that some such system existed in those ancient ages,—was correct: certain, however, it is, that in very early times many of the nations of the East, Arabs, Persians, and others, employed the system, which has continued to be used up to the present moment, and which seems to retain much of its original simplicity.

At the sitting of the Académie de Médecine, held 13th January of the present year, M. Sedillot presented a fracture-apparatus which he brought from Constantine, and which he had taken from a Turkish woman, whose arm had been fractured by the bursting of a bombshell. This apparatus was composed of thirteen "*nervures*" of palm, each one inch wide, two to three lines in thickness, and nine inches long, convex on one surface, plane on the other, and arranged along and secured to a portion of prepared sheepskin; a space of three or four lines being maintained between them. This apparatus was supported upon the members by means of cords of wool loosely applied, but susceptible of constriction by twisting pieces of wood under them until the necessary force is given: in fact, a species of tourniquet. So arranged, this apparatus is perfectly applicable to simple fractures; but, in the case of the poor woman referred to, it was compound, and required dressing: to accomplish this without removing the apparatus, a portion of two of the splints, with the skin which covered them, was removed. Many modifications of this apparatus appear to be in use among the different tribes, and they are all ingenious. Certainly the contrivance which they employ to give to the apparatus the necessary constriction is worthy of remark; for it does not expose the member to any shock, and completely answers the end.

Although, in our estimation, the means to which we invite attention may be incontestably superior, we cannot withhold our admiration of the address and ability displayed by men who, deprived of all the resources we possess, having at their disposal only pieces of wood, coarsely divided, skins of animals, and undressed cords of wool, treat so methodically such a serious lesion, without neglecting any of the indications which European art, despite of all its perfections, has only lately recognized: we mean, the necessity of well and conveniently maintaining a state of perfect rest until the consolidation is complete.

According to De Pouqueville, (*Voyage dans la Grèce,* 1820-1,) the ancient as well as the modern Greeks employed a permanent apparatus, which was applied soon after the receipt of injury, and which, with perfect security, enabled the patient to move the limb in any direction: for the purpose of consolidating the apparatus, which was not removed from the moment of its application to that of cure, a composition into which mastic entered largely was used. A similar system, though brought about by other means, is employed in Spain, where it would seem to have been introduced by the Moors; in Corsica, where it is accounted a surgical heresy to remove an apparatus before the consolidation is complete; in the Brazils; in Italy, where it has been modified, of late, by Assalini, who used moistened pasteboard as the agent; in Switzerland, where, according to Fodéré, is found a class of men called "Rébouteurs," whose business is to set broken bones and to reduce luxations, and who travel through Switzerland, the Alps, Savoy, Alsace, and the Vosges, in the practice of their art. In the Vosges there is a family named Valdajos, who for two centuries have transmitted from father to son the secret of treating such accidents. Their success is said to be remarkable, and their modesty to equal their celebrity. Valdajos, Fleurot, and the Jollans attribute their success entirely to the permanence of their apparatus, which is composed of moistened card-board, or osier splints, solidified by mastic or pitch.

In 1734, the ancient Academy of Surgery in Paris offered the following as a prize-subject: "To determine, in each kind of surgical disease, the cases in which it is desirable to dress frequently, and those where it should be rarely done." The essay to which the prize was awarded was that of Lecat, who, in speaking of fracture, says, "A fracture, if simple, when reduced, only requires to be maintained: it may be examined *once*, to see that there is no displacement or deformity." In the year 1768, a memoir was presented to the same Academy by M. Moscati, in which he described a mode of treatment which he had employed for the cure of several fractures: he made a kind of mould, by means of compresses and bandages, saturated with white of egg: this apparatus remained applied up to the moment of cure.

The system thus introduced was not employed, even by the French, until the time of the earlier campaigns of Napoleon. In the first campaigns of Prussia and Poland, where the stations of the grand army were inconvenient and distant, and where M. Larrey found much difficulty in transporting the wounded, especially those suffering from fracture, he employed the system extensively, and with great success: in fact, many of the wounded at the battle of Moskwa were removed into France completely cured, and without a single removal of the apparatus. M. Larrey asserts, that, "whatever may be the nature of the fracture, the apparatus ought to remain applied, without removal, until the union is complete, and the wound, if there be one, entirely cicatrized; that no thought need be taken of the fluids or purulent matter which may be exhaled from the wound, because, in depriving these solutions of continuity, of contact with air, we isolate them, on the one hand, from the humidity or insalubrious condition of the atmosphere; on the other, we spare the patient the pains of frequent dressings; we prevent the chance of motion among the fragments of bone, local irritation, erysipelas of the part, or nflammation."

The apparatus he employed for the lower extremity was composed of a linen cloth, of the necessary length; two junks, an inch and a half in diameter, somewhat shorter than the linen; a small thin mattress, or cushion, stuffed with oats, and of the length of the linen; a "talonnière," or heel portion; a conical cushion, six inches long, three wide, and at its base two thick; three six-tailed compresses; bandages, and resolvent liquid—a mixture of camphorated spirit, extract of lead, and whites of egg, beaten up with water. The mode of application is quite simple: the limb is carefully raised, reduction is made, and the apparatus is disposed in the following order: the ligatures, five or six, the linen several times double, the bandage; the limb is now placed on the bandage, which lies upon the linen. The linen must extend beyond the heel and the popliteal space; compresses, dipped in the discutient fluid, are smoothly placed upon the fractured point; the bandage, similarly impregnated, is now brought carefully around the leg; the heel-piece is placed under the tendo Achillis, its base corresponding to the heel; the cushions are placed laterally in the hollow of the leg, the longest externally; the junks are now brought on either side of the leg, and rolled in the free border of the linen, until they are on the level of the leg: the ligatures are then drawn, constriction being gradually made from the knee downwards, care being taken to avoid placing one over the fracture; a stirrup is placed under the plantar surface of the foot, and attached to some of the ligatures. Such is the apparatus, which does not require much longer time to apply than those which are ordinarily used. It requires more care, however, because it is destined to remain through the whole time necessary for the cure.

In the preamble of his German translation of the *Parallèle* of Roux, published in 1817, Froriep says, "I am astonished that the method of the Moors of northern Africa has not been used: it consists in surrounding the limb with an envelope of plaster." He thought that it would be necessary to cast it in many pieces, so that it might be easily removed, if necessary. The first experiments on the subject at the Berlin Hospital were made by Keyl, at the suggestion of Professor Kluge, director of the hospital, in 1828 and 1829. In 1829, Dr. Rauch, and afterwards Dr. Muttray, made it the subject of their inaugural theses; and, after the latter, we shall describe the manner in which Dieffenbach arranges his apparatus.* He prepares a box of the necessary capacity; the limb is rubbed over with some fatty substance, is properly extended, and the semifluid plaster is poured in in sufficient quantity to envelope the limb: at present, if the fracture be complicated, he envelopes only four fifths, or even less, of the limb, so as to permit of the necessary dressings.

A correspondence has lately appeared in the London Medical Gazette, in which the merit of originating this method has been claimed by Mr. Sweeting, for Mr. Bond of Glastonbury, and by Mr. Beaumont for himself: perhaps, if this statement meet the eye of the parties, they may think it unnecessary to prolong a discussion which cannot end in giving to either party the merit of the invention. The apparatus invented by Mr. Beaumont for receiving the leg and the plaster, and at the same time keeping up extension, is ingenious: it is in the museum of the College of

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* Muttray, De Cruribus Fractis gypso liquefacto curandis.—Berlin, 1831.

Surgeons in London. In the late Polish struggles, a similar method to that of Dieffenbach was advantageously employed by M. Malcz.

In 1834, M. Seutin, chief surgeon of the Hospital Saint Pierre, at Brussels, charged with the care of a large number of the wounded at the siege of Antwerp, (many of whom suffered from fracture,) employed the method of Dieffenbach as a means of treatment; but he found that the material lost in tenacity what it gained in solidity; that it did not resist external shocks—that it was easily broken; thus losing the most precious advantage, that of permitting progression. He then employed the apparatus of M. Larrey, which possessed neither of these drawbacks; but he found in it another: the ingredients which enter into its composition are not always easily procured, and its removal is troublesome, and the patient sometimes complains of its weight. These circumstances induced him to seek some means by which the principle might be carried out in a manner more free from objection; and such a means was presented to him in *starch*. By means of starch an apparatus was arranged which was easily procured, and not costly: it acquires considerable solidity, and a tenacity which enables it to resist perfectly external shocks; and, when its removal becomes necessary, it is easily accomplished without the destruction of any portion of the apparatus. From that time he has treated all cases of fracture, not only in the hospital, but in his private practice, by this method, with the most complete success.

We have ourselves employed this mode of treatment with the most gratifying result in some cases where the tumefaction was considerable.

In the practice of M. Seutin, on each occasion the bandage has been promptly applied, and the patient has been, in the greater number of cases, raised up, and placed on crutches, as soon as the bandage was thoroughly dry. Persons have proceeded on their journeys at the end of three days from the application of the apparatus. Those living in the town, with simple fracture, are bandaged, looked after for four or five days, then discharged: at the end of five or six weeks they come again to the hospital; the apparatus is removed; they are found cured, and ready to return to their usual occupations.

Now, beyond the success of the treatment, this is a matter of first-rate importance. At present, with us, a tradesman to whom such an accident occurs is prevented from attending to his business; a traveller, on urgent business, must be detained at, perhaps, a country village where the accident may have happened; while, if our statement as to the efficacy of the treatment be correct, no such inconvenience need attach to either.

The mode of application of this apparatus is as follows: immediately after the reduction of the fracture, the limb is surrounded by compresses dipped in Goulard water; a common bandage is carefully applied over these, from the root of the toes to the knee, (supposing the leg to be the seat of fracture:) this bandage is then, by means of a brush, covered over with thick starch; then another bandage is applied, beginning at the knee and descending towards the foot: this is covered like the first, to which it adheres, except on either side of the tendo Achillis, where a little padding is applied. Four pieces of thick pasteboard are moistened and moulded upon the leg, before, behind, and on either side of the fractured point: these are secured by two other bandages, one passing from the heel to the knee, the other from the knee to the heel, and covered by starch as before. From two to four days occur before the apparatus is perfectly dry; and from this moment the patient may get upon crutches, a support being given to the affected foot by a stirrup around the neck.

On the 25th September, 1837, M. Velpeau presented to the Academy of Sciences a memoir in which is discussed the two prevailing systems of treatment. He maintains that, whatever may be the nature of the fracture, whether accompanied by tumefaction or wounds of the integuments, reduction should be immediately proceeded with: this being accomplished, the part is to be surrounded by compresses, and a bandage moderately firmly applied, extending upwards from the points of the fingers or toes to the superior extremity of the fractured limb: the bandage is then to be brushed over with starch, and so on. The compression, being equal and moderate throughout, sustains the tissues, without occasioning the slightest uneasiness: the patient may turn, move, and act in bed as if there were only simple contusion of the leg. He is no longer condemned to lie on his back for six weeks or two months: he may get up on the third day; he may, without inconvenience, sit on a raised seat, because the leg may be moderately flexed; he may walk with the assistance of crutches, the foot being sustained by a stirrup around the neck. M. Velpeau submitted to the inspection of the section of Surgery, patients who walked on the second, third, fourth, and sixth day after the occurrence of the accident. It is admitted by M. Seutin that the principle upon which his system is framed is a simple, but certainly a successful modification of that of Larrey. It has been employed by him in about two hundred cases, with the greatest success; by Velpeau; by a considerable number of surgeons in France and Belgium; and by ourselves, in cases in every respect confirming the statements made in its favour by the persons to whom we have referred.

We shall now shortly consider the objections which have been raised against the principle upon which the system is founded; and we shall hold that, at the present moment, the method is most perfectly carried out by the apparatus of M. Seutin.

The principal objections which have been raised against this method of treating fractures are the following: If complete immobility be so necessary to prevent motion at the fractured point, this, it is objected, is often not attained because from thirty to sixty hours are required for the complete desiccation of the apparatus, during which time motion may be permitted; and because, if, at the moment of application, the tumefaction be considerable, when that subsides, a large space will be left between the bandage and the limb; and there will be, therefore, no resistance to displacement. The answer which may be given to this objection is, that, when a fracture is reduced, it is not usually in the first two or three days that displacement happens; it is after this period, and when motion, involuntary or indispensable, is daily repeated, and the ordinary bandages have become loosened, that this occurs. Now, although humid, the starch apparatus maintains coaptation during the first three days, with at least as much firmness as the ordinary apparatus; that time passed, the danger of displacement is over; the pieces entering into its composition form a compact, coherent mass, exactly moulded upon the limb, adapting itself to all the inequalities of surface, and forming with it a perfect whole. A displacement, according to the axis or to the circumference of the limb, is completely prevented, because the superior fragment is included in the same mould as the inferior, and both must together be carried in any direction which is given to the limb; for no separate portion or fragment can possess the power of moving without the motion of the whole.

If we move a lower extremity thus maintained, what happens? If an impulsion is given to the foot, the movement is transmitted to the leg, to the thigh, to the pelvis: no partial movement can occur, but the whole member must be borne in the direction of the impulsion, and no displacement of the fragments can have place, because all are moved in the same direction at the same moment.

Already many substitutes for starch have been proposed, with the view of removing the inconveniences with which the apparatus is charged. A composition of starch and alum, glue, pitch, and other substances, have been named. M. Lafargue has just detailed a case in which he employed a mixture of starch with gypsum in fine powder, in which the apparatus was perfectly firm in six hours. M. Velpeau at present employs, with the same view, dextrine.

With respect to the second point, as to the space which may be left between the bandage and the limb, it usually happens that it is not considerable; it is not produced until after many days; the work of consolidation is then somewhat advanced; the ends are included in a callus which affords a powerful support.

Supposing the apparatus to be placed on a limb which is much tumefied, and that tumefaction rapidly subsides, so that at the end of a few days a considerable interval is found to exist between the limb and the bandage, the inconvenience may be remedied by firmly applying another bandage. The solidity of the apparatus might be thought an obstacle to the action of the bandage, but experience has shown that the difficulty is not insurmountable.

Another objection which has been raised is, that the early application of a bandage, before the development of a coming tumefaction, may produce a strangulating effect. Experience has shown that the fears of gangrene, as a consequence of this pressure, are unfounded. When an ordinary bandage is too tightly drawn at the level of the fractured bone, whilst the lower or more distant portion of the limb is less firmly compressed, tumefaction follows, which, if the constriction be continued, will end in gangrene; but a pressure of an uniform character, extended from the toes or fingers, uninterruptedly, along the member, is opposed to the tumefaction of the parts and to gangrene: in fact, by such pressure, we may procure atrophy of the limb, but not gangrene. Therefore is it important that the compression should be exact and uniform; and, in a large majority of cases, its effects, so far from being injurious, will be found to be decidedly salutary.

In fact, there are two kinds of compression: the one which is opposed to the reflux of venous blood, (that, for instance, which we establish circularly around the arm when we wish to bleed a patient;) the other, which is opposed to the ingress of arterial blood and favours the egress of venous blood, is that of every agent which compresses a limb equally at all points, and commences by a methodical compression of the portion farthest removed from the trunk. The first determines gangrene when long continued; the second is decidedly opposed to inflammation: the first is as strongly repulsed by the advocates for permanent bandaging as by others; and the second must belong to that method properly employed. Still we would say that, where the tumefaction of the limb is very great, the application of the apparatus should be delayed for some days; not because pressure would exercise any baneful influence, but because, when the tumefaction is dissipated, too great an interval may be left between the limb and the bandage, and because some difficulty might be experienced in determining upon the exactitude of the coaptation.

Another disadvantage which has been attributed to this method, is, that the practitioner is left in the dark as to the state of the limb; that, when once applied, it must there remain up to the moment of consolidation of the fracture; and that, before this period, we cannot ascertain whether there be eschars, abscess, or other similar complications. If only moderate attention be used, none of these accidents need occur: they must produce pain and general symptoms; and nothing can be easier than to remove the bandage. As far as strangulation is concerned, too, the extremities of the fingers or toes, which are uncovered, will always give timely indication of the approach of gangrene.

Again, it has been said that the bandage may produce excoriation and eschars, in consequence of the unyielding nature of its materials, against which the integuments may rub and become ulcerated. When we consider that the soft parts within the bandage remain perfectly immobile, and that the compression is exercised equally at every point, the apparatus being exactly moulded on eminences and depressions, we shall conclude that the occurrence of such accidents must be extremely unfrequent. Besides, the first layer of starch is on the outside of the bandage, and not in contact with the skin, and, even were it otherwise, the inconvenience might easily be avoided by interposing between the bandage and the limb any substance which, without preventing the necessary compression of the parts, would obviate such an inconvenience.

Such are the objections which have been urged against the treatment of simple fractures by this method: against the system, as applied to the treatment of fracture with wound of the integument, a more formidable objection has been made. It has been stated that pus is confined, and that the consequences are extremely injurious. The most conclusive answer which can be made to this objection is, that many hundreds of cases have been treated by this method, and that such consequences rarely occur. When the tumefaction is dissipated, and the period of suppuration has set in, the pus is poured out, penetrates the compresses, and gradually the different parts of the apparatus become impregnated with it. Some time is occupied in the production of this effect, and a multitude of facts clearly demonstrate that, beyond this point, suppuration very rarely proceeds. At first the purulent matter occupies the space left vacant by the subsidence of the tumefaction, but its force of expansion proceeds no further; the compressive power of the apparatus restrains it; there is no reflux, and infiltration of the integuments hardly ever takes place. It is, therefore, a new obstacle to the farther pouring out of purulent matter, which soon undergoes many changes; certain fluid portions are absorbed; and, being preserved from contact with atmospheric air, it does not contract those deleterious qualities which, under the influence of this agent, it usually acquires: the other portion extravasated between the integuments and the apparatus, and which pervades the latter, evaporates, leaving a concrete portion, which forms a stratum around the limb, which singularly adds to the solidity of the apparatus.

Before we proceed further, we will refer to the apparatus of the Arabs; and, certainly, we cannot say less in its favour than that it is a true "immoveable" apparatus, of easy application, more simple and effectual than that of Dieffenbach; little less so than that of Seutin.

Those persons who have no fears with regard to the necessity of providing for the escape of pus, may urge the bad consequences of leaving the wound exposed, and of the less rapid cicatrization; and no doubt the introduction of that principle would be a most important improvement upon the Arab system. Certain it is their apparatus perfectly accomplishes the common indications in treatment; the fragments are maintained motionless, the member is submitted to a uniform compression, variable at will, without shock or disturbance: and, with respect to their arrangement when the fracture is compound, it must be admitted that, when the opportunity of examination is unfrequent, the course they take is the safest.

We will now contrast, very shortly, the two systems of treating fracture of the lower extremity:

The method commonly employed in this country requires a confinement to bed, in the same position, for a period varying between three weeks and as many months. If the patient be in robust health at the commencement of this treatment, it is damaged at the end; if he be debilitated, that debility is aggravated; if he be occupied in trade or any occupation requiring his personal attendance, it must suffer in his absence; if he be a traveller, where he falls there he commonly lies, whatever may be the inconvenience: if the fracture be compound, all these evils are aggravated; the patient may be pulled down by profuse suppuration, or his wound may be exposed to all the accidents to which such a surface is liable. There are also other considerations of much importance which belong to the subject: frequent watching is necessary, and this among the poor in rural districts is frequently impracticable. For instance, a man may be surgeon to a large union of parishes under the present administration of the poor-laws; three or four fractures may happen at different points of that union: it is impossible for him to give them all the necessary attention; and, though the fractured bone may unite, it is often at an angle. Under the other system, the necessary apparatus may be found in a cottage; it is of easy application; it is not irksome to the patient; tumefaction, when present, is more speedily dissipated, and, if it be not already developed, is almost constantly restrained; the patient may, under ordinary circumstances, be left with a conviction that all will go on well; and, when the union is accomplished, instead of being reduced, by confinement, to a debilitated condition, the patient is enabled, on the fourth or sixth day, to leave his bed; the tradesman to attend to his ordinary business; the traveller to proceed on his journey without risk; and, if the fracture be compound, the tendency to suppuration is restrained, and the injury rapidly reduced to one of a simple nature.

It is true that, in Mr. Amesbury's apparatus, we have long had a means of enabling the patient to rise early from his bed; but we have not usually availed ourselves of it; and, for this reason, we apprehend—though at last reason, if opposed to experience, must yield,—that an impression exists, based upon the analogies of wounds of the soft parts, that a depending position is not adapted to early consolidation. But the adoption of this system does not necessitate the depending position for a longer time than is perfectly comfortable to the patient; but it allows of the patient changing his position as often as he may please; rising from his bed, and moving without assistance, and without risk from one room to another. This might, no doubt, have been accomplished by means of Amesbury's apparatus, but it is cumbersome, costly, and much more fatiguing to the patient than that of Seutin.

To those persons who are unconvinced by our statements, we would say, make the experiment yourselves; make it fairly and without prejudice; and do not pronounce judgment without having experimentally tested its correctness.

There yet remains one method of treatment of sufficient importance to require that we should direct attention to it,—the method described by Mayor under the term hyponarthecia. This plan, suggested by M. Sauter, essentially consists in a square or oblong piece of board, longer than the fractured limb, a pillow destined to receive it, cords to be attached to the corners of this board, and a pulley by which it is to be suspended. Upon this apparatus the limb is placed, and secured by means of bandages, so as to form, as it were, one piece with the machine: if the bones are disposed to ride, extension is made, a bandage is applied on either side of the fractured portion to secure it.

In the *Gazette Médicale*, (1835, p. 433, and 1836, p. 187,) may be found propositions to modify this apparatus, made by M. Munaret: he proposes to substitute for the board a metallic gutter, not unlike that adopted by Mr. Liston.

The great object accomplished by these contrivances is to suspend the limb at any required elevation, which might vary with the exigencies of the patient, to admit of the application of dressing, and to enable the patient to be moved. But these desiderata are obtained at considerable sacrifice as compared with the immoveable system; but, compared with the system usually employed in our own country, its advantages are manifest.

So long, however, as the opinion is entertained that the consolidation at the fractured point is the result of an action by means of which a substance is poured out in and around the fractured extremities, and that that process is best accomplished by the most perfect repose, so long must we maintain the opinion that, the more perfectly this end is answered, the more easily and rapidly will the business be performed.*

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* It would seem that, in the time of Cheselden, white of egg was employed by a quack in London, for the purpose of giving firmness to his apparatus.

## ART. III.

1. *Commentatio de Delirio Tremente.* Auctore O. C. HÖEGH-GULDBERG, M.D., Medico Nosocomii Fredericiani secundario.—*Hauniæ*, 1836. 8vo. pp. 293.
*An Essay on Delirium Tremens.* By O. C. HÖEGH-GULDBERG, M.D.—*Copenhagen*, 1836.
2. *Prize Dissertation (for 1831) on Delirium Tremens.* By JAMES CONQUEST CROSS, M.D., of Lexington, Kentucky.—*Albany*, 1832. 8vo. pp. 149.

DELIRIUM TREMENS is one of the many diseases which have found investigators and a name only in more recent years. Before the commencement of the present century, no author had made that series of accurate observations on its symptoms and nature which could enable him to distinguish it from those affections with which, in many respects, there exists a very considerable resemblance. Our countryman, Dr. Samuel Burton Pearson, was the first who called attention to its history; and to him we likewise owe the distinctive epithet by which it was originally characterized. In 1801, and subsequently in 1813, he concisely and very accurately describes the symptoms of this disease under the title of BRAIN FEVER *following intoxication.* On referring to his papers, then published, it is gratifying to see that the treatment he recommended and adopted is almost the same as that which, in the present day, is esteemed to be the most judicious.

The work of Dr. Höegh-Guldberg is essentially a practical work, and is especially valuable from the extensive opportunities he has enjoyed of witnessing cases of this disease. The deductions drawn from the observations he has made, both as regards its nature and treatment, are well worthy of attention.

Dr. Cross's book is one quite of a different description. His opportunities of testing the correctness of his own opinions, and those of others, do not seem to have been extensive; but he has furnished us with a very excellent and comprehensive literary history of the disease: he has placed fully before us the different views which have been entertained by those authors who have written on the subject; and, from having canvassed judiciously their relative merits, has thus presented us with a very copious digest of what is at present known about delirium tremens.

The number of names by which this disease has been designated is evidence sufficient of the difficulty there exists in referring its symptoms to some certain type. The following catalogue, which we extract from Dr. Höegh-Guldberg's work, contains only a part of the synonymy of delirium tremens. Even this bare nomenclature is not devoid of interest. It will be observed that the fancied seat of the disease or its remote cause has furnished many authors with names for it; while others have derived these from the more prominent symptoms.

"Phrenesia potatorum, (Albers); delirium afebrile tremens, (Graff); oenomania, (Rayer); erethismus cerebri abdominalis, (Toepken); delirium vigilans, (Hayward); delirium ebrietatis potatorum, (Hufeland); encephalitis tremefaciens, (Frank); mania a potu, (Nancrede); mania a temulentia, (Klapp); mania potatorum, (Barkhausen); ebriositas sive hallucinatio ebriosorum, (Clarus); dipsomania, (Hufeland); delirium nervosum, (Ryan); erethismus ebriositatis, encephalopathia nervosa, (Leveille); polydipsia ebriosa, (Henke); ecstasis nervosa, (Clarus); delirium nervosum s. traumaticum, (Dupuytren); delirium cum tremore, (Elliotson); brain fever following intoxication, (Pearson); irritative fever of drunkenness, (Tripler); tremulence and delirium from intemperance, (medicus quidam americanus); encephalopathie crapuleuse, (medicus quidam gallicus); denique delirium tremens, (Sutton.)" (p. 6.)

Dr. Cross has entered into some discussion on the relative value of these and other names; but into this we cannot follow him. We entirely agree, however, in the propriety of adopting the term most generally received, viz. *delirium tremens;* although we admit, with him, that it is "highly objectionable." In this, as in other cases, when we consider how impossible it is accurately to comprehend in a name the series of symptoms which constitute to the senses of a physician the disease itself, we had at all times better be contented with an empirical epithet which has from universal consent become established, than involve the subject with fresh difficulties by seeking for another name, which, though it may perchance be somewhat preferable, yet can rarely, if ever, from the diversified nature of disease, be entirely free from objection.

As with the name, there is the greatest difficulty in arriving at any thing like a true and concise definition of this disease. The attempt is, however, hazarded by Dr. Höegh-Guldberg. "Delirium tremens," he says, "is a disease which, in the first place, shows itself by aberrations of the functions of the cerebrum and nerves generally as evinced in *pervigilia*, delirium, hallucinations on some peculiar subject, together with (often) a trembling of the limbs: it is almost always associated with fever, (in which respect, he adds, his observations differ somewhat from those of other writers who have described it;) is very prone to occasion collapse; and is never recovered from but with a critical sleep: it has its principal and almost only cause in a daily abuse of spirits."

Though this definition may in the main be true, yet in many cases it fails of its purport; and there is no disease in which more mischief could arise by tying down one's opinion strictly to the symptoms detailed in a definition. That this disease may have other origin than the habitual abuse of ardent spirits, no one who has enjoyed extensive opportunities of witnessing cases can doubt, any more than that occasionally there is an attendant sthenic fever, now a *tremor artuum*, and now none; and that cases not unfrequently occur where, with all the other worst symptoms, there is no delirium, but only an excitability and restlessness of mind. In fact, a true knowledge of this most opprobrious malady can only be arrived at by studying its symptoms under many different circumstances. We have ourselves witnessed enough of this disease to enable us to conclude that there is no one symptom which is constant or essentially characteristic of its presence. A case came lately within our observation, in the person of a female, whose station in life and ample means should have inspired her with more creditable habits, where the only departures from health were a general irritability of manner and a morbid impression that her legs had become black and were ready to separate from her body: the constant examination of her legs, and her anxiety to draw attention to that supposed condition, were the only external evidence of her labouring under delirium tremens. In another case, which came under our notice, a gentleman of considerable literary attainments, addicted to irregular and occasional excess only, rose suddenly from the head of his own table, where he had been partaking too largely of wine (the only drink of an intoxicating nature he indulged in) in a temporary but very violent phrensy, which quickly subsided into sobs and fears lest his servants should betray his infirmity. In both these instances a critical sleep was the restorer to a healthful state.

When speaking of the etiology of this disease, we think our author a little too strong in his expression when he asserts "that all writers agree in saying that this disease is produced by the abuse of ardent spirits, and that in this abuse both the remote and predisposing causes are to be sought; and, that this assertion is true, daily observation, as well as the confessions of the sick themselves, have confirmed." (p. 9.) That this is by far the most frequent, nay, nearly the only cause of the disease, there can be no doubt; but it most assuredly can by no means be asserted to be exclusively so. Dr. Höegh-Guldberg, in another part of his work, somewhat modifies this statement, by allowing that there are other substances which may produce symptoms having a resemblance, more or less marked, to delirium tremens: "Among the substances which may give rise to symptoms bearing greater or less similitude to this disease may be enumerated various narcotics, as belladonna, stramonium, and especially opium." (p. 18.) There can, however, be no doubt, as above stated, that, although amongst us the chief and almost only cause of this disease is the abuse of ardent spirits, yet that most certainly delirium tremens may have its origin in several other causes; and that in these cases it not only bears "a greater or less similitude" to, but is essentially the disease itself. One reason why it more rarely has its origin in these other substances than in ardent spirits may be the fact of the very general abuse of the latter; while the former are but rarely used, excepting for medical purposes. In contradiction to the statement of Dr. Höegh-Guldberg, we offer the few following instances: Dr. Armstrong gives us the case of a female, who had long been in the habit of taking opium to a considerable extent. On lessening her daily quantity, she became a victim to this disease. Dr. Coates also says he has seen strongly characterized cases, in which it was produced by the intermission of the use of opium. Höring refers to the case of an old man, in whom he witnessed this disease after the long abuse in strong coffee.* Dr. Sutton saw it in a lady, after an habitual indulgence in the use of red lavender.† Dr. Barkhausen, who affirms that he has seen this disease, in a mild form, follow the use of strong beer, doubts if it ever arise from wine. This appears a strange reservation on the part of this well-informed physician. The case of the gentleman to which we have referred is, however, proof to the contrary. It would, no doubt, be difficult to prove, from a great number of cases, that wine is a remote cause of the disease, from its very rarely happening that those who indulge very deeply in vinous drinks altogether refrain from spirit; but there is every analogy to induce us to conclude that an abuse of wine solely may produce it. Dr. Cross justly remarks "that this disease may proceed from wounds, fractures, &c.: the *delirium traumaticum* of M. Dupuytren does not appear essentially unlike *delirium tremens* in its characteristic features;" and Dr. Gregory, in his Practice of Physic, refers to a case he once saw, in which "a distinct affection of the brain, by metastasis of acute rheumatism, assumed the very remarkable character of *delirium tremens.*" These cases, and many more might be quoted, sufficiently show that our author has been too restrictive in limiting the origin of this disease to the abuse of ardent spirits only.

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* Kleinert's Repertorium. Oct. 1833.

† In this instance, however, the disease may be attributed solely to the alcohol with which this tincture is made.

Dr. Höegh-Guldberg, when discussing the remote causes, has omitted the by no means unimportant questions, whether a protracted course of immoderate drinking is always essential to the production of the disease; and whether, as many suppose, there must be a suspension in the use of ardent spirits immediately preceding the attack. With regard to the former of these questions, the general impression seems to be that a long course of dissipation is necessary to cause an attack; and, although for the most part this certainly is the case, yet there can be doubt it does not always hold good. In persons of irritable temperament, and liable to præcordial anxiety, a single excess is sufficient to produce it; but then the disease, though it may be as acute at the onset, is not of the same continuance or fatal tendency as when preceded by a debilitating course of dissipation. It also occasionally occurs in those individuals who, never drinking to that excess which produces drunkenness, are frequently in the habit of allaying a constant thirstiness by stimulating drinks. Dr. Barkhausen says that he has seen it "in those who have seldom, or perhaps never, become intoxicated." We think we have seen the same; and therefore conclude that this disease is not wholly to be attributed to the quantity of spirits drunk at one time, or to the period in which the habit has been indulged in: constitution and other circumstances must also be taken into consideration.

> "The natural strength and irritability of the constitution," says Dr. Cross, "will always exercise great influence in modifying the result. Thus, when there is but little constitutional strength and considerable nervous irritability, ardent spirits will produce much higher excitement than where the circumstances of the individual are directly the reverse. If this be true, the legitimate conclusion is that delirium tremens will be developed with the least promptitude in the constitution which most reluctantly comes under the influence of unnatural or spirituous stimulation." (p. 20.)

With regard to the second question, as to whether it is necessary to the production of the disease that the use of ardent spirits should be suspended, we can have no hesitation in stating, from personal observation, that it is not requisite; though undoubtedly the attack is most usually preceded by a state of depression resulting from the temporary discontinuance of the wonted stimulus.

One of the most valuable parts of Dr. Höegh-Guldberg's essay is the deductions from observations made upon 173 cases of this disease, which came under his own observation. From these we are enabled to see the relative influence of age, sex, and season. Dr. Bang, Lind, and Rayer appear to think that the disease occurs more frequently between the age of thirty and fifty; whereas, our author limits its greatest frequency to the period between forty and fifty. He witnessed it but once in a young man of twenty-two years of age, and but once in an old man of seventy. With regard to its frequency in women, Rayer observed it in seven women out of 176 cases. Bang states that, in 456 cases which he treated, only ten were females; and from the tables of our author we find but one case out of 173 occurring in a female. Krüger-Hansen in sixteen cases, and the chief physician of the hospital Christiana in eleven cases, met with no instance in a female. These statements differ very much from the accounts given in this country; for we find both Dr. Ryan and Dr. Roots state that the sexes are equally obnoxious to its influence. This discrepancy can only be explained by supposing that the indulging in intoxicating drinks amongst females is carried to a greater extent in this country than in the north of Europe.

With regard to the influence of season, the month of May appears to be the time when attacks are most frequent. Dr. Höegh-Guldberg observed in this month twice as many cases as in any other month. This does not altogether accord with the observations of Bang, who states that in June and July cases are as frequent as in May, but that the mass of cases occur during these months. It appears evident from the above, as well as from the following facts, that atmospheric phenomena exert a considerable influence over this disease. Between the years 1826 and 1829, of nine thousand sick in the hospital at Frederickstadt, 456 were cases of delirium tremens; while, in the period from 1830 to 1832, amongst seven thousand patients in the same hospital, only 173 were cases of this disease.

From a synoptical view of the 173 cases above mentioned, given at the end of the work, we obtain some other statistical results of importance. Thus we learn that, of this number, 149 were cured, (twenty-two of these not having undergone any curative means but quiet,) and twenty-two died; that the critical sleep occurred in the greater number on the fourth day; that the period of life most obnoxious to the attack was from the fortieth to the fiftieth years; that it occurred mostly in street porters, literary men, and tavern keepers; that, in 159 cases, it was, without doubt, brought on by the abuse of spirits, and most probably so in the remaining fourteen; that, of those who took opium, eighty-three were cured, and thirteen died; that, of those who were bled, eleven recovered, and seven died; and that a deposit of lymph between the membranes of the brain was the most usual appearance after death. In another part of the volume, a very full account is given of the appearances on dissection, occupying a space of thirty pages: but we do not see that the observations essentially differ from what had previously been noted, nor do they tend to throw any new light upon the pathology of the disease.

In the history of the symptoms and general character of delirium tremens, Dr. Höegh-Guldberg very properly draws a distinction between those cases which occur in otherwise healthy persons, the disease having its origin solely in the abuse of spirits, and those cases where it is complicated with other lesions, either internal or external. The sad condition to which the state of health is reduced in the confirmed drunkard is ably summed up by Dr. Höegh-Guldberg, in the history of the precursory signs.

"An almost constant precursor of this disease is great disturbance of the digestive organs; the long indulging in the abuse of ardent spirits has destroyed the appetite for food; a mucous and nauseous taste is felt in the mouth; the food which is taken in is often rejected, especially in the morning; oppression and tension of the epigastrium are felt, with flatulence and general anxiety; which symptoms, though they may be for a time allayed by taking spirits or bitters, progressively increase. At length the temporary relief afforded by these means is worn out, or else a dislike is taken to them. Then the gastric evils become intolerable; vomiting supervenes, and mucous matter, sometimes tinged with bile, and more rarely with blood, is evacuated: this state is often accompanied by a torminous diarrhœa. Occasionally the patient is attacked by epileptic fits; and some medical men have so constantly observed these to take place, that they have been convinced that, within twenty-four hours, an attack of the disease would commence: sometimes an epistaxis happens, so considerable as, if neglected, to continue for many days; sleep begins to leave the patient's eyelids; his mind undergoes a complete change; his ordinary business is less pleasing to him; he becomes morose, irritable, and obstinate, impatient of almost every contradiction, especially from those with whom he is familiar; while the anxiety which he suffers he desires and uses every endeavour, but in vain, to conceal from the observation of others." (p. 27.)

Although this account of the dilapidated health caused by a long series of dissipation is very correct, yet we are convinced that such a condition cannot be called precursory to an attack of delirium tremens; for, however susceptible of this disease a person reduced to such a state may be, yet it is by no means a consequence that an attack should follow; and, on the other hand, many who come within the influence of the disease have not been previously sickened down to any such lamentable extremity. We by no means desire, however, to take from the force of the above statement that such a condition is in every way likely to predispose to the disease. The remark that epileptic fits are constantly and immediately followed by an attack of delirium tremens, is no doubt founded in error; for we have not unfrequently seen these convulsions produced in the drunkard, and that, too, in the most aggravated and distressing form, without the severer disease supervening.

"In the first stage, the precursors are mingled with the disease itself. Though still of sound mind, yet the patient complains bitterly of a troubled sleep: scarcely are his eyes closed when a whole host of phantasies obtrude upon him: he seems to hear various sounds, the causes of which, rising up with fright, he at first vainly endeavours to discover: at one time he thinks them to be in his own ear, and then that they proceed from a distant spot; sometimes he attributes them to the agency of demons, which seem to blow air into his ears; and yet, singular to say, he is sensible of these errors of his senses. If he be roused from sleep, in the first moment he knows not where he is, or to whom he speaks: he complains of vertigo, a feeling of weight in the head, and loss of memory; and an inconstancy and unsteadiness are evident in his thoughts and words, and in all the voluntary motions. In this early stage of the disease, the patient accommodates himself, to a certain extent, to the will of others: sometimes he makes replies so apt to the questions put to him, that the existence of the disease might be mistaken, were it not for a certain inconstant expression of countenance, a silly distraction of mind, or perhaps from a sudden and unexpected motion, by which he endeavours to seize a thread or insect, the phantasms of his own imagination, which he believes to be affixed to the floor or bedclothes. If he be asked what he sees, or what he endeavours to lay hold of, he will tell you; adding, that he was in error; and then, for some little time, his replies are sufficiently apposite and coherent. At the request of his physician he sometimes goes to his bed, and for a short period composes himself for sleep; then he rises, and seeks for one thing or another among the bedclothes; or he replies to something he imagines to be addressed to him." (p. 29.)

In this first stage the patient may be permitted, with proper care, to walk about; and it may happen that the symptoms do not further increase: this, however, is seldom the case. Should he not be refreshed by sleep, the disease will certainly increase, especially towards night, when hallucinations of the senses, delirium, and trembling of the limbs supervene; in fact, all the essential symptoms of the confirmed disease. This is the stage which our author terms the *incrementum morbi.*

> "The restless hilarity of the patient now increases: he talks much, and to questions often gives sufficiently apt and even witty replies; then, after a little time, forgetful of the tenor of his discourse, he answers questions made to others, or does not understand those addressed to himself. It is not unfrequently a source of much fear to him that his medical assistant may not effect that which he desires. He is irascible, angry, or even furious; yet, for the most part, if not strongly contradicted, he may still be controlled. During his hallucinations, he almost always thinks himself to be engaged in his habitual pursuits. The postboy is driving his horses—he cheers them, he beats them; the servant appears to hear the voice of accustomed command, and, mistaking some other individual for his master, offers him his services; the watchman calls the hours; the sailor works his ship; the porter endeavours to move from its place the table, bed, &c. This restlessness continues to increase with the delirium, until, exhausted by exertion, and covered with perspiration, the poor victim subsides into a temporary quiet. It more rarely occurs, though many such cases are not wanting, that the mind becomes overwhelmed with melancholy, with religious forebodings, &c. These people do nothing, or else seem unable to do what they desire; or they wander up and down the room without purpose, or seek to hide themselves from importunate creditors or the officers of public justice. If at this time they be suffering from any local pain, all anxiety and attention is referred to the spot, and it has sometimes been observed that they attach to themselves any pain they may at the time hear complained of. Barkhausen gives the case of a man who supposed himself pregnant, from hearing his wife's complaints during labour. The patient's mind is taken up occasionally with the strong desire of ardent spirits; his joy is raised because he thinks it is before him; he asks for it with eagerness, and the cup offered to him is not refused; but his taste is often lost; whatever be in the cup he drinks it, believing it to be spirits; at another time, however, he is not thus deceived." (p. 32.)

Very frequently the sense of sight is greatly depraved, the hallucinations being extremely diversified, yet for the most part more or less referable to certain forms. The cause of these Dr. Höegh-Guldberg thinks to be congestion of the capillary vessels in the transparent parts of the eye. The sense of hearing is also particularly affected: imaginary conversations are heard, in which the patient joins; spirits laugh, and the patient emulates their mirth; tempests roar, dogs howl, &c. &c. The only sense which is unaffected is that of touch.

In defining the commencement of the third stage, Dr. Höegh-Guldberg follows Göden, who says this stage commences when the patient loses *all* consciousness of his past and present condition.

> "He knows not where he is, or to whom he speaks; altogether inattentive to external objects, he follows out his own ideas; the delirium is constant and violent; the expression of the countenance is fierce and immoveable; the eyes are closed, the lids being frequently covered by an acrid secretion; the conjunctiva both of the ball and lids is inflamed; the pupil, which was before dilated, becomes contracted, and seems scarcely sensible of light. To one unaccustomed to witness this disease, the strength would appear exhausted; but, on the advent of a paroxysm, it is so considerable as to require much power to control it. Sometimes the tremors of the limbs pass into a spasmodic rigor of the whole body, and the patient is suddenly prostrated on the ground, without the power of rising; but he soon recovers, and resumes his previous state of restless occupation. This stage may last from one to six days." (p. 38.)

The fourth stage is computed from that time when frequent remissions, lassitude, and prostration of strength supervene, and can only be said to have fairly set in when sleep overtakes the exhausted patient: sleep is the only natural and salutary crisis in this disease. It must be particularly noticed that this condition is not sudden in coming on: for some time manifestations of a sleepiness are present, together with an increasing tranquillity both of mind and body; but yet some space elapses before the patient is perfectly free from hallucinations: at length, however, he gradually passes into a state of repose, which ends in a true sleep, the prelude of a speedy, sometimes of an immediate recovery. If, however, in place of this critical sleep, the restlessness continue, or if the patient be roused up with tremors and convulsions, we are justified in expecting a speedily fatal termination.

The critical sleep is a subject worthy of much attention: it frequently continues for twenty-two or twenty-four hours, and in some cases even for thirty-six and thirty-eight; only being disturbed by natural causes. A sleep of only six or eight hours is, however, frequently critical, being followed by recovery.

Whether delirium tremens is to be considered as of a febrile nature or not, has been the cause of much dispute. The latter opinion has been maintained by Göden, Blake, Dupuytren, and others; while Coates, Barkhausen, and Armstrong, as also Dr. Höegh-Guldberg, hold to the former. He says he has never known the pulse less than eighty or ninety, more frequently upwards of one hundred; but, in the generality of cases, 110 or 120, and not unfrequently 140.

The following are some of Dr. H.-G.'s observations on the symptom which gives its name to the disease:

> "*Tremor artuum* generally happens amongst all drunkards, even before they are attacked by this disease; especially if a life of habitual dissipation has undermined the constitution. That it sometimes is not present cannot be denied, but the exceptions are only met with in young or robust men, who, from some accidental cause or another, have become affected with this disease before the constitution has been destroyed by habitual drunkenness. For the most part, the hands are the parts which are affected with the greatest tremor; though not infrequently the arms, and occasionally even the whole body, suffers. In some cases the tremor becomes so considerable that the patient is unable to lay hold of small objects; nor can he raise, without spilling the contents, a cup to his mouth. In the more aggravated cases, it prevents his walking steadily unless supported by others; if he rise from his seat, he totters, and the eagerness with which he endeavours to restore his lost equilibrium, and the strenuous efforts made to grasp some object for support, only increase the tremor. As the disease advances, the organs of speech become affected; the tongue being, contrary to the will, thrust out, or retracted or fixed between the teeth." (p. 52,)

Under the head of *Delirium tremens chronicum*, Dr. H.-G. well describes that chronic condition of imbecility of mind and prostration of body to which the habitual drunkard becomes liable. In the following chapters, which treat of the Prognosis and the Diagnosis, we also find much valuable matter; but we pass over these to notice the more important subject of the Treatment. Dr. H.-G. is of opinion that much time has been unnecessarily consumed in fixing on the indications of treatment. He thinks it sufficient if it be borne in mind that the first object to be obtained is the critical sleep; "for, whether the physician select the antifebrile or antiphlogistic mode of cure, or the exciting or sedative, he does nothing else, nor is anything else to be done, than prepare the way for sleep, by lulling the disturbed state of the vascular and nervous systems." It sometimes happens that recovery from this disease takes place spontaneously; but the result of such rare cases is only to impress upon the mind the advantage of treatment. Dr. H.-G. says that he has seen the best effects from a cool regimen, spare diet, internal refrigerants, and the application of cold lotions or cold affusions to the head. In the sthenic variety he prefers the use of the lancet, observing that in this form, which for the most part occurs in robust young men, and after excesses newly committed, local congestions are to be especially guarded against.

He is persuaded that this form of delirium tremens is, in fact, a species of arachnitis; and that the congestion in the brain, attended, as it is in young and robust men, with a full, strong, and tense pulse, is of such pressing moment as to indicate immediate bloodletting. This practice, he adds, can however be only safely pursued in those whose health has not been weakened by the occurrence of previous attacks or a debilitating course of dissipation, or in whom the disease has existed but for a short period; for, in the latter stages of the disease, when there is always great debility and a tendency to sudden collapse, it must be instituted only with much caution, or altogether avoided.

When venesection is proper, he recommends great circumspection as necessary in regard to the quantity to be abstracted; observing, that he had ever found twelve ounces sufficient, and that he never repeated the operation unless there remained unaltered some marked sthenic character of the disease. He has frequently observed the symptoms remit so quickly after this mode of treatment, that the critical sleep has supervened within an hour. After passing in review the names and opinions of many of those who have either maintained the efficacy or have entirely condemned the use of the lancet in this malady, he sums up by saying that the middle course is to be pursued, and that these latter are in as great error as those that commend its promiscuous use. Local bloodlettings, either by leeches or by cupping, he says, may be employed with less reserve than general bleeding. They may be resorted to at the commencement of the disease, if such indications as have just been referred to, when speaking of general bleeding, be present; as likewise in the later stages of the disease, should symptoms of congestion in one or other of the organs of the principal cavities manifest themselves, and when venesection is precluded. The use of local bloodletting is particularly indicated if heat or congestion of the head, attended by vertigo, singing in the ears, and redness of the eyes, continue.

Dr. H.-G. is particularly partial to the employment of cold applications and affusion, the use of which is indicated, in young and strong persons, by the presence of urgent and continued congestion of the brain, with redness and heat of face. He says there does not appear to be any better remedy for controlling the continually rising hallucinations and phantasies, which unbidden crowd on those afflicted with delirium tremens, in the first stage of the disease; but he entirely precludes their employment in the subsequent stages, and says that, when used at such times, he has seen universaltremors occur, occasionally so considerable as to similate convulsive motions. Lind, Barkhausen, Burrow, Armstrong, Albers, and others, who have found the use of cold affusion serviceable, caution us against its employment if any profuse perspiration be present, recommending in such cases the employment of the tepid affusion. Dr. H.-G. has, however, no reserve of this kind, but adds further, that such cases very rarely occur; for, in the stage of the disease in which cold affusion is serviceable, or is indeed only practicable, the head is dry and hot, and rarely if ever suffused with perspiration. He has observed that the mind is somewhat steadied, or even recovered, after every affusion; and therefore recommends that this remedy should be repeated frequently, (every two hours,) and so continued until the head becomes perfectly cooled.

Following Horn and Richter, Dr. H.-G. has tried cold affusion to the head while the patient has been immersed in the tepid bath, and not without success; but this practice is almost precluded by the difficulty which attends the carrying it into effect; for those affected by delirium tremens almost universally resist the tepid bath. Richter, however, affirms that its efficacy is so great that he has frequently found the use of the bath, together with the employment of nauseating medicines, effect a recovery within a few hours. Cold applications to the head may be applied after the cold affusion, though it rarely happens that the patient is sufficiently tranquil to render this course practicable: should it be so, however, they are very useful. Their efficacy may be increased by their being mixed with slight derivatives, as mustard: the stronger rubefacients and vesicatories are most useful in the asthenic form of the disease, and should not be used in the sthenic, unless to overcome some dullness of the mental faculties, which might otherwise become permanent.

Of the use of laxatives, Dr. H.-G. has had but little experience: in those cases, however, in which they were employed, the sulphate of magnesia was resorted to. He makes the observation that delirium tremens frequently occurs in those labouring under diarrhœa, and that such condition of the system does not alleviate the delirium.

In about half the cases that came under his care he administered the nitrate of potass; not, he says, because it is endowed with any specific power of controlling this disease, and in which other cooling medicines are deficient, but that it is a febrifuge which he has been accustomed to use unless contraindicated. He believes it to be only useful at the commencement of the disease, and quite inefficacious if the disease have reached its acme.

Of the supertartrate of potass and sal ammonia the same may be said.

The frequent complication of delirium tremens, during one year, with typhus, induced him to try the effect of acids diluted by water or spirit; a remedy much praised by Albers and Barkhausen. He found it successful at the commencement as well as in the latter stages of the disease, and recommends it as being greatly serviceable when delirium is superadded to wandering and restlessness of mind, and likewise when the thirst is more intense, and the sweatings greater, and the tremor more violent than is usual in the sthenic variety of the disease. When symptoms such as these manifest themselves, as is not unfrequently the case, five or six drops of Haller's elixir[*] he often found restore the tranquillity, and in some measure subdue the *tremor artuum*. He recommends, in order to satisfy the morbid fancies of the patient, that medicines should be administered in a cup similar to that from which he has been in the habit of drinking his intoxicating draughts.

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[*] ℞. Acid. Sulph. dil. ʒj.; Aquæ Ceras. nig. ℥iv.; Syr. Rub. Id. ℥j. M. Radius Heilformeln, p. 14. Leipzig, 1836.—Ed.

On examinations after death, the frequent appearance of inflammation and the formation of lymph on the membranes of the brain, together with the recommendations of Lind, Armstrong, Gunther, and others, induced him to try the effect of calomel in "accustomed doses, and according to accustomed rules," (two grains every alternate hour.) This he did, however, in those cases only in which the delirium tremens was complicated with inflammations of the chest, and in which cases he found it very useful.

Dr. H.-G. canvasses at some length that mode of treatment by emetics so much recommended by Stoll, Göden, the American physicians, and others. The doses of tartar emetic which it has been the custom of some of these to administer appear, indeed, surprising to the British practitioner; for we find Stoll giving from four to seven grains, and Sperce (Spence?) administered the enormous doses of thirty grains, which he repeated every hour, and even every half hour. This last writer assures us that the effects of these doses were an immediate lassitude, a slight vomiting and diarrhœa, tranquillity of body and mind, and then the critical sleep. Dr. H.-G. never prescribed it to an extent anything like this.

It has been asserted that, after the injudicious and continued use of tartar emetic, vesicles are formed in the cavity of the mouth: these Dr. H.-G. has never seen, although, during the year preceding that in which he wrote, large numbers of cases have been treated by this medicine; but he has often seen on the tongue foul and deep ulcers, caused most probably by its being bitten during the epileptic fits which so often precede attacks of delirium tremens. He also says he has never noticed, after the use of tartar emetic, though he has employed it in gastric and bilious fevers and in affections of the chest, those inflammations of the membranes of the bowels which have been observed by Orfila, Dupuytren, and Trousseau; yet that, having no doubt of their occurrence, he was induced to be very cautious in the use of this medicine. After general or local bloodletting, he administered half a grain in solution: if this did not produce nausea, he increased it to a grain, and then to a grain and a half: and this treatment he continued for two or three days, if the excitement of the nervous and vascular systems did not yield. The greatest amount of tartar emetic he ever administered during the whole course of the disease was forty-three grains. The usual effects which he observed to follow its use were vomiting, diarrhœa, and depression for two or three days. Take it altogether, he does not approve of it, and thinks it unworthy of the praises which have been bestowed upon it. He also tried ipecacuanha, and ipecacuanha combined with tartar emetic; but did not find that their use was attended by any very marked beneficial results. In one case, however, after employing bloodletting, nauseating and other medicines, for some days without avail, he administered the tartar emetic in conjunction with the infusion of digitalis, and they were responded to beyond all hope. The patient, before ungovernable, became tractable, sought his bed, and fell into the critical sleep. The tartar emetic, combined with nervous medicines, as valerian, &c., he found useful in the later stages of the disease.

In the asthenic, that is, the more common form of the disease, opium is, with Dr. H.-G., as with us, the main remedy. After canvassing the various opinions and practice of other authors respecting its employment, he informs us that his own observations have led him to conclude that, although it is a safe and most valuable remedy, it should be used with more caution than is wont to be the case. Though he very commonly administered it (in about half the cases treated), he yet gave it in no one instance to a greater extent than forty-eight grains during the whole course of the disease. The more usual quantity, and that which he preferred, varied from two to ten grains. He says there is no particular preparation required before administering opium. The white mucous tongue, the flatulence, and the oppression at the cardia, to which drunkards are liable, and the morning vomitings under which they labour, often for many months, do not contraindicate its use. Should there, however, exist any severe gastric affection, or any congestion, either general or local, means must first be resorted to for their removal: but such condition belongs rather to the first stage of the disease, a period when the employment of opium is improper. He states the fit time for its exhibition to be the beginning of the third stage, which is characterized by the loss of present and former consciousness. If, on using it, symptoms of apoplexy supervene, as coma, stupor, difficult or stertorous breathing, &c., it must, of course, be discontinued.

He makes some few observations upon the relative value of many other remedies, as valerian, camphor, musk, &c.; but, as these are but of very secondary importance, we pass them over without further comment; as likewise the observations upon the treatment of the complicated and chronic forms of this disease.

Before concluding, we must again turn for a moment to Dr. Cross's essay. We have already characterized it as being an excellent digest of the facts, views, and opinions of others; and we regard the conclusions he has deduced from them as, for the most part, fair and unexceptionable. But the work is throughout disfigured by a very dogmatic tone, and displays frequent passages of the absurdest grandiloquence and bad taste. We quote a few specimens, taken at random, from two or three adjoining pages:

> "In the language of Demosthenes, the cause of medical science might have been saved an Iliad of misfortunes, had physicians been more temperate in the commendation and denunciation of this Herculean remedy. Like woman, however, they are rarely mindful of the advice given by Phœbus to Phaeton: *in medio tutissimus ibis.*" (p. 81.) . . . . "If, therefore, in the succeeding pages, independence should characterize the expression of our practical opinions, the reader should not ascribe it to an overweening self-confidence, but to the irresistible conviction of their truth. Should the author sacrifice the sensibilities of the heart, and disregard the dictates of the head, in order to float buoyantly and gracefully along upon the *aura popularis?*" &c. &c. (p. 82.)
>
> "By the pulse in this variety it is unsafe to be governed: it will not be found as faithful a guide as Great Heart in Pilgrim's Progress. On the contrary, it is in this variety too frequently a perfect *ignis fatuus.*" (p. 87.)

## Art. IV.

*An Experimental Enquiry into the Physiology of Cutaneous Absorption, and its Application to Therapeutics, &c.* By W. H. Madden, m.d.—*Edinburgh*, 1838. 8vo. pp. 151.

It cannot be doubted that the rewards which have, within the last few years, been offered at some of our medical schools and universities for proficiency in the various branches of medical study, have, among a certain class of their students, been productive of most beneficial results. The volume before us is one among those results. If we turn through a pile of the theses which, either in accordance with the rules of the university or the vanity of early authorship, have been given to the world, we find, with an occasional exception, productions which clearly proclaim the bare necessity or vanity which conceived and gave them birth. The present essay, "that to which the Medical Faculty of the University of Edinburgh awarded a gold medal, at the graduation on the 1st of August, 1837," is a very gratifying evidence of the discretion of the body which established the reward. It is written by one who has acquired a full knowledge of the subject of which he treats; in an excellent style; with a clear and systematic arrangement; and with a cautiousness of expression towards those whose opinions he has found reason to oppose, with which, in a young author, it is extremely agreeable to meet. There has been long known a large class of facts, both occurring in the healthy and unhealthy states of the body, which tend strongly to favour the idea of an absorbent quality in the skin. Such are the effects of the local application of medicine; the relief afforded to great thirst by external moisture; the apparently nutritive effect of baths occasionally employed when other means of conveying nourishment into the system were inapplicable, and the like: and numerous direct experiments, such as those of Dr. Young and many other physiologists, have afforded positive evidence of a kind scarcely to be contradicted. The contradiction has, however, been given; and we, therefore, consider experiments as valuable, which, although they contain nothing in themselves very novel, and lead to no new conclusions, add a mass of new facts to one side of the question; and determine, with all the evidence which the case admits, that which is the true side. That Dr. Madden has done so much; we think will be admitted; and he has, besides, examined with great care, and with much success, the experiments on which inferences opposite to those which he has himself been led to adopt repose; showing that some of them, at least, lead to conclusions contrary to those which their authors derived from them. In the following pages, our object will mainly be to give an analysis of the essay before us; the opinions which it contains being so much in accordance with our own that we have scarcely an objection to offer to them.

The thesis is divided into four parts: 1, The Anatomy of the Skin; 2, Absorption from its Uninjured Surface; 3, Endermic Absorption; 4, The Agents by which Absorption is Effected, and the Application of the Subject to Therapeutics.

In the first chapter of the First Part, it has been the object of the author to point out the analogy of structure of the skin of the lower classes of animals with that of man; from whence he infers that "the striking similarity which we have seen to exist in these organs, throughout the animal kingdom, must be a powerful collateral evidence in favour of cutaneous absorption, since even its most determined opponents are compelled to admit that its existence in the lower tribes is unquestionable." Whatever argument can be founded on analogy of structure we agree with Dr. Madden in considering legitimate in the case before us.

Chapter ii. is devoted to the *Anatomy of the Dermis.* We greatly regret that, in an essay of such considerable merit as the one before us, we should have to charge its author with unfair dealing towards ourselves. This chapter is mainly founded on the researches of Breschet and Gurlt. In our second volume, after a very careful examination of their separate works, as well as of the discoveries of others in the same field of enquiry, we arrived at certain conclusions which we think have been confirmed by subsequent experience. It may be considered as asserting too much to say that our conclusions have been adopted without any acknowledgment by Dr. Madden; but such an inference is warranted when we find that, without the slightest allusion to the source whence they have been derived, our quotations have been appropriated, and in the very form in which we had abridged them from the original works.*

By his own examination, the author has satisfied himself of the existence of the sudorific apparatus described by Breschet and Gurlt. He is not satisfied as to the existence of vessels in the epidermis, from the extreme difficulty of coming to any certain conclusions from the appearance of this substance beneath the microscope. He does not appear to be aware of the existence of injected preparations of the epidermis; and if they are not to be regarded as proofs of its vascularity, we know not on what evidence the vascularity of mucous membranes can rest. To these preparations we have alluded, and from personal observation, in the article already referred to. Some recent examinations of cuticle by Dr. Henle have disclosed its intimate structure, apparently, with great minuteness. These will be new to many of our readers.

"The scales in the external parts of the cuticle have a most irregular form, being subrotund, angular, polyedral, and frequently with imperfect or lacerated margins, but all with flat surfaces, frequently bent, and so thin that the microscope discovers innumerable quantities of them, in the smallest particle which the naked eye can see. In these it is rarely possible to discover any traces of a central spot. But from considering the manner in which the cuticle grows, from examining this membrane as it exists in the embryo, and attending to its structure in the adult, where in close apposition to the skin, or where naturally thin and delicate, as on the glans penis and prepuce, Dr. Henle has been led to believe that such are not the regular forms. For in all these situations, he found the scales thicker, and bearing the appearance of cells, with straight margins, generally of a pentagonal form and all provided with a central nucleus. This nucleus, which is in the centre of each cell, has a smooth or slightly granulated surface, is subrotund or oval, sometimes appears to contain in it lesser granules, and is thicker than the cell, so that it projects on each side. The so called rete Malpighii consists of a similar structure, with this exception, that the nuclei of the same size as in other parts, occupy almost the whole of the cells." . . . [The investigations of Dr. Sharpey and of the author confirm the above statements.] . . . "In the epithelium of mucous membranes, which, as will be seen immediately, possesses a structure closely resembling that of the epidermis, the cells are united by means of a delicate net-work of some substance, the precise nature of which is as yet unknown, and we may therefore reasonably conclude that the same means of connexion also exist in the parts we are now considering." (p. 28-29.)

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* In confirmation of what we have above said, we refer the reader to pages 17 and 25 of Dr. Madden's Thesis, and to Vol. II. pages 435, 436, 441, of the British and Foreign Medical Review.

In favour of the cuticle being an organized substance, the author alludes to its definite structure, as above shown; to its differences in different parts, according to the specific purposes it is destined to fulfil; to the fact of its being the seat of a disease to which Dr. Wallace applied the term *morulus*, and in which the true skin is unaltered; and to its resisting chemical reagents which act upon it immediately that it is separated from its connexion with the body. Its permeability by fluids is regarded as established by the various recorded facts of the transmission of fluids, such as ink, spirit of turpentine, &c.; and by what is observed when the feet have been for some time soaked in warm water, the thick cuticle of the sole becoming whitened and opaque, and allowing a quantity of fluid to exude if pressure is then made upon it. The analogy between skin and mucous membranes has generally been maintained.

> "In the conjunctiva and the mucous membrane lining the mouth and œsophagus the scales and cells present the exact resemblance of those which we have already described as existing in the skin. At the cardiac orifice of the stomach, these cells disappear, and are replaced by cylindrical bodies. They again reappear in the body of the stomach, and are finally lost at the pylorus: the whole remaining extent of the intestinal mucous membrane being covered by cylindrical or cuneiform bodies, united by their sides and fixed to the subjacent tissues by their apices. These cylinders are connected by a net-work of some unknown substance, and have often, though not always, the central nucleus. In the air passages, the vibratory cilia are placed upon analogous cylinders, an additional proof, if such were needed, that they are truly organized, for although it has appeared probable to many that a layer of inorganic matter should cover the surface of a living tissue, few, I apprehend, would be inclined to hold the same opinion with regard to one which was interposed between two living organized structures, and such are both the mucous membrane and the vibratory cilia." (p. 34.)

The analogy shown to exist between the structure of the skin and mucous membranes must be regarded as in some degree justifying the inference of similarity of function.

The second division of Dr. Madden's essay is devoted to the consideration of *Absorption from the Uninjured Skin*. In Chap. I., which treats of "*Absorption in the Bath,*" are considered the effects of baths, both general and local, of pure water and of water containing nutrient matters. The importance of this enquiry is justly regarded as enhanced by the fact that the opponents of cutaneous absorption have regarded the experiments performed in the warm bath as the most conclusive in favour of their opinions. We shall pass over the author's account of early opinions and experiments on the subject, as, from not having guarded against certain sources of fallacy, and from not having conducted them with sufficient minuteness, experimenters have drawn inferences which have been rendered inconclusive by subsequent experience. Some recent experiments of Collard de Martigny, in which all sources of fallacy appear to have been guarded against, tend strongly to establish the fact of cutaneous absorption.

> "By means of a very sensible balance, capable of turning with a quarter of a grain, he carefully weighed a handkerchief. Removing this, he carefully placed in each scale

a china vessel of the same diameter, established an equilibrium between them, and then poured into both an equal quantity of tepid water. One of the vessels was then removed from the scale, and replaced by weights equal to the other, and thus the quantity of water employed was determined. These circumstances were all carefully noted. The weight of the handkerchief and of the water being thus known, and a perfect equilibrium established between the two vessels, he plunged his arms up to the elbows in one, leaving the other untouched, remained thus for half an hour, then dried his arms with the handkerchief and weighed it immediately. Having now replaced the vessels under precisely the same circumstances as before, he found the one in which his arms had been immersed, considerably lighter, and accordingly added weight to restore the equilibrium. The experiment was performed six hours and a half after taking food, the air was cold and dry, the day serene, and the temperature of the water, 74° F. It gave the following results:

| | | |
|---|---|---|
| " Weight of the handkerchief | before, | ℥ij. + gr. xiv. |
| | after, | ℥ij. + gr. xl. |
| Increase | | gr. xxvi. |
| Weight of each vessel full of water in equilibrium, | before, | ℥xij. + ʒiv. + gr. xxiv. |
| Weight of lightest, | after | ℥xij. + ʒij. + gr. lxvss. |
| Total loss | | gr. 104½ |
| Water absorbed by handkerchief | | gr. 26 |
| Absolute loss | | gr. 78½." |

(p. 40.)

The following experiment we select from among others by Collard, tending to establish the fact of cutaneous absorption from the hand:

> "He took a tube of glass, bent like a syphon, and widened into a funnel at one extremity; some mercury was placed in the bend, and the limb near the funnel was filled with water. The palm of the hand was then applied to it, and retained there for the space of two hours, at the expiration of which period the column of mercury had risen considerably towards the hand, thus clearly indicating the absorption of some part of the water. Milk, tried in the same way, was absorbed less quickly, soup more rapidly than water." (p. 41.)

Supposing these experiments to be strictly accurate, we can see no other inference to be derived from them, than that an absorbing power was possessed by those parts of the skin with which the fluid came in contact. They appear to have been well devised, carefully and minutely conducted, and with a knowledge and an avoidance of the ordinary sources of mistake. The experiments which Dr. Madden has himself performed on this part of the subject are justly regarded by him as strongly corroborating the above-mentioned testimony; but, as they add nothing to the strength of this, and as they are objectionable (and the first four of them in particular) on the ground that there was no absolute certainty that the arm was always immersed to exactly the same degree in the fluid, we shall pass them over, particularly as we must give more at length Dr. Madden's observations on the effect of *general baths.* The ancients appear to have placed considerable reliance on the effects, both of medicinal and nutritive fluids, when applied to the skin; and there are very numerous facts which afford a certain degree of foundation for the justness of this opinion. Various physiologists have laboured, with various success, to establish or to refute the doctrine of absorption from the whole surface: and, among the opponents of the doctrine, Dr. Madden refers especially to Séguin, the results of whose experiments are often referred to as conclusive against cutaneous absorption; but which results he regards as far from justifying such a conclusion. Our author's remarks on this subject are very valuable, and we shall therefore condense them. After thorough washing, Séguin was weighed in the air, and the process was repeated after a certain interval, the whole loss of weight being divided by the number of minutes during which the experiment lasted, the quotient indicated the mean loss for every minute. Immediately afterwards he entered the bath, and, at the end of three or four hours, was again weighed, and a similar calculation instituted. The results of thirty-three experiments, thus performed, showed, 1, that in no case was the weight increased during immersion; 2, that a little less was lost in the bath than in the air, but that this diminution varied much according to particular circumstances, especially according to the greater or less temperature of the water; 3, that, at a pressure of twenty-eight inches of mercury, the loss of weight in the water was, to that in the air, in the proportion of 6.5 to 17, provided the temperature of both media was not above 10° or 12° Reaumur; 4, that, under the same barometric pressure, the proportion is as 7.5 to 21.7, the temperature being from 15° to 18°, and, as 13 to 23, when the temperature was so high as 26° or 28° R.

Now, it is necessary to remember that the contact of water does not prevent the process of exhalation. The experiments of Edwards are referred to as conclusive on this point. He found that cold-blooded animals, when confined in air saturated with moisture, and at the same temperature as their bodies, and when, consequently, no perspiration by evaporation could take place, were yet found to lose weight; an effect solely ascribable to transudation. In man and other warm-blooded animals, when exposed to the same conditions, transudation takes place to such an extent that the sweat streams from all parts of the body. Thus, Delaroche and Berger found that air, very hot, and charged with extreme humidity, excited a more abundant transudation than dry air at a higher temperature. Now, if these facts are applied to the warm-bath, it will be seen that transudation is favoured, and that this will take place the more abundantly according as the temperature is more elevated. Thus, in his experiments on batrachians, Edwards found that, on comparing the total losses in the space of six hours at the temperature of 32° F. and those which took place at 104° F., they were nearly as 1 to 55. But does evaporation take place from the lungs during the use of the warm bath? Is the atmosphere so saturated with moisture that pulmonary evaporation ceases, as some have supposed? Dr. Madden, and apparently with justice, decides on the negative. In his joint memoir with Lavoisier, M. Séguin has stated that the proportion which the pulmonary bears to the general exhalation is as 7 to 18. In his first series of experiments, at a temperature of 60° F., the quantity lost in the bath was to that lost in the air as 6.5 to 17; showing that here the lungs alone were concerned, and, consequently, since cutaneous transpiration was proceeding as before, a quantity of moisture sufficient to supply this loss must have been absorbed. In the second series, at a temperature between 70° and 80° F., the result is still more favorable, the proportion being as 7.5 to 21.7; but, in the third, the loss was much greater, the proportion being as 13 to 23, the temperature being about 100° F.

These experiments are regarded by Dr. Madden, therefore, as conclusive evidence in favour of water having been absorbed. Whether this effect were produced by the skin or the lungs many may be inclined to doubt, and those who deny the existence of cutaneous absorption will at once attribute it to the latter. It is certain that the bronchial membrane absorbs some substances with great rapidity, e. g. turpentine, vapour of mercury, &c.: but it is contended that evidence is quite wanting in favour of the absorption of water by this membrane. The experiments which Dr. Madden has made tend to show that, if the lungs are capable of such absorption, the capability is very small, and very variable: and, he remarks, that, in thus gratuitously conferring this power upon the lungs, (and it must be confessed that evidence is wanting in its favour,) the difficulty remains of explaining why no such absorption occurred in the experiments of Séguin and others.

We have thus abridged the author's observations on Séguin's experiments, because they appear to render extremely doubtful the conclusions which have been drawn from these. We have but one remark to make on them; and that is, that those who seek for an *experimentum crucis* may object that, although cutaneous exudation takes place very rapidly in a heated moist atmosphere, it is not *therefore* to be inferred that it occurs equally in hot water. The subject probably admits of no closer demonstration, but, as the circumstances are not precisely similar, the objection may be made; and will be estimated of more or less importance according to individual views and bias. We do not regard it as necessary to allude to any more negative facts, such as those mentioned by Dr. Currie; and without quoting any of the experiments of Dr. Young, with which our readers are familiar, or those of Dr. Dill, we proceed to our author's own experiments.

"The balance which I employed was extremely sensible, vibrating perceptibly with a weight of a very few grains; and, in order to obtain the greatest possible accuracy in my results, I made use of the following plan. Having carefully noted the spot to which the extremity of the beam, when perfectly horizontal, pointed, this was taken as the centre of a scale, constructed with the greatest care, by adding weights alternately to the beam and the seat, and marking the respective points which the beam indicated. By this means I was enabled to measure with the greatest accuracy any weight not less than gr. x., but lower than this I did not attempt to go, nor do I think that it was necessary. The scale then being affixed to the wall, and the balance so arranged that the extremity of the beam pointed exactly to the horizontal line, I was accurately weighed and the same process was repeated in half an hour. Immediately afterwards, I entered the bath, and to avoid the most distant chance of fallacy from pulmonary absorption, my head was enveloped in an oiled-cloth bag, to which a long glass tube was attached, and passed out of window, so that I breathed the external air alone. After remaining immersed for the same length of time, I was carefully dried and again weighed. The results of twelve experiments thus performed, though not uniformly the same, were yet, upon the whole, extremely satisfactory. The whole of these bath experiments were performed in August and September 1836, the weather being for the most part fine, and the air of moderate temperature." (p. 58-9.)

We give one of Dr. Madden's experiments in detail, and adjoin a table of the whole.

"*Experiment* xi. At a quarter before seven A.M. having just risen, my weight was 111lb. + ℥j. + ʒiv. + ℈ij. In an hour and half, that is, at a quarter past eight, I had lost ℥ij. + ʒvj., giving an average expenditure of ʒvij. + ℈j. during the half hour. I then entered the bath at 84° F. and remained immersed during the space of thirty minutes. At the expiration of this period, I had *gained* ℥ss. Add to this, ʒiij. for pulmonary exhalation, and the whole increase, even upon the supposition that cutaneous transpiration was completely suspended, amounted to ʒvij." (p. 59.)

| Exp. | Date. | Time. | Bar. | Expenditure. | Temperature. | Gain. | Loss. |
|---|---|---|---|---|---|---|---|
| 1 | Aug. 20 | 8 A.M. | 29.63 | ʒvij. + ℈j. | 84° F. | ʒiv. | |
| 2 | ... 22 | 8 A.M. | 29.35 | ʒv. | 84°—88° F. | | ʒij. + ℈j. |
| 3 | ... 24 | 7½ A.M. | 29.96 | ʒvj. | 84° F. | ʒj.+℈j.+gr.x. | |
| 4 | ... 25 | 7¼ A.M. | 29.60 | ʒvj. + gr. x. | 84 ... | ʒj. | |
| 5 | ... 26 | 4 P.M. | 29.80 | ʒijss. | 88 ... | ʒj.+℈ij.+gr.x. | |
| 6 | Sept. 2 | 7 P.M. | 29.60 | ʒjxss. | 88 ... | ʒj. + ℈j. | |
| 7 | ... 5 | 6½ P.M. | 29.45 | not observed. | 94 ... | ʒj. | |
| 8 | ... 6 | 4 P.M. | 29.5 | ʒvij. | 98 ... | | ℥ij.+ʒij.+gr.x. |
| 9 | ... 7 | 6 P.M. | 29.8 | not observed. | 91 ... | ℈ij. | |
| 10 | ... 8 | 3½ P.M. | 29.65 | ʒvj. + ℈ij. | 92 ... | ℈ij. | |
| 11 | ... 9 | 7 P.M. | 29.8 | ℥j.+℈ij.+gr.x. | 90 ... | ʒjss. | |
| 12 | ... 10 | 4 P.M. | 29.9 | ʒvjss. | 90 ... | ʒv. + ℈j. | |

(p. 63.)

On the above analysis of his experiments, Dr. Madden remarks that it affords indubitable evidence in favour of cutaneous absorption; although this function appears to be exceedingly variable, and it is very difficult to discover any general laws by which it is regulated. We certainly can hit on no other explanation of the above facts, conducted as they appear to have been with great caution, and a knowledge and avoidance of those sources of error which have rendered the experiments of some previous enquirers of no utility in arriving at a safe judgment on the subject before us. Still it is very unsatisfactory to meet with such different results of similar experiments, conducted by different individuals. It is, no doubt, wrong to allow (admitting the credibility of the facts adduced, and admitting also the completeness of such facts,) positive conclusions to be affected by negative ones: at the same time it is remarkable, and we know not how to explain it, that, in ten out of twelve of Dr. Madden's experiments, there appears to have been actual increase of weight in the bath; whilst Séguin states that in no instance was the weight increased during immersion; and this remark applies to no less than thirty-three experiments, instituted with great care. These discrepancies are among the wonders that never cease: and similar ones meet us not only on this subject but in all physiological subjects which admit of doubt and experiment and difference of opinion. May they not be supposed to indicate that we are ignorant of some fundamental fact which may hereafter be discovered to the disturbance of much of our present knowledge, and which, perhaps, it is essential that we should be acquainted with, before attempting to explain them?

Chapter ii. is on *Absorption of Aqueous Vapour from the Atmosphere.* On this point it is not to be expected than any very satisfactory evidence can be at present obtained. For lovers of the marvellous this chapter contains many curious histories of the enormous disproportion between the quantities of urine and other fluids excreted, and the quantity of fluid taken into the body. It appears also that, in certain cases, thirst is allayed and life preserved by the moisture of the atmosphere alone; that there are recorded examples of the *actual increase of weight* which the body has been found to obtain through the medium of a moist atmosphere. "Thus De Gorter observed that under these circumstances, his body would absorb from two to six ounces during the night." But being somewhat incredulous of the virtues of jockeys we should hesitate to ascribe to cutaneous absorption the fact that "a jockey who had been for some time in training for a race—and who had been reduced to the proper weight, on the morning of the trial, being much oppressed with thirst, took a cup of tea, and shortly afterwards his weight was increased 6lb., so that he was incapacitated for riding." We do not doubt the fact, but we have some suspicions that the individual in question could have explained it without reference to cutaneous absorption from a moist atmosphere. Mr. Bell has remarked, that frogs kept in a moist situation, without having access to water in any form but vapour, have a constantly moist skin, and a water bag filled—an effect which can only be ascribed to absorption. There is in these cases evidence of absorption, though not of the surface from which such absorption takes place. Do Dr. Madden's reasonings on the matter make sufficient allowance for the fact that the lungs are a *vital* organ?

We pass over chapter iii., on *Absorption of Contagious Matters and Miasms.*

Chapter iv. is on the *Absorption of Medicinal Agents.* The first section is on the *Absorption of Solids;* viz. "powders, ointments, solid extracts, plasters, and poultices, both when simply applied to the common integuments and when administered as frictions; the latter being mentioned, not so much in support of the doctrine, as with a view to its practical application in the treatment of disease." We shall briefly notice some of the facts under this head. Kellie mentions the fact of salivation from wearing a mercurial plaster. Tartar emetic, applied as a powder by Séguin, was absorbed, and without producing its usual local effects; but it has been known, when rubbed upon the skin, to produce nausea and vomiting. Arsenic has produced violent inflammation, when employed to destroy lice. Some purgatives applied to the skin of the abdomen, in a solid form, have acted on the bowels. Haller states that pills placed upon the epigastrium have purged, and that the bowels have been opened from handling colocynth. A rhubarb poultice will purge children. Frictions with squill and digitalis have been said to effect diuresis. Some of the effects of opium, applied in a solid form, are well known: the same may be said of belladonna, tobacco, veratria, assafœtida, &c. —*Absorption of Fluids.* Salivation has been produced by the absorption of a solution of corrosive sublimate. After immersing his arm in a solution of hydriodate of potass, Dr. Madden has distinctly detected iodine in his urine. He also rubbed a solution of tartar emetic in water into his hands: this was followed by an unusual degree of languor and debility, which continued for some hours, and was accompanied by frequent though slight attacks of nausea, but the appetite was unimpaired. No local action was excited. Solutions of salts of lead have been known to be absorbed from the skin. In experiments on his own person, Dr. Madden has produced purgative effects by infusion of rhubarb, jalap, and gamboge. The experiments performed with turpentine, which have reference to the subject of cutaneous absorption, are often referred to. Dr. Young's experiments are well known. Klapp and Rousseau repeated

Young's experiments, but with a contrary result. The following is our author's experience on this point: With his head enclosed in an oiled cloth bag, to which a long tube was attached, and passed out of the window, his arm being luted into a large glass jar, he had some turpentine poured upon it, and the jar accurately stoppered. The urine acquired a distinctly violet odour. The experiment was repeated four times, with equally satisfactory results. The effects of the external application of solutions of opium are well known; those produced by belladonna and tobacco are equally unequivocal. Dr. Madden has not been able to produce the physiological actions of nux vomica, by applying solutions of it to the skin of animals. The following is one of his experiments:

> "At ten minutes before eleven A.M., I dropped half a drachm of the oil (of bitter almonds) into the axillæ of a pigeon, and then introduced it into a jar, closed at the mouth with oiled silk, through which its head alone protruded, and was carefully luted. At 11 + 5′, the breathing became accelerated. In two minutes more, the bird vomited. At 11 + 12′, respiration was very laborious, and became gradually worse until 11 + 23′, when a fit of convulsions came on. After this, the breathing grew slower and slower, and death took place at ten minutes past twelve." (p. 122.)

In several experiments performed by Dr. Madden, he was unable to detect ferrocyanate of potass in his urine: but, although respecting some of the fluids mentioned in the above section, there may be a doubt whether their action was subsequent to cutaneous absorption or no, the instances of turpentine and iodine appear to justify no other inference.

*Absorption of Gases and Vapours.* Our present limits do not allow us to enter upon the disputed subject of cutaneous respiration in man. The contradictory results of experiments thereupon is sadly unsatisfactory. The following is an experiment of Collard de Martigny on the action of carbonic acid on the surface:

> "He placed himself entirely under the cloth which covered a deep tub, partly filled with fermenting grapes; and, his nostrils being accurately closed, he breathed through a long tube which communicated with the external air. 'In about five minutes,' says he, 'I felt a slight weight in the head and troubled vision; in eight minutes, considerable pain in the temples and under the orbit, ringing of the ears, and vertigo; in ten minutes, the same symptoms remained, with general weakness, and the heart's action became slightly accelerated; in twelve minutes, all these were aggravated, respiration became slow, a vague and indefinable terror seized my senses; and, finally, at the twenty-ninth minute, the weakness and torpor were so pronounced that the tube by which I breathed fell from me, and I could scarcely leave the spot.'" (p. 131.)

Dr. Madden found no effect produced on a pigeon, the body of which was confined in carbonic acid gas: but the action of sulphuretted hydrogen gas was very rapid upon a rabbit confined in it, so that it could not come in contact with the lungs. The anus of the animal was covered with sticking plaster. The rabbit died within twenty minutes. A solution of acetate of lead, poured beneath the skin and into the cavity of the peritoneum, gave distinct traces of the presence of this gas. The physiological action of mercury is capable of being produced by this metal in a state of vapour. Mr. Abernethy appears to have had a confidence in its efficacy which subsequent experience has not fully justified. The evidence in favour of the absorption of gaseous substances by the skin Dr. Madden does not consider as quite so satisfactory as that which he has adduced in the case of liquids.

Our Tenth Number contained an article on Endermic Absorption. It renders unnecessary any notice of Dr. Madden's chapter on the subject; and, indeed, there are other details which we have passed over, having already noticed them in that paper. From what has been previously said concerning the anatomy of the cutaneous system, it may be inferred that our author has nothing very satisfactory to suggest as to the agents by which absorption is effected. Without stating it as a decided opinion, he regards the first step in this process to be endosmosis. But there is so little that is satisfying in physiological speculation, that we are willing to give room for more important subjects.

We would fain hope that, in the disappointing path of practical medicine, our author may find that he has not over-estimated the advantages which cutaneous absorption is about to afford him. Commencing, as he has done, the theoretical part of his subject with such considerable care and such meritorious research, and being, as it were, a partisan (though an honest one) of the system of which he has shown himself so able an advocate, we shall be very glad to see a careful and unprejudiced detail of the effects produced by medicinal applications to the skin, in cases of disease, observed in the course of his own practice. It is a comparatively easy task to collect from a host of authors (of whose power, or even will, to observe correctly we almost require to be satisfied before we yield much credit to their recorded observations,) a number of cases illustrative of the benefit of this or that remedy in various forms of disease; and such an array of triumphs sometimes induces the forgetfulness that there may have been a far greater number of mishaps. That there is an advantage in the application of medicines to the surface, in various ways and forms, appears to be a growing opinion; and the facts collected by Dr. Madden, proving medicinal action through the skin, together with the additional evidence which he has afforded of the absorption of substances under various forms from it, render his essay well worthy of perusal, both by the physiologist and medical practitioner.

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## ART. V.

1. *Final Report of the Committee of the Philadelphia Medical Society, on the Construction of Instruments, and their mode of action, in the radical Cure of Hernia, &c.* By HEBER CHASE, M.D.—*Philadelphia*, 1837. 8vo. pp. 243.
2. *De la Cure Radicale des Hernies.* Par M. le DOCTEUR BELMAS, Ancien Chef des travaux Anatomiques de la Faculté de Médecine de Strasbourg. (*Revue Médicale francaise et étrangère.* No. 2 and 3. —*Février et Mars*, 1838.)
3. *Uber radicalen Heilung reponibler Brüche.* Von DR. PH. FINCK, Grossherzogl: Badischem Militärarzte. *Mit Zwei Kupfertafeln.*—*Freiburg*, 1837. 8vo. pp. 48.

THAT mode of treating reducible hernia which has for its object a radical cure of the complaint, and not a mere palliation of the inconveniences attending it, has engaged the attention of surgeons in all countries, and at all periods. Within the last few years, the subject seems to have received a fresh impulse, both on the Continent and in America, where considerable talent, combined with much patient ingenuity, appears to have been bestowed on this important branch of surgery. On this account we purpose, in this article, to lay before our readers a concise summary of what has hitherto been attempted and accomplished with regard to the radical cure of hernia, and to institute a comparison between the different methods proposed and executed by the contemporary surgeons of France, Germany, and America.

As the inguinal or scrotal hernia is by far the most common, the most unmanageable, and entails the greatest degree of evil and suffering on those afflicted with it, and as it has, consequently, received the greatest share of attention from surgeons, our remarks and enquiries will chiefly refer to this particular species of rupture.

Almost every variety of hernia may be described as the escape of part of the contents of the abdomen through one of those openings which have a normal existence, and which form communications between the interior and exterior of the cavity, for the transmission of vessels, nerves, or other structures. Thus a hernia can hardly be said to establish a new opening, but rather consists in the dilatation, accompanied by alteration as to form and direction, of one which existed previously in the healthy state. This view of the subject is strikingly exemplified in the formation of a common inguinal hernia. The animal economy renders a communication necessary by means of the spermatic cord, between the testes and the abdominal cavity, and nature, foreseeing the risk which this necessity involves, has, as it were, exerted all her ingenuity in rendering the communication of such a kind as shall allow the transmission of vessels, nerves, absorbents, and ducts, without compromising the integrity of the abdominal walls through which they have to pass. Hence the oblique and lengthened course of the cord as it traverses the walls of the belly; hence the adaptation of fasciæ and tendinous structures around the different openings, maintaining the security of the internal and external abdominal rings. A hernia in its descent slowly but surely undoes the work of nature, destroying step by step the barriers purposely opposed to such an accident: the inguinal canal becomes gradually dilated, and as the parts which surrounded the cord yield and expand before the superincumbent pressure, so does the obliquity and length of the passage disappear, until a nearly straight and direct communication becomes established between the abdomen and the scrotum; leaving scarcely a trace of what, in the anatomical language of the schools, are commonly but erroneously denominated "the parts of hernia." It is a dilatation rather than a rupture; but at all events there is a breach, a deficiency of the abdominal parietes; and a radical cure can only be brought about by such means as will lead to the filling up or closure of the abnormal opening.

The rationale of all methods adopted to promote a radical cure may be resolved into two principles, 1st. The prevention of protrusion until nature has had sufficient time to remodel the parts, to rebuild the fabric which the hernia has destroyed; in other words, to fill up the opening and restore the integrity of the abdominal walls. 2d. The establishment by artificial means of so much inflammation either within or around the neck of the hernial sac, or in both these situations, as shall produce adhesion and agglutination between the separated parts, and thus obliterate the opening which allowed the descent of the protruded viscera.

I. The first of these principles, or that of allowing nature to contract and close the opening, is of course only to be carried into effect by the application and adjustment of trusses. Its success depends entirely on the skilful construction and the accurate adjustment of the mechanical contrivance, which, as a work of art, belongs as much to the artificer as to the surgeon. We have no hesitation in affirming that the only way in which a truss can be rendered available in producing a radical cure, is by its preventing all protrusion or even tendency to protrusion. Therein consists the excellence of its mechanism and its adjustment. In its application, the nearer it can be brought to resemble the natural walls of the abdomen, the more effectual it will be. It should just afford as much support to the abdominal walls as will compensate for the inability to resistance which has led to the complaint; and it should just occupy or oppose itself to the breach which the previous descent of the hernia has left. As regards the promotion of a radical cure, we believe that a truss can do no more than to prevent the process of nature, in contracting the opening and restoring the walls to a normal condition, from being interrupted and thwarted by the occasional recurrence of a fresh protrusion: and where the resources of nature are inadequate to repair the breach, we are inclined to deny the efficacy of a truss in stimulating or assisting her to effect the object, otherwise than in the manner just mentioned. In such cases the truss becomes a palliative not a curative remedy. Trusses have indeed been constructed for the express purpose of producing irritation on the surface against which they are applied, and of exciting so much deep-seated inflammation as to condense the cellular tissue, fascia, and tendon about the hernial orifice, into one common mass by adhesion, thus carrying into effect the second of the two principles upon which the rationale of a permanent cure must be founded. These instruments have never stood the test of experience and fair investigation. Their application and use are in many instances so intolerable to the patient, as to render the remedy far worse than the disease; and, if persevered in, their pressure tends rather to produce a gradual absorption of the abdominal walls than that thickening and consequent increase of solidity and strength which they were intended to promote. The report of the Philadelphian Committee condemns, in decided terms, the trusses of Messrs. Stanger and Wood, which were constructed on this principle and furnished with blocks of wood, lead, and iron, exerting a strong pressure with the view of exciting inflammation and adhesion. The committee represent them as unnecessarily severe for the purpose of preventing protrusion, and utterly useless if not mischievous as far as regards any ulterior object. Returning, then, to the axiom which we just laid down, viz. that a truss is only useful in producing a radical cure inasmuch as it allows nature an undisturbed opportunity of healing the breach, it becomes a matter of the deepest importance, as regards the diagnosis and treatment of hernia, to ascertain the positive efficacy that may be anticipated from the use of a truss under the different forms and degrees of hernia; not merely its efficacy to retain the bowel, but to accomplish a cure within a reasonable period of time.

If we are to place implicit credence in the assertions and the alledged experience of the different inventors and improvers of mechanical contrivances for support and retention, we must believe that the truss is all-sufficient to prevent protrusion, and that the vis medicatrix naturæ is in every instance competent to perform its part in the removal of the complaint: the genius of the mechanic has triumphed, and precludes the necessity of farther interference on the part of the surgeon.

We must refer our readers to a former number of this journal, (Vol. V. p. 300,) and to the work which heads the present article for a description of the trusses invented by Dr. Chase, and the success which has attended their use. He seems to have brought the instrument to a perfection which it had never previously attained, and the Philadelphian Committee, whose testimony is certainly entitled to our confidence and respect, report in the highest terms, of its efficacy in producing a radical cure, not only in ordinary cases, but in many which held out but little prospect of so fortunate a result. We must, however, observe that the report of the committee must necessarily have been confined to those cases which were brought under their own observation, and these, in all probability, by no means included all the individuals who had been subjected to the apparatus recommended by Dr. Chase, although he certainly seems to have given every opportunity of submitting his mode of treatment to a fair and impartial trial. In the report referred to in our ninth Number, Dr. Chase's instruments are figured, their mode of application minutely described, and the general results derived from their use exposed at length; otherwise we should have thought it necessary to lay before our readers, in this place, such an account of them as might enable the surgeon fully to comprehend their mechanism both of construction and application, in order that he might himself test their efficacy. We must therefore request our readers, before going further, to peruse the document, which he will find well worthy his best attention. We are far from thinking that the mechanical contrivances for preventing protrusion have in this country received all the improvements of which they are capable. Too much has generally been left to the instrument-maker and too little has been suggested by the surgeon; but, at the same time, we feel convinced that cases of hernia will be constantly met with, where no expectation of a radical cure through the application of a truss, can possibly be entertained; and where the surgeon feels himself called upon to tell his patient that he must make up his mind to wear the instrument as a palliative for the remainder of his life.

II. From the time of Celsus to the present period, attempts have been constantly renewed to produce obliteration of the hernial opening, either by attacking the dilated inguinal canal from without, through the agency of severe pressure, escarotics and actual cauteries applied to the surface of the skin; or else by the bolder method of at once penetrating to the seat of mischief and endeavouring, by various methods, to stimulate the neck of the peritoneal sac itself, and its immediate investments, to inflammation and adhesion. We have already noticed the futility and the mischievous effect of high-pressure trusses: they have been employed at different times by Paré, Petit, Richter, Langenbeck, Arnaud, and a long list of less distinguished names, either simply or combined with irritating applications; and while they appear, in many instances, to have failed in effecting a cure, the remedy has occasionally involved the patient in fresh disasters, not only by subjecting him to a long and tedious confinement, but by giving rise to extensive sloughing of the external parts, which in more than one case terminated fatally. In fact, the skin does not appear capable of transmitting sufficient inflammation to the parts beneath without itself becoming destroyed in the process, and all external remedies appear to have failed in effecting the desired object.

Our ancestors, however, were not deficient in resources for striking at once at the root of the evil and applying their means to the neck of the sac and the region of the inguinal canal. Many were the contrivances put into practice, some of which savour strongly of the adventurous and reckless spirit which animated the older surgeons: thus, the neck of the sac was firmly tied up by means of a ligature passed around it immediately below the external ring, and frequently including the spermatic cord as well as the integuments; or else it was sewn up by a succession of stitches with an equal disregard to the neighbouring structures. Such operations, by which the loss of the testis was compromised and the life of the patient endangered, were nevertheless enobled by the names of "Golden Incision," "Royal Suture," &c. The latter of these methods, including a few additions, as practised by the surgeons at Constantinople, is so graphically described in a French history of the Ottoman Empire, and affords so striking an example of the heroic practice of the times, that we shall be excused for transcribing the original passage. The author after alluding to the means employed for the cure of rupture by the practitioners of the sublime Porte proceeds to say—"Ils l'entreprennent sur toutes sortes de gens; durant mon séjour à Constantinople, j'en fis l'épreuve sur mon secretaire. Ils le lièrent sur une forte planche, ils ouvrirent le sac, réduisirent les parties sorties, puis firent une suture en enlevant avec un rasoir toute la portion de peritoine qui se trouvait au-devant d'elle. Après quoi ils couvrirent la plaie avec de la graisse de porc, y mirent le feu avec un fer rouge, et arrosèrent ensuite les parties avec des blancs d'œufs. Le patient ne donnait plus aucune signe de vie, et quand il revint à lui ce fut pour se plaindre de violentes douleurs dans le ventre, &c." We are not informed whether the secretary was fortunate enough to survive this treatment, or whether the inflammation produced was effectual in closing the hernial opening.

The operation of surrounding the neck of the sac by a ligature to the careful exclusion of the adjacent parts, was likewise practised and recommended by Paré, Heister, and Schmucker, also by Désault and Sharpe; but the method was finally abandoned in consequence of the pain and danger attendant upon it and its frequent inefficacy in producing a radical cure. Several other contrivances proposed and practised with a view of obliterating the communication between the hernial sac and the peritoneal cavity of the abdomen shared the same fate: these consisted in scarifying or applying caustic to the neck of the sac, injecting it as in hydrocele, &c.

A more recent attempt on the same principle, and with somewhat better success, has been made by M. Bonnet, chief surgeon of the Hôtel Dieu of Lyons, which we think of sufficient importance to notice more at length. Several cases treated in this manner have been published very recently in a French journal.

In the first case, operated on by M. Bonnet, he passed eight pins through the hernial sac, near the inguinal ring. These were maintained in their situation by a cushion and a bandage. On removing the bandage, three days afterwards, the pins had all escaped. The object of

M. Bonnet was next to pass the pins completely through the root of the scrotum, and to twist their points so that they could neither pass in nor out. In order to prevent their sinking into the inflamed skin, and to effect a sufficient degree of pressure upon the parts situated between the extremities of the pins, he enlarged them by means of a piece of cork. The hernia being reduced, the root of the scrotum was seized as close as possible to the abdominal ring, and the cord placed at its external side, in a circle formed by the left thumb and index finger. The points of these fingers being forcibly pressed together, he passed a pin in front of their nails, behind the coverings of the hernia, and near the suspensory ligament of the penis. Having inserted it, until its head rested upon the skin, and its point escaped in front, he passed this point into a piece of cork, which he pressed upon the point until the parts between the cork and the head of the pin were somewhat compressed. The point was then fixed by twisting it with a pair of pincers. The first pin being thus fixed, he carried the cord between it and the points of the left thumb and index finger. He then, following the ends of his fingers, passed a second pin parallel to the former, situated from six to seven lines farther outwards. At the same distance was then placed a third and a fourth. The pins were removed from the sixth to the twelfth day, and, as soon as the sensibility of the parts permitted, a compressing bandage was applied. As this process was not sufficient, a second row of pins was inserted, so as to multiply the points of adhesion, and these pins were allowed to remain in until the ulceration of the skin was complete, and the compression was exercised upon the cellular tissue itself. But, notwithstanding this improvement, M. Bonnet effected but a temporary and apparent cure on the only old person upon whom he operated. He failed in three operations upon adults for very large herniæ, which escaped from the inguinal canal, and which had become direct and large enough to allow of the simultaneous introduction of several fingers. He had complete success however on three adults, whose hernia were as large as the fist, and in whom, the inguinal canal, still oblique, allowed the introduction of but one finger. And a similar success attended the operation on a child of ten years old, where the hernia descended to some inches above the knee, after he had taken continued exercise.*

But among the modern propositions and plans for the radical cure of hernia by the obliteration of the passage, conceived in a spirit more in accordance with the advanced state of our science, those recently introduced by MM. Belmas and Gerdy deserve a more circumstantial notice and fuller consideration. Each of these surgeons has chosen a different road to the attainment of his object. The principle carried out in the operation of Dr. Belmas, is the closure of the neck of the sac by exciting adhesive inflammation on the serous surface itself; while Mr. Gerdy has followed up the method of closing the dilated canal and rings through which the sac had descended from the abdomen. The one addresses his remedial measures to the peritoneal communication between the scrotum and the belly, the other seeks to supply by artificial means the deficiency in the abdominal walls which has led to the abnormal protrusion. The first is a modification of the principle which occupied the attention and exercised the ingenuity of so many of the older surgeons: the latter presents altogether a new feature as regards the application of surgical means for the cure of reducible hernia. We shall give an abridged account of the two methods, and endeavour to compare them with each other, both as relates to the operations themselves, and the results which may reasonably be anticipated from them. We ought to state here that the small work of Dr. Finck contains a brief and accurate account of these methods, and also short references to those proposed by former surgeons: it is a respectable compilation, and does not affect to supply any original information.

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* Bull. gen. de Therapeutique. T. x. 9e liv. Mai, 1836.

III. With a view of making himself acquainted with the safest and most certain mode of exciting a local adhesive inflammation between the opposed surfaces of a serous membrane, Dr. Belmas instituted a series of experiments on animals, and finally ascertained, that when a small bladder of gold-beater's skin, inflated with air, was introduced into the peritoneal cavity of a dog, the spot to which the foreign body had applied itself was indicated, after a lapse of three months, by the presence of a globular adhesion ("adherence globuleuse") a sort of fibrous nodule, ("noyau fibreux"), while the general peritoneal cavity remained free from any other trace of inflammation and adhesion. The gold beater's skin itself had disappeared, that is to say, it had either been absorbed, or else become so completely identified with the lymph effused, as no longer to be distinguishable.* Dr. Belmas having now, as he considered, discovered an animal substance, the presence of which was sufficient to produce a mild form of adhesive inflammation, purely local in its character, between the walls of a serous cavity, and which would be spontaneously removed by absorption after accomplishing the desired object, proceeded to make trial of its efficiency in procuring the obliteration of a hernial sac. Thirty dogs afflicted with rupture were selected for experimental operation. Inflammation was excited in the hernial sac, more especially about its neck, by the introduction of gold-beater's skin. In the majority of cases obliteration of the sac was obtained; in some it merely became diminished in size, but in no instance was the operation followed by serious mischief. From this, Dr. Belmas felt himself justified in applying his principle to the human subject. The operation of conveying the gold-beater's skin into the mouth of the sac was commenced by means of a trochar, which entering the lower part of the sac was carried upwards until it had passed the neck, and then brought out through the integuments just above the external ring. A very complicated apparatus of stilets, canulas, and stop-cocks, contained one within the other and individually removable during the manipulations, was then put into motion. The machine by which a fragment of gold-beater's skin was deposited and left in the sac is minutely detailed, but, as little idea of its construction can be conveyed in words, we shall pass it over, more especially as its use was soon abandoned for another contrivance. Indeed, the severity of the operation and some rather untoward results, subsequently induced Dr. B. to adopt the method of merely puncturing the lower part of the sac with a trochar and canula, and then introducing through the latter one or more portions of the gold-beater's skin supported on sticks of dried and hardened gelatin.

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* See a further account of these experiments in our last Number, p. 232.—Eds.

These were pushed up into the neck of the sac and left there. The gelatin soon dissolved, and left the membrane which had surrounded it, in contact with the serous surfaces. To accomplish this object also, a peculiar apparatus was used, the mechanism of which we have in vain attempted to understand, but we are assured that if complicated in structure it is simple in its action. A bandage, or properly prepared truss, must be worn for a certain period after the operation, or until the adhesive process has become confirmed and solid. The result of ten operations is thus stated:—"Two out of the number left the neighbourhood after the operation but have represented themselves as cured. In three other cases the success was placed beyond a doubt. In three more an obliteration was obtained of the neck of the sac, but the subsequent necessary compression at the ring having been abandoned too early, a fresh protrusion of the obliterated point took place: the bandage has been reapplied, and the return of the ring to its normal condition is already such that we anticipate a decided cure. The two remaining individuals were obliged to lay aside their bandages at an early period in consequence of the inconvenience they produced, and in them the herniæ have reappeared: they are anxious to undergo a fresh operation." [*Revue Med. Mars*, 1838. p. 412.]

We conceive that the table of results offers the best commentary on the method adopted by Dr. Belmas for the radical cure of hernia. In our opinion the operation is open to the following objections, which certainly have not been satisfactorily combated by the author in his treatise on the subject.

1. By Dr. Belmas' own showing the operation is by no means easy or simple, requiring considerable practice in the use of a rather complicated apparatus, together with great tact and delicacy of manipulation.

2. The means employed seem occasionally to be inadequate for the accomplishment of the object desired, viz. the sealing up of the neck of the sac, or, at all events, to require the application of pressure for so long a period, as may render it doubtful whether the cure ought to be ascribed to the operation or the continued use of the truss.

3. The success of the operation is confined to the sac itself, and merely cuts off its communication with the peritoneal cavity without diminishing the size of the opening in the abdominal walls through which the hernia descended: consequently, we find that in three instances a subsequent protrusion took place, the neck of the sac being forced down by the pressure of the viscera. This very circumstance must render the operation less efficacious when we have to deal with old voluminous wide-mouthed ruptures, which of all others are the very cases considered to be incurable under the ordinary treatment by means of trusses. Future experience will however afford the best test of the utility of Dr. Belmas' plan, and the trials have certainly at present been too few to warrant us in passing a decided judgment on the question.

IV. The plan adopted by M. Gerdy is, as we have stated, founded on an entirely different principle. He practises a very summary but at the same time effectual method of closing the abdominal opening, by carrying a fold of the scrotal integuments upwards into the external ring, and securing it in that situation by one or more sutures. In fact, by establishing a sort of artificial hernia in an inverse direction, (that is from without to within, or from the scrotum towards the abdominal cavity,) he seeks to fill up the dilated inguinal canal, and thus oppose the future descent of the viscera into the original hernial sac. The principle which has guided him was probably suggested by the result of two cases recorded by Arnaud, wherein, during the reduction of an inguinal hernia, a fold of skin was carried up and drawn into the ring: adhesion took place and the cure was permanent. The operation of M. Gerdy is simple, the only instrument required being a curved needle fixed in a handle and pierced with an eye at a short distance from its point. The patient is placed in the horizontal position, and after the surgeon has ascertained that the hernial sac is empty, and has carefully examined its relative situation and connexions with respect to the spermatic cord and the inguinal canal, he passes the fore finger of his left hand upwards through the external ring, carrying of course before it the skin of the scrotum. A blind pouch of integument resembling the finger of a glove is thus introduced into the inguinal canal, anterior both to the cord and to the neck of the hernial sac. The point of the needle, armed with a ligature, is then guided along the finger to the bottom of the pouch, and by depressing the handle is brought out through the anterior walls of the inguinal canal. One end of the ligature is then laid hold of by an assistant; the point of the needle is withdrawn into the pouch, and a second similar perforation is effected, but at a few lines distance from the previous puncture. In this manner both ends of the ligature are carried from the inside of the pouch, or invagination as it is called, through the inverted skin, through the tendon of the external oblique, the superficial fascia and common integument on to the inguinal region, where they are tied together over a piece of bougie. One or more of these stitches are made according to the size of the opening, and the invaginated fold of scrotum is thus retained in its new situation. Caustic ammonia is then freely applied to the internal surface of the pouch formed by the invaginated scrotum, and the operation is concluded by covering the parts with a slight compress spread with common cerate. About the third day a copious suppuration is established, and at this period the sutures may be removed unless circumstances should warrant their further retention. Nature is then allowed to complete the process which the surgeon has commenced. Adhesion gradually takes place between the fold of invaginated integument and the sides of the inguinal canal into which it has been received, while at the same time the pouch itself becomes obliterated by the process of granulation set up by the caustic. Within the fifteenth or twentieth day the suppuration ceases, and the dilated inguinal canal will then be found completely blocked up by a strong thick plug, the presence of which is indicated by an external projection, and which may be distinctly felt beneath the skin and tendon. This gradually disappears, until no trace of the operation remains except a trifling scar at the entrance of the pouch, and a slight elevation and shortening of the scrotum on that side. The patient should be kept on his back for a month after the operation, and his shoulders and pelvis should at first be somewhat raised for the purpose of relaxing the walls of the belly. The scrotum should also be supported. The patient's diet should be low, and everything that tends to bring the abdominal muscles into action, as coughing, sneezing, difficult evacuation of the bowels, &c. carefully avoided. The progress of inflammation ought to be watched, and the usual antiphlogistic remedies adopted, if it evinces a disposition to exceed the proper degree of intensity or to spread beyond the limits prescribed. A truss should be worn for about three months after the patient has ceased to observe the recumbent posture.

In large old inguinal herniæ where the peritoneal sac is closely connected to the integuments of the scrotum and to the circumference of the ring, there would probably exist some danger of its becoming folded, carried up before the finger during the process of invagination, and subsequently pierced by the needle; this, in fact, has occurred in the practice of Messrs. Velpeau and Gerdy, but does not seem to have been followed by any untoward consequences. The risk of wounding the cord, the epigastric artery, or the abdominal peritoneum itself has been urged as an objection to the operation, but we conceive that such accidents could hardly occur under the hands of a cautious and skilful surgeon.

M. Gerdy has operated on thirty individuals, all of whom were cured, temporarily at least, with the exception of one, who was attacked with pleuritis under which he sunk. We are not, however, furnished with any history of the cases extending beyond the period of their return to their ordinary avocations; and, in all probability, sufficient time has hardly yet elapsed to test the ultimate efficacy of the plan in producing a radical and permanent cure.

We think that the operation of M. Gerdy has many advantages over that of Dr. Belmas. In the first place, it is simple and easy in its execution, much more speedy and certain in its effect, and strikes at once at the root of the disease by plugging up the dilated inguinal canal. It is more calculated to overcome the difficulties presented by a large widemouthed hernia, as the number of sutures employed to retain the invaginated fold of integument can always be proportioned to the size of the opening. It likewise involves parts which do not readily resent the injury offered to them; and by leaving intact and undisturbed the serous cavity of the abdomen, offers an immunity against subsequent peritoneal inflammation which no other method can boast of. How far the neck of the hernial sac itself is rendered impermeable through this operation, can only be ascertained by experience and the evidence afforded by post-mortem examination. There can be no doubt that the mouth of the sac becomes narrowed, and, in many cases, may be completely obliterated by the pressure or the extension of inflammation excited in the adjacent parts; but so desirable a result is not necessarily inferred by the principle of the operation; and, should a communication still remain between the original sac and the cavity of the belly, a return of the hernia may possibly take place at some period after the use of the truss has been altogether abandoned. M. Gerdy does not appear to have confined his practice to those cases which held out no other prospect of relief, but to have operated indiscriminately for all kinds and conditions of ruptures. This we infer from the recommendation which he gives to return the sac as well as its contents previously to commencing the operation; and from the directions, that when the ring is too small to admit the finger, the invagination is to be accomplished by means of a curved hollow staff, through which the needle may be introduced. Of course this advice can only relate to the treatment of small or recent hernia, which we should consider remediable by the use of a truss.

The limits of this article will not allow us to extend our remarks; but we have been induced to direct our attention to this subject, not only because we are convinced that there are many cases of hernia which are incurable by the common palliative mode of treatment however judiciously applied; but also, because a speedier and more certain cure than can ever be accomplished by the use of trusses, would be an inestimable boon to a very numerous class of individuals. We allude more especially to the lower orders, to the working people who are the principal sufferers from rupture, who may be said to earn the complaint by their bodily labour, by the sweat of their brow, and in whom a continued necessity for renewed exertion almost precludes the possibility of rendering available those gentler means of cure which may be successfully adopted among the higher classes of society.

We think that the radical cure of hernia has in this country hardly received that share of attention which so important a subject deserves. We would ask our professional brethren to remember the many instances they must have witnessed, where the patient has sunk after the operation for strangulated hernia, often performed under the most disadvantageous circumstances; and we would then remind them that nine times out of ten the strangulation has supervened on an old rupture, which, through carelessness, ignorance, or poverty, has been suffered to exist from year to year, either totally disregarded or at best imperfectly supported. We conceive that, in the treatment of reducible hernia, the attention of the surgeon should be more strongly directed than it is at present towards the accomplishment of a radical cure. His knowledge and his experience will alone enable him to decide upon the most efficacious means for attaining this object and placing his patient beyond the reach of accident, whether by the use of trusses or by operation. It is not our intention to enter into the circumstances and symptoms which will guide him in the choice of the former or the latter remedy; but the size of the rupture, the period of time it has existed, the nature of its communication with the abdominal cavity, the age of the patient, his temperament, his condition in life, the occupation he is obliged to follow,—must all be taken into consideration as influencing the decision. In some instances it is probable that the inclinations of the individual himself may settle the question, leading him to prefer an operation as the speediest method of restoring him to a sound condition.

We have given a free but we hope an impartial opinion on the different operations for the cure of hernia, which have formed the principal subject of this article. They have yet to stand the test of time and experience before their respective merits can be definitively settled; but we are sure that the profession, and the public in general, will owe a tribute of gratitude to that surgeon, who shall succeed in rescuing a numerous class of his fellow creatures from an affliction, which, like the sword of Damocles, serves to embitter every enjoyment, and places their lives in continual jeopardy.

## ART. VI.

*Nouvelles Recherches sur le Rheumatisme articulaire aigu en général, et spécialement sur la Loi de Coincidence de la Pericardite et de l'Endocardite avec cette Maladie, ainsi que sur l'Efficacité de la formule des Emissions sanguines coup sur coup dans son Traitement.* Par J. BOUILLAUD, Prof. de Clin. Med. à la Faculté de Méd. de Paris. —*Paris*, 1836. 8vo. pp. 159.

*New Researches on Acute and Articular Rheumatism in general, and especially on the Law of Coincidence of Pericarditis and Endocarditis with this Disease, as well as on the Efficacy of quickly repeated Bleedings in its Treatment.* By J. BOUILLAUD.—8vo. pp. 159. 1836.

*Leçons de Clinique Médicale faites à l'Hôtel-Dieu de Paris.* Par le Prof. A. F. CHOMEL: *recueillies et publiées par* A. P. REQUIN, D.M.P. Tome II. (*Rheumatisme et Goutte.*)—*Paris*, 1837. 8vo. pp. 524.

*Lectures on Clinical Medicine, delivered at the Hôtel-Dieu, Paris.* By Professor A. F. CHOMEL: *reported and published by* Dr. A. P. REQUIN. Vol. II. (*Rheumatism and Gout.*)—8vo. pp. 524. 1837.

THE distinction of any class of diseases as *specific* is very unsatisfactory. The word is but a threadbare cloak of ignorance: and, although it may be said that we actually *know* but little of any disease or class of diseases, still there are certain tolerably uniform characteristics which enable us to arrange some of them with apparent correctness. Take, for instance, those forms of inflammation of organs about which opinions are nearly uniform. It is in these cases that examination after death has, when connected with symptoms and signs of disease during life, afforded a ground of classification: but, in the disease which we are about to consider, together with a great variableness in symptoms, there is the most scanty pathological history; so that, whilst many of its phenomena oblige us to regard it as inflammatory, it is, as it were, excluded from inflammations by the term *specific.* It must be expected that, under these circumstances, medical authors will scarcely be likely to agree; and the works before us are a curious specimen of this difference of opinion. Two physicians, enjoying similar opportunities of observation, living in a city where it can scarcely be supposed but that the disease which they were occupied in investigating must have presented itself in a similar form to each, are directly at variance respecting the etiology, the pathology, the symptoms of some of its attendant lesions, and the treatment of the disease. M. Bouillaud's work was published the first: in it he maintains that, in the majority of the cases of acute rheumatism, there are certain inflammatory affections of the heart; claiming, at the same time, the merit of having made this discovery: he asserts the purely inflammatory nature of the disease; he places it in certain tissues of the joints, and vaunts the efficacy of large and repeated bleedings for its cure. Although the present work of M. Chomel was published subsequently to that of M. Bouillaud, most of his opinions on the disease had been previously stated in a thesis, published in 1813, (and which is reprinted at the end of this volume,) and in journals and other works. He denies the frequency of the cardiac affection, and all M. Bouillaud's merits as a discoverer; maintains that the disease is not essentially inflammatory; that its primitive seat is not that stated by M. Bouillaud; and that the treatment recommended is quite unjustifiable.

M. Bouillaud's pamphlet is written with great spirit, but is the result of much less experience than the work of M. Chomel. The former confines his investigations to acute articular rheumatism: the latter considers the disease in all its forms. M. Bouillaud laughs at the notion of essential or idiopathic fevers: his opponent admits them. They both enter into the defence of their own notions with very considerable bitterness, and they are both led beyond the strict limits of truth in the support of their respective opinions.

In the present article we shall take M. Chomel as our guide, and shall now proceed to give, at considerable length, an analysis of his work; and, in order that there may be no misapprehension of the author's views, we must state, *in limine*, that he considers gout and articular rheumatism to be the same disease. It is necessary for the reader to bear this in mind; not only to understand the author's own views, but to estimate justly the value of the facts and arguments which he opposes to the views of M. Bouillaud. In our analysis we shall follow the order adopted by M. Chomel, leaving, as he has done, to the last chapter, our opinions as to the correctness of his classification.

Rheumatism, says M. Chomel, constitutes a natural class of diseases, distinguished by, 1, *their seat in fibrous organs;* 2, *their mobility*, or the facility with which they pass from one part to another; 3, *their intermittence*,—i. e. more or less frequent and sudden alternations of disappearance and recurrence. To these may be added a fourth characteristic, the *diversity of their forms:* for example, though rheumatism of the joints most frequently appears with heat, redness, pain, and swelling, there may be only pain, generally increased by pressure. Morgagni has cited one case of change in muscular structure from rheumatism; but this does not appear (as we shall hereafter see) to be a case of any value. M. Chomel knows no other case, nor is he aware of any change produced by rheumatism in muscles: hence this rheumatism, often characterized by pain only, cannot be regarded as an inflammatory disease. It is rare, but such has been the case, that pain is alone met with in articular rheumatism. It has more than once happened that no appreciable lesion has been found in the joints of rheumatic patients who have suddenly died of some other affection. Here, then, are two opposite forms of rheumatism; one, with the accompaniment of inflammatory lesions; the other, without appreciable organic change in the dead body. In a third species, rheumatism affords examples of lesions which approximate to *organic lesions, properly so called;* swelling of the parts is added to the pains, together with the formation of tophaceous concretions, erosion of cartilages, &c. Lastly, in some cases of articular rheumatism, where the most marked phenomenon, next to the pain, is an effusion into the cavity of a joint, which is serous and perfectly limpid, the disease has the true form of *essentiul dropsy* of the ancients.

Anatomical examination does not discover the seat of rheumatism. It is only after the observation of symptoms that the muscles were regarded as its seat; and to what class of their fibres it is to be referred still remains a question. But, as fibrous tissues are attacked where there is no muscular tissue,—e. g. the tendo Achillis,—it is probable that the aponeurotic expansions covering muscles are the parts affected in muscular rheumatism. It is but a matter of presumption that the fibrous tissues of joints are the parts first affected. M. Bouillaud, however, regards the essential seat of articular rheumatism as the synovial membrane. It is a question difficult, if not impossible, to decide, from the absence of morbid changes after death, and from the circumstance that, when these are found to exist, various tissues are implicated; and the question remains as to which was first affected. We content ourselves, therefore, with expressing our authors' opinions. In internal rheumatisms, following the same analogy, M. Chomel considers that the fibrous tissues are the parts affected; such, for instance, as exist in the heart, stomach, &c.

The division adopted by M. Chomel, and which, in our remarks on each of the volumes which stand at the head of this article, we shall follow, is this:

*Order* 1. Muscular Rheumatism; or rheumatism of the voluntary muscles.

*Order* 2. Articular Rheumatism; or rheumatism of the joints.

*Order* 3. Visceral Rheumatism; or rheumatism of certain fibrous organs situated within the splanchnic cavities.

ORDER I. MUSCULAR RHEUMATISM.—1. *Situation.* Every region of the body may be affected by it, but it is more frequent in the trunk than in the limbs. Lumbago, torticollis, and pleurodynia are the most common kinds; and, when the limbs are attacked, it is in those parts of them which are nearest the trunk. 2. *Etiology.* In many cases it is brought on by cold, fatigue, &c.; but very frequently it depends on that condition of the body called rheumatic diathesis, which will be discussed in another place. 3. *Symptoms.* The essential symptom is pain, increased by contraction of the muscle: this pain is not attended, as inflammatory pains are, by increased heat; on the contrary, a sensation of cold sometimes accompanies it; there is neither swelling nor redness, and generally no febrile symptoms. 4. *Mobility.* It is very subject to change from one muscle to another, sometimes extending to the next muscles, or to the corresponding muscle of the opposite side of the body. 5. *Complication with articular rheumatism.* Sometimes the muscles and joints are attacked at the same time; in other cases alternately. This coincidence has been remarked by all good observers, and it is of importance, as tending to prove the identity of the disease. It is not rare in acute, but it is frequent in chronic rheumatism. 6. *Termination.* Always by resolution. Some cases will be found in print which were supposed to be rheumatism terminating in suppuration; but we fully agree with our author that it is most probable, in these cases, that the diagnosis was imperfect, and that the formation of matter was in the origin overlooked, and the pain consequent upon it mistaken for, or attributed to, rheumatism.

1. *Muscular Rheumatism of the Head.* *a.* In the epicranial region. This may attack the whole or some part of the fibro-muscular layer covering the cranium: sometimes it is confined to the nucha, sometimes to the vertex, sometimes to the sinciput, sometimes to one half of the cranium, when it is called hemicrania. The motions of the occipito-

frontalis muscle and pressure increase the pain, which in some cases is exasperated by everything producing a determination of blood to the head, so that some patients cannot cover their heads or bear the heat of a pillow.

The treatment recommended is, the application of leeches two or three times behind the ears and temples, and exposing the head or merely lightly covering it. If the complaint is obstinate and severe, the head should be partially shaved and leeches applied, followed by a blister: where the pain becomes chronic, it may be advisable to cover the head warmly, so as to encourage perspiration; for this purpose a cap of oiled silk is advisable. Our author gives some sage cautions as to the danger of such warmth in some cases producing apoplexy; and, where such a dangerous tendency exists, he recommends a contrary treatment, gradually lessening the coverings of the head until the patient can bear complete exposure. This fear of apoplexy we take to be very absurd, and calculated to make the young practitioner cautious in trying the most effectual remedy in hemicrania, keeping the part warm.

*b.* In other regions of the head. M. Chomel has sometimes observed rheumatism of the masseter, producing pain and difficulty in mastication. In some rheumatic patients, he has also seen the motions of the palpebræ become painful. The following case is one of rheumatism of the muscles of the eyes.

*Case.* Marie Rabure, æt. 28, servant, was admitted into the Hospital of la Charité, the 19th July, 1815. Both her parents were rheumatic. For some time previously she had been subject, for four or five days together, to pains of the knees or shoulders. Eight days before admission she had suffered from rheumatic pains of the nucha, preventing her turning her head from side to side; and a similar pain in the eyes, so that she could neither elevate nor depress them, nor turn them to either side, without suffering acutely. When she wished to look on one side, she turned her whole body. These symptoms remained for about three days, when she was attacked with typhoid fever, which proved fatal. On examination, no changes were detected either in the nucha or in the muscles of the eyes. Morgagni relates a case in which the tongue was affected with rheumatism, and a similar one has fallen under M. Chomel's observation. The muscles of the pharynx are sometimes affected with rheumatism.

2. *Rheumatism of the Muscles of the Neck, or "Torticollis."* This, which we vulgarly call "stiff neck," is generally produced by cold. The author recommends leeches, if the pain is very severe; in slighter cases flannel, or in obstinate ones blisters; and, if these do not relieve, he applies from a fourth to half a grain of the acetate or muriate of morphia on the blistered surface.

3. *Rheumatism of the Thoracic Muscles, or Pleurodynia.* This often occurs without any manifest cause; sometimes it is produced by cold, and sometimes by the exertion of coughing or sneezing. It is generally confined to a small space on one side of the chest, and very often (like the pain in pleurisy) near the nipple; but it may affect every part of the chest: sometimes one whole side is affected, and this guides the diagnosis; for it very seldom happens that pleurisy or pneumonia produces pain over so extended a surface. Dr. Gaudet, who has published a

series of cases of pleurodynia, found that, in eleven out of thirteen cases, the pain attacked the left side. The importance of this observation could only be satisfactorily estimated by a much larger number of cases. The pain is very intense, much more so generally than in pleurisy; it frequently forces the patient to cry out, which is hardly ever the case in pulmonary diseases. The pain is increased by motion of the trunk, by inspiration, expiration, and by pressure. Lying on the painful side is more distressing than in pleurisy, and very often the pain is aggravated by moving the arm of the affected side. Add to these symptoms the absence of physical signs of pleurisy and pneumonia, the apyrexia, which is the rule in pleurodynia, and the want of cough, except in such cases as the cough is the cause of the disease. But, if pleurodynia continues, it is necessary to be watchful for the occurrence of pleurisy. Hence it is desirable to treat the disease actively from its commencement. Pleurodynia may be confounded with pains of the stomach, spleen, and liver, more easily than with inflammations of the pleura or lungs. A minute examination of the antecedent circumstances and existing symptoms is here necessary to clear the diagnosis. Emollient applications will often suffice to remove a pleurodynia. A hot omelette, and a loaf just taken from the oven, are popular remedies in France. A large cupping-glass, the air of which is well rarified, has succeeded in removing the pain: but, if the pain be obstinate, local depletion, followed by one or more blisters, will be required. Pitch plasters are useful in obstinate but not very active forms.

4. *Rheumatism of the anterior and lateral Parietes of the Abdomen.* This has no particular name. When trifling in degree, it passes unnoticed; but, when violent, it may be mistaken for gastritis, enteritis, or peritonitis: and this accounts for its having been little noticed. The first symptoms presented by such a case are, indeed, very like those of peritonitis. The abdomen is so sensitive that the knees are kept elevated to prevent the bedclothes resting upon it: the most moderate pressure causes intolerable pain, and the expression of countenance is that of peritonitis. But, with these symptoms, the pulse is not excited; there is no fever, no nausea or vomiting; food and drink pass as usual, and the abdomen yields a normal sound to percussion. Dr. Gesnet has observed this rheumatism in puerperal women, and regards the state of the system existing in them as its frequent cause. Many of the cases called puerperal peritonitis he believes to be of this nature; and the violent efforts made by the abdominal muscles in aid of the uterine contractions are regarded as capable of accounting for it; as, in other muscles, forced exercise is an ordinary cause of rheumatism. To this state are referred those cases termed by Dr. Gooch "*nervous affection of the peritoneum.*"* One of the cases quoted by our author from Dr. Gooch died, and no morbid appearances were found. The cause of death in this instance is supposed to have been over-bleeding; and, indeed, such was Dr. Gooch's opinion. But, whether the cases above mentioned were of a rheumatic character, or affections of the peritoneum of a nervous character, it is perhaps difficult, without more information, to decide. In neither case

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* See two cases at pages 63 and 67 of Gooch's "Account of some of the most important Diseases peculiar to Women."

would examination after death afford any satisfactory evidence; for muscular rheumatism appears to be characterized by no appreciable morbid appearances; and it was the absence of morbid changes which was the groundwork of Dr. Gooch's opinion. There are, however, some points of difference which prevent our agreeing entirely with our author on this point. It is said that the abdominal rheumatism is much more obstinate (in respect to treatment) than torticollis or pleurodynia: Dr. Gooch's cases yielded to opium and fomentations very speedily; in one case within twenty-four hours. In the rheumatic cases, it is said that, "together with the most alarming signs, there is no trouble in the pulse:" Dr. Gooch speaks of his cases as accompanied with "pain and tenderness of the abdomen, with a rapid pulse." The French author, laying down rules of treatment, says, after commenting on the "intrinsic tenacity of abdominal rheumatism, or rather the superaddition of peritoneal inflammation," "Be prodigal in (*prodiguez*) the use of leeches, until there is no formal contraindication:" Dr. Gooch speaks of his cases as "not requiring bleeding, as not relieved by it, but as speedily relieved by fomentations and opiates."

We do not, however, regard these circumstances as showing clearly that the two terms do not include the same disease. The mere quickness of pulse, and perhaps the effect of treatment in Gooch's cases, are not of much importance as distinguishing signs; because rheumatism is so varying in its character, and a woman's pulse is disposed to be quickened after labour. We mention them only as facts requiring further observation.

The sign, almost pathognomonic, of abdominal rheumatism is, that pressure, although very painful, does not produce so much pain as motion of the body: hence the patient lies constantly on his back. Peritonitis may be developed during the course of this rheumatism. The knowledge of this fact, and of the obstinacy of the disease itself, must influence the treatment. Repeated use of leeches will not always subdue the disease; but it is, happily, rare to find it resist proper treatment, and become chronic. The copious application of leeches, revulsives on the skin of every kind, baths (in which the patient should be allowed to remain a long while,—four, five, six, or even eight hours,) are the remedies to be employed.

5. *Rheumatism of the Lumbar Muscles: Lumbago.* Under this section we shall only notice the author's remarks upon a case related by Morgagni, of an individual, apparently affected by chronic lumbago, in whose body, after death, the lumbar muscles were found changed in colour and consistence. This appears to be a solitary fact of the supposed structural changes produced by rheumatism in muscles. A young goldsmith suffered from a pain in the right lumbar region, which yielded to no remedies. When this had lasted a year, the left side became similarly affected. The cervical region became also affected with pains, apparently rheumatic. Paralysis of motion of the inferior limbs followed, and tympanitis before death. The thick fleshy mass, which serves as a common origin to the sacro-lumbalis and longus dorsi muscles, was, for about five fingers' breadth, longitudinally and transversely, of the colour of old furniture made of walnut. This change of colour was not alone superficial, but affected the substance of the fleshy mass as well as the subja-

cent muscles. Throughout the whole of this discoloured mass, the muscular fibres were very loose and soft, separated in many places by effused and grumous blood. These lesions were the more apparent the nearer to the spine.*

Is this lesion, says M. Chomel, to be attributed to the rheumatic affection? and he answers the question negatively. Morgagni himself suspects, in order to explain the paralysis of the lower limbs, that a lesion analogous to that found in the muscles existed in the nervous branches constituting the crural plexus. The lumbar pain, doubtless, was connected with this lesion of the muscles: but it is denied that this lesion, which was not traced beyond the loins, and which probably extended to the spinal nerves, was an affection proper to rheumatism. We certainly think with M. Chomel, that from such a case, imperfectly examined as it was, nothing can with certainty be inferred as to the organic changes produced by rheumatism in muscular structures.

6. *Muscular Rheumatism of the Limbs.* This is the more frequent, the nearer to the trunk. It is very mobile, and has a great variety of seats. It springs, as it were, from one limb to another, and from one muscle to another in the same limb. The differential diagnosis of rheumatism appearing in these parts, it will be evident, is important when the various sympathies which exist between them and the other organs of the body and the painful affections hence produced are considered. But it would lead us too far to enter minutely into these. That, however, which distinguishes all these sympathetic pains from the idiopathic pains of rheumatism is, that the former oblige the patient constantly to change his place, in the vain hope of obtaining a more advantageous position; whilst absolute rest is observed by the rheumatic patient. Certain cases of neuralgia, the hysteric pains to which some individuals are subject, the pains in the bones accompanying syphilis, those which are produced by lead, and those which attend scurvy, are referred to, and distinguished from the characteristics of muscular rheumatism of the extremities. It is only necessary for us to mention them, to show in what the differential diagnosis must consist. The treatment of this is to be conducted on the same principles as that of the previous forms.

ORDER II. ARTICULAR RHEUMATISM. We will here again remind our readers that M. Chomel includes gout under the above head, believing, as he does, in the identity of the two diseases. The seat of the disease he considers to be the fibrous tissues surrounding the joints. Whilst there are points of resemblance between it and muscular rheumatism, there are also other distinctions than those of situation. There are more signs of inflammation in articular rheumatism, and its phenomena are sometimes witnessed on the external surface. It appears more frequently to originate without apparent external cause; it is more mobile; it is much oftener accompanied with fever, and, as a general rule, is of much longer duration. Articular rheumatism is generally ushered in by a number of febrile symptoms, which but rarely precede muscular rheumatism; and it is accompanied by other symptoms, varying in severity with the number of diseased articulations. When a limb is attacked with muscular rheumatism, it is rare that the joints contiguous to the rheumatic muscles become affected; but the converse is not true: and, lastly, the articular is less amenable to treatment than the muscular rheumatism. The importance attached by the author to the study of causes has given rise to some long chapters on the subject; but, in our opinion, he omits to notice the most important consideration of all,—viz. the relation of the different forms of the disease, or, as we would say, the relation of the two different diseases, gout and rheumatism, to these causes respectively. It is admitted by M. Chomel that there must be an internal *predisposition*, although we know not in what this consists. There is also a great disposition to recurrence of the disease, when once the body has been attacked by it; and the oftener that it has happened, the greater is the danger of its return: this return being occasioned frequently by the most trifling causes, but most frequently succeeding to an impression of cold, excess at table, or too much venereal indulgence. A regular and temperate mode of living is the best method of preventing relapses, which are occasionally found to be periodic. The disease is hereditary, a fact established by M. Chomel in thirty-six cases out of seventy-two; and this number he regards as sufficient to show that such a mode of transmission is certain. No predisposing causes are of such importance as the hereditary disposition and the previous occurrence of an attack. All the external predisposing causes are obscure. It is not denied that the combined action of cold and moisture contribute much to engender the rheumatic disposition. This fact is illustrated both by the history of the disease in different climates and in different seasons. Youth and manhood appear to be the ages most subject to attacks: the first attack is generally between the fifteenth and thirtieth year. Of 73 cases, M. Chomel found 35 who had been first attacked between the ages of fifteen and thirty years; 22 between thirty and forty years; 7 between forty-five and sixty; 7 after sixty years; 2 before fifteen, one aged nine, the other aged ten years. When the disease attacks young individuals, it is almost always indicative of hereditary disposition. Men are more subject to the disease than women; the sanguine than the other temperaments. The *occasional causes* of rheumatism would have required or obtained from M. Chomel little notice, had not M. Bouillaud maintained that they reduced themselves to a single one,—the action of cold, particularly in combination with moisture. The observations of the latter are all of a positive character: he is evidently too zealous an admirer of pathological laws. Of fifty patients whom he carefully examined, all owed his or her rheumatism to cold. But, since the controversy which has existed on this question, nine patients, who entered the Hôtel Dieu during November and December, 1835, and January, 1836, were most carefully questioned; and in two only of these cases did cold appear to have acted as the determining cause.* We quite agree with M. Chomel that cold is not a *constant* and *necessary* antecedent in the production of acute articular rheumatism; that, among other causes, it may be occasioned by the suppression of various long-established or habitual discharges, &c. But, in the consideration of M. Bouillaud's opinions, we must remember that he has not contended for the identity of rheumatism and gout; and, unless the strict meaning of these words is laid down,—unless, as a preliminary question, it is decided whether they mean to MM. Bouillaud and Chomel the same or different diseases, it is clear that no conclusions can be satisfactorily arrived at as to the influence of cold as an exciting cause. As a cause of what is called acute articular rheumatism in this country, it is doubtless most frequent; as a cause of what we term gout, it is probably rare: and herein partly lies the difference in the opinions of our authors,—M. Bouillaud, who recognizes rheumatism as a distinct disease, attributing it to cold and wet; M. Chomel, who associates all the forms of gout with rheumatism, finding that some other cause besides cold must be sought for to explain its occurrence. M. Chomel has added two cases to those already recorded by Dr. Murray, in the Edinburgh Medico-Chirurgical Journal, of rheumatism occurring in the decline of scarlatina.

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* Morgagni, Epit. lvii. 17.

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* We shall hereafter refer to these nine cases, recorded by Dr. Grisolle, chef de clinique, in the Journal Hebdomadaire, No. 13. 1836.

It would be superfluous to give a minute description of acute articular rheumatism; but there are some facts, and several questions arising out of them, well deserving our attention.

*Invasion.* The fever of invasion, says M. Chomel, is generally long and intense in proportion to the number of inflamed joints, and hence it is of some value as a guide in prognosis. With the partial rheumatism (i. e. of one or two joints,) there is but little fever; though during the nocturnal reaction there may be some: but, if the fever is very strong, internal organs should be most carefully examined, to ascertain whether any internal affection coincide with the rheumatism.

If no internal affection exist, there is no doubt that a general rheumatism is on the point of declaring itself. The increase and diminution of disease in a single joint do not take place in a continuous manner, but with exacerbations during the night and remissions during the day, both when the disease is on the increase and the decrease. Recurrence of disease is more frequent after partial than after general rheumatism: hence this partial rheumatism is important as a signal of attacks to come. Of itself, it is of short duration, and ceases spontaneously after fifteen days at the latest. Febrile symptoms announce acute general rheumatism; doubtless the product of a hidden cause which is about to show its special character by fixing on the joints. There is nothing in the fever which marks its speciality. The general symptoms persist sometimes for two or three days before the joints are affected. The view of the nature of the disease to which these facts lead is, that rheumatism belongs to the class of essential fevers. We shall hereafter notice M. Chomel's remarks on them. M. Bouillaud, however, is quite of the contrary opinion: he sneers at essential fevers, as mere imaginary existences; he regards this disease as a local inflammation with symptomatic fever. The general opinion, however, in this country will, we believe, be, that although the local affections may accompany the general from their first appearance, still they are not to be considered as holding the relation of cause and effect. If it be admitted (and M. Bouillaud does not allude to the fact) that the fever may precede the affection of the joints by two or three days, we cannot see on what ground the fever can be regarded as symptomatic. There are some observations on the mobility of the inflammation of the joints, made by M. Bouillaud in support of his opinion that rheumatism is an inflammation with symptomatic fever, which we here quote.

He says that this characteristic of rheumatism has not yet been investigated with sufficient care; that it is not correct to believe that acute articular rheumatism can pass from one part to another, or rather be dissipated, either easily or quickly. If, for example, the knee is much swollen, with abundant effusion into the joint, examine whether the rheumatism is terminated by *metastasis* or by *delitescence*. Two very different things are here confounded. It is true that, even in this case, the pain in the joint may suddenly disappear, with or without the occurrence of pain in another articulation; but the same cannot be said of the articular effusion, which, however, constitutes the essential element of the disease. The pain is but a symptomatic neuralgia of the affection of the joint, similar to "stitch" in pleurisy; and in each case this symptom constitutes rather an accident than an essential characteristic of the disease: for pleurisy may exist without pain, and so also may articular rheumatism, which is but, as it were, a pleurisy of the synovial membranes. To say that articular rheumatism with effusion can be suddenly displaced, is equal to supposing that pericarditis with effusion may disappear in the same manner; a hypothesis rejected by sound observation. These remarks, which we do not believe to apply to every case of acute articular rheumatism, because the effusion frequently appears to be *external* to the joint, and not within the synovial membrane, in no way confirm the assertion of rheumatism being a simple inflammation of the joint.

The paroxysms both of general and partial rheumatism, says M. Chomel, are during the night, and show themselves either by more pain or by increase of the number of joints affected. Throughout the whole course of the disease there is no necessary agreement between the fever and the articular symptoms. If the joints cease to suffer, but the fever continues, and if an examination of the viscera does not explain this by the sudden development of some internal inflammation, there is no doubt of the near reappearance of the articular pains. This is mentioned as a fact which M. Chomel was the first to point out, and a case is given, and others are referred to, in illustration. M. Bouillaud rejects as absurd the idea of a fever without an inflamed joint or organ. To explain this sort of pathological mystery, he says,—to localize *rheumatic fever without rheumatism*—it required but an attentive and exact examination of the circulating system (including the blood), and of the heart in particular. He contends that inflammation of the pericardium or endocardium is the rule in acute articular rheumatism; and that he has discovered the most satisfactory explanation of the rheumatic fever, which others have wished to *essentialize*, in the existence of a rheumatic affection of the heart, vessels, pleuræ, &c.; that these inflammations are almost always indolent, and have consequently remained undiscovered until now. In answer to the mystery supposed to attach itself to the existence of rheumatic fever, independently of local inflammation, Chomel rejoins, Does not fever precede, by twenty-four hours or more, the inflammation of the articulations? The fever is not in relation to the articular affection, as effect to cause: it is primitive, it is independent. Is it difficult to imagine the persistence of fever during the suspension of the local symptoms? There is a common cause of both, unknown but real. The phenomena of eruptive fevers and typhus are appealed to as analogous, and the case is considered as no more mysterious than those of variola, scarlatina, or measles, without the exanthema; a pathological fact now pretty generally admitted. But the question is one which must rather be decided by an appeal to facts. On the morbid anatomy of the question our authors are completely at issue, and, as it is one of considerable importance, we must enter somewhat into its details.

One object of M. Bouillaud's researches is to define the exact relation which exists between pericarditis and endocarditis, and acute articular rheumatism. Quoting from his previous work on Diseases of the Heart, he says, "Pericarditis exists in about half the number of individuals affected with acute articular rheumatism," (p. 8;) "that the pericarditis and endocarditis of rheumatism almost always accompany one another," (p. 9.) But, at page 11 of his present work, he says that, "from his calculations, it is shown that inflammation of the pericardium and endocardium coincide with articular rheumatism, in the proportion of one third." This difference is comparatively unimportant; but if, as it is maintained, the diseases can be detected during life by a skilful physician, we do not see why the proportions should be stated with so much uncertainty as to vary from one half to one third. M. Bouillaud's opinion is, that "this coincidence is the *rule*, and the non-coincidence the *exception*." The symptoms of pericarditis occurring in an individual affected with acute articular rheumatism are, "dullness of the præcordial region, much more extended than in the normal state, (twice or thrice in every direction;) an arching of the same region; pulsations of the heart remote, little or not at all sensible to the touch; sounds of the heart distant, obscure, accompanied by different abnormal sounds,—some depending on the friction of the opposite surfaces of the pericardium, others on the complication of valvular endocarditis. Pain more or less acute of the præcordial region, palpitations, irregularity, inequality, and intermittence of the pulse are sometimes associated with the preceding symptoms." With these symptoms M. Chomel is agreed, and he does not wish to throw any discredit on M. Bouillaud's cases: but he claims for his own observations the same credit, and alludes to those already mentioned, as recorded by M. Grisolle, in which, after a most careful examination, seven out of nine were found to be entirely free from any sign or symptom of heart affection. He, therefore, most properly objects to any pathological *law* being founded on a certain number of cases, observed by one individual only. In this opinion of M. Chomel we quite concur, and for other reasons than those mentioned in his book. We shall presently see what right M. Bouillaud has to the credit of having made any discoveries as to the coincidence of rheumatism and inflammations of the heart. The fact has been long known in this country; and any one who has had an opportunity of observing cases of acute rheumatism knows that the heart is *not* affected in the majority of cases. The question as to the frequency of the coincidence is one which time only can decide. The reports relating to rheumatism, lately made by Dr. R. Macleod, in the Medical Gazette, are a valuable contribution towards such a decision. In Grisolle's cases, two out of seven showed signs of affection of the heart. Of eighty-five cases related by Dr. R. Macleod.

the heart is said to have been implicated in eighteen, or in rather more than one fifth.

Much of what has preceded applies also to endocarditis. Bouillaud states that it almost always accompanies pericarditis; and "I regard its existence as certain," he adds, "when the following signs exist: *Bruit de soufflet, de râpe,* or *de scie* in the præcordial region, which is dull on percussion over a surface much more considerable than in the normal state, and is also arched, though less than in pericarditis with effusion; the pulsations of the heart lift with force the præcordial region, and are frequently irregular, unequal, intermittent, accompanied sometimes by a vibrating '*frémissement*.' The pulse is hard, strong, vibrating, intermittent, like the actions of the heart." The *differential* signs of the two affections are not always very marked; so that it is sometimes difficult to say whether pericarditis or endocarditis, or the two diseases combined, exist. Such cases are those where the inflammation of the pericardium exists without much effusion, and with the formation only of false membranes. Then the pulsations of the heart are sensible to the touch, as in simple endocarditis; and the *bruit de scie* or *de soufflet,* the *frémissement* of the præcordial region, may be found as in the case of endocarditis. We have already expressed the opinion, whilst commenting on M. Bouillaud's treatise on Diseases of the Heart, "that, in the present state of our knowledge, the distinction (between endocarditis and pericarditis) will often be impossible." (*Brit. and For. Med. Review,* vol. ii. p. 328.) There is nothing in the above catalogues of symptoms to make us alter that opinion. The law (as it is termed) of the coincidence of endocarditis with acute articular rheumatism is, in M. Bouillaud's work, an inference from symptoms alone; for the cases which he treated recovered. To say the least, then, there is no foundation for the law but very uncertain symptoms. In one only of the seven cases of Grisolle was there a *bruit de soufflet;* and in this case copious bleeding had been employed. M. Chomel thinks that, in many of the cases given by M. Bouillaud, the *bruit de soufflet* and its modifications may have been the consequence of the enormous bleedings to which he subjected them, (sometimes taking as much as eight pounds.) Dr. Marshall Hall's experiments on the occurrence of this sound from loss of blood are alluded to, and a case is given in which the loss of a considerable quantity of blood, at different times, was followed by a *bruit de soufflet;* for which such evacuations afforded the most plausible explanation. But we cannot admit this explanation, unless it be shown that the anormal sounds are always subsequent to the bleedings, and that they do not coexist with other signs of affections of the heart. There is an unfairness of criticism in many of the remarks of both the authors, the avoidance of which would have obtained them credit for a desire for the prevalence of truth, instead of for that of their own favorite opinions. M. Chomel does not reject the possibility of endocarditis, but quite the contrary: he gives a case which died, and afforded an opportunity of proving the point. With regard to the precise frequency of the occurrence of endocarditis with rheumatism, we do not know where the facts are to be found on which a safe opinion can be formed. It is a question, as we have said, for time to decide, and one which those who have opportunities of examining cases of rheumatism will do well to bear in mind.

There are other serous inflammations to be looked for with acute articular rheumatism: these are chiefly in the pleura, rarely in the arachnoid or peritoneum. During the disease, internal organs, and particularly the chest, must be carefully examined. That he may not be taken by surprise, M. Chomel examines the chest every two or three days; even in the complete absence of all apparent disturbance of the circulation and respiration. This is a most valuable caution, and one which, from our own experience, we should strongly urge,—if it required to be urged by anything beyond the knowledge of the insidious and latent form which these inflammations assume.

It is remarkable that, notwithstanding what has been so long known, in this country, of the connexion of rheumatism with inflammation of the heart, and considering the great contributions made by French pathologists to our knowledge of cardiac diseases, that Corvisart only incidentally names rheumatism in conjunction with gout, as the source of one of the three forms of adhesion of the pericardium to the heart; that Laennec leaves unnoticed all such causation; whilst M. Bertin, in his excellent work on Diseases of the Heart and large Vessels, published but twelve years ago, in the composition of which M. Bouillaud was his collaborator, and M. Louis, in his treatise on Pericarditis, observe a similar silence.

The question of priority of discovery has been warmly contested by our two authors. Bouillaud says that "the subject of his researches which is most novel and curious is, without contradiction, the coincidence of inflammation of the external and internal sero-fibrous tissue of the heart with acute articular rheumatism. The date of the publication of his volume is 1836, but he states that he had collected the cases which enabled him to discover this important relationship about three years before, viz. in 1833. We have seen reason to doubt the evidence which he has given in favour of the frequency of such coincidence; and therefore, before examining at all the history of the discovery, we should repeat that, although pericarditis is found to exist frequently with acute articular rheumatism, it is not so in half or one-third of the cases; and that there is not the same amount of proof in favour of the existence of endocarditis which there is in favour of pericarditis. We think that M. Chomel has satisfactorily shown that the charge of ignorance of the occurrence of rheumatism and pericarditis, which M. Bouillaud makes against him, is unfounded; and that, in 1813, in an inaugural thesis he had written, "I have myself seen pericarditis succeed to rheumatism, and cause the death of the patients." In 1826, M. Chomel wrote in the "Dictionnaire," in twenty-one volumes, "Pericarditis has been most often observed either with pleurisy or pneumonia, *or in the course of acute articular rheumatism*." Bearing in mind how much we consider to be proved of the coincidence of pericarditis and rheumatism, M. Chomel may fairly claim precedence of M. Bouillaud, at least in having expressed such knowledge. But M. Chomel does not claim the merit of having discovered rheumatic pericarditis. He regards it as a discovery made "par tout le monde, peu à peu par le travail commun." In fact, M. Bouillaud appears to have been singularly ignorant of the prevalent opinions on this subject, and to have discovered what almost everybody else, at all acquainted with the literature of medicine, must very well have known. In this country, at least, such has been the case. Dr.

Baillie, in his Morbid Anatomy, informs us that Dr. Pitcairn was "the first person who made this important observation;" and so long ago (as we are told by Dr. Elliottson, *Diseases of the Heart*, p. 9,) as the year 1788. Since the publication of Dr. Baillie's work in 1797, the same fact has been noticed by innumerable English writers, and, among others, by Sir David Dundas in 1809, and Dr. Wells in 1812. In 1824, it was formally noticed in Dr. Cox's compilation, On Rheumatism and its Metastasis; and still more so, in 1826, by Dr. Hawkins, in a work principally dedicated to the subject. In Dr. Elliottson's Lumleyan Lectures, read in 1829, we not only find the complication noticed as a fact generally known, but the very same practical caution given as that announced by M. Chomel. He says, "I make it as invariable a rule to examine the cardiac region by the touch and hearing, in every case of acute rheumatism, as the usual seats of hernia are examined by us all in cases of colic and intestinal inflammation." It is also but justice to Dr. Elliottson to state that he refers the affection of the heart, in all cases, to pericarditis in the first instance.

We have stated our doubts as to the extreme frequency of the complication of endocarditis with acute rheumatism, as mentioned by M. Bouillaud; and we must be allowed to tell him, also, that the fact (although he regards it as new) has been long known on this side of the Channel. In the very excellent work of Dr. Brown, of Sunderland, published ten years ago, the writer, describing the appearances found on dissecting cases of rheumatic cardiac affection, after stating that the pericardium is the principal seat of the disease, thus expresses himself: "The membrane lining the organ has participated in the affection. On detaching the abundant lymph which adheres strongly to the valves and chordæ tendineæ, it is generally found highly vascular."* It is more formally noticed by Dr. Elliottson, in the Lectures above referred to. At a period much later, but certainly preceding the publication of M. Bouillaud's work, although not anterior to his observations, the recognition of the affection was still more fully described by Dr. Watson, in two clinical lectures delivered at the Middlesex Hospital, February 28, and March 4, 1835, and which were afterwards published in the Medical Gazette. A brief extract from the first of these very excellent discourses will suffice to show Dr. Watson's opinions as to the nature and frequency of the affection termed endocarditis by M. Bouillaud.

After detailing the general symptoms of the affection of the heart, which accompanies or follows acute rheumatism, and which affection he states to be of "extreme frequency," he proceeds:

> "And what are the parts upon which the inflammation fastens? They are the membranous parts of the heart; its investing membrane alone, or its lining membrane alone, or (what is infinitely the most common) both of these membranes together. On the exterior of the heart, it is probable that the inflammation begins in the fibrous texture of the pericardium, and then extends rapidly to the serous. Here it produces its usual consequences—the effusion of serum; the deposition of lymph; adhesion, general or partial, of the opposite surfaces of the membrane. The inflammation usually spreads over the greater portion, or even the whole of the serous surface; &c." . . . "When the inner membrane is affected (and very seldom, I believe, does it escape, although its alterations may sometimes elude an inattentive observer), the

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* Medical Essays, by J. Brown, M.D. p. 159.—*London*, 1828.

inflammation exercises (if I may so speak) a kind of preference; its effects are limited, in a great measure, to the valvular apparatus of the heart. Occasionally that naturally transparent portion of the membrane which covers the muscular fibres is thickened, and rendered whitish and opaque; and occasionally some of the deposits that are common on the valves encroach also somewhat beyond them, or even stud, here and there, the interior of the several cavities. But the valves, or the cartilaginous rings from which they spring, are the parts first and chiefly implicated, especially the mitral valve, and the aortic valves, not uncommonly the tricuspid valve also; and sometimes even the semilunar valves of the pulmonary artery." . . . "The valves themselves become thicker—they lose their transparency and pliancy—they become puckered; sometimes they are folded down and glued, as it were, to the opposite surface; but more frequently than all, they present small wart-like granulations or excrescences—what the French call *vegetations*." . . . "These are the appearances commonly seen when the patient does not long survive the first attack of rheumatic carditis; for under that name I may now speak of the disease we are considering. When death takes place at a later period, you find more than this; you find the consequences which flow from these primary lesions operating as mechanical causes of further change—hypertrophy, and dilatation, in their various degrees and combinations."

In his zeal against M. Bouillaud's opinions, M. Chomel states that the pleura is as often inflamed in acute rheumatism as the pericardium. This is but a statement, and one which we believe is incorrect. Andral, however, gives, in his Clinique Médicale, two cases, (vol. ii. p. 502-504;) one of which was latent, the other not so. Affections of the peritoneum and arachnoid membrane in acute rheumatism are rare.

Except in the cases mentioned, says M. Chomel, the *termination* of acute articular rheumatism is favorable, and rheumatic pericarditis and pleurisy are less dangerous than the ordinary forms of these diseases. Can it end in suppuration? Cases have been recorded, apparently of such termination. But, since pathological anatomy has been more studied, it is very probable that metastatic abscesses, and purulent collections after common inflammation have been set down as instances of rheumatism ending in suppuration. Chomel says that pathological anatomy has been as vainly interrogated with regard to acute articular as with regard to muscular rheumatism; and that, to speak truly, there is none, either for the one or the other, in the actual state of science. Bouillaud is, of course, of the contrary opinion; and instances cases, in his book, of rheumatism ending in suppuration. To some of these, only, has Chomel answered; and as, in these cases, there was phlebitis, or other conditions than those of simple rheumatism, we agree with him in his objections to them. At page 85, however, of M. Bouillaud's book, is a case, communicated by M. Raciborzki, which appears to be a fair example of rheumatism ending in suppuration. There is certainly a great want of evidence on this subject; but, while we doubt the frequency of suppuration as an effect of acute articular inflammation, we by no means agree with M. Chomel that it has never been shown to exist. Few as the cases are, says M. Bouillaud, they serve to show that the termination by suppuration or purulent effusion is not foreign to acute articular rheumatism, and that is all that could be claimed for it.

Our two authors are at issue, also, as to the *duration* of acute articular rheumatism. M. Bouillaud says that it is in relation to the treatment: M. Chomel thinks that treatment cannot with any certainty stop or even abridge it. He says that its duration is very uncertain; that its average is three weeks; that this uncertainty renders any inferences as to the action of medicines very doubtful. What are called *good* cases show themselves under the most different forms of treatment; i. e. cases which last from twelve to sixteen days. This spontaneous termination is, of course, a very important matter to decide, in estimating the value of any treatment. We believe that Louis has lately suffered several cases of acute rheumatism to take their own course, and that he has found their average duration to be three weeks. Show us a medicine, says M. Chomel, which, in forty cases, always cures in fourteen days, and then there will be no reason to doubt its efficacy. However, although we cannot dissent from M. Chomel's principle, we should not conclude that a great degree of probability was not afforded of the utility of any system of treatment, if, *cæteris paribus*, it considerably reduced the average duration of the disease. We shall pass over the squabbles of the rival doctors on this question; the object of M. Bouillaud being to show that hitherto the duration of the disease has been very long, that he may magnify the more his own system of treatment; and of M. Chomel to reduce this average duration as much as possible. If they were not such useful physicians, we should almost lament that they had not been educated as advocates, they are such thorough partisans. Having got the clue, you can tell at once what their conclusions will be, on many points; a circumstance very unsatisfactory for readers who cannot participate in their insignificant animosities.

Chomel examines successively the various *remedies* which have been employed in cases of acute rheumatism. *General Bleeding.* His own practice is, neither to bleed profusely nor to interdict bleeding; employing it as a means simply of moderating the fever. We need not follow him in his examination of the various authorities for or against bleeding. M. Bouillaud's opinion on the subject is thus expressed: "The true specific of acute articular rheumatism, its *quinine*, if the expression is allowable, is the antiphlogistic system, and bleeding is the prince of antiphlogistics." But this *quinine* of rheumatism is bleeding after a regulated formula, termed "*coup sur coup.*" It does not merely concern the quantity of blood taken, but the mode in which it is taken. Its panygeric shall precede the description of this mode.

> "The success obtained by this new formula of bleeding is such, that unless it was seen, it could not actually be credited. I am not therefore surprised at the *philosophic scepticism* of some individuals. . . . By the use of the new formula, the average duration of acute rheumatism, is, from one to two weeks, only, instead of from six to eight." [The formula is as follows.] "*The day* on which the patient enters (supposing him of strong constitution and at a vigorous age) at the evening visit, he is bled to sixteen ounces. (In very sanguineous subjects, this bleeding is sometimes carried to twenty or twenty-four ounces.) *Second day.* Two bleedings of from fourteen to sixteen ounces, and in the interval of the two bleedings, a local bleeding either by leeches or cupping (the latter is preferred). By this local bleeding, twelve, sixteen, or twenty ounces of blood are drawn. The cupping-glasses are applied around the most inflamed articulations, and upon the præcordial region, when the heart is seriously affected, that is to say, in the great majority of cases. *Third day.* A fourth bleeding similar to that of the evening of the previous day, and a second cupping (from twelve to sixteen ounces) either upon the precordial region or around the articulations. *Fourth day.* The fever, the pains, the swelling, in a word, the whole inflammation sometimes ceases from the fourth day. In this case, further bleeding is not performed; in the contrary case, another bleeding of twelve or sixteen ounces is performed. *Fifth day.* In general, the resolution of the disease is in full activity. But in very serious cases, the rheu-

> matic fever may still be very marked; and twelve ounces of blood are drawn from the arm, or the same quantity is taken locally. From the sixth, seventh, or eighth day, the convalescence is established, and nourishment may be commenced. If serious relapses occur (the *new formula* does not secure the patient against them, but perhaps exposes him to them less than the *old*) it may be necessary to recur to the bleeding. It is thus that in a case where four bleedings had stopped (jugulé) a serious attack of acute rheumatism, a violent relapse took place, and this could only be subdued by five new bleedings. If the relapses are mild, emollients, abstinence, baths, opiates, &c. may suffice. To prevent relapses, the patient should most carefully guard against any exposure to cold. The additional means to bleedings thus practised, are,—abstinence, emollient drinks, blisters, compression of the diseased articulations, the application of mercurial ointment upon compresses; position; emollient poultices; baths; opium at a moderate dose, internally or endermically. The medium quantity of blood taken from vigorous subjects, in a violent case, is from four to five pounds. In some cases it may be necessary to draw six, seven, or even eight pounds of blood. But in moderate cases the dose drawn need not be more than two or three pounds." (p. 133.)

Such is the plan of bleeding, "*coup sur coup*," of M. Bouillaud. Of course, the only claim to any novelty in this system must be the manner in which it is performed; and those who have instanced the inutility of large bleedings in their hands receive as a reply from M. Bouillaud, "you did not bleed according to my formula;" and we must admit the fairness of the reply; although the objection would hardly apply to the practice prevalent in Edinburgh some thirty or forty years since, while the Cullenian doctrines and practice were still in full force. When we remember that acute rheumatism is not a fatal disease; that it runs a certain course when no treatment is employed; that, under the influence of remedies well known in this country, and of which, in the practice of many, bleeding never forms a part, its termination is favorable; we are utterly unable to justify such sanguinary practice as this of M. Bouillaud. Were it shown that it was a means of materially abridging the disease, we should regard this alone as a very trifling argument in its favour. We ourselves would rather suffer a certain number of days from rheumatism than from the after-consequences of the loss of four, five, six, seven, eight pounds of blood! Of the history of these patients after they have left the hospital we hear nothing, but it is the most important part of it. The cure is considered complete when the poor wretch has left the hospital free from rheumatic pains; the hospital which receives him next is left to the reader's imagination. But, without denying any of M. Bouillaud's facts, M. Chomel has taken some pains to show that the treatment does not abridge the average duration of the disease. We regard the average duration as not very well settled, and some exception might be taken to M. Chomel's observations. Facts already stated (and, among others, the spontaneous termination of the disease,) will excuse our entering into the niceties of M. Chomel's calculations to show that this new treatment cannot, on the evidence produced, be said to shorten the duration of the complaint. In the main we agree with M. Chomel, although he has taken some little advantage of his opponent in some points of his calculations.

In his remarks on the effects of *sudorifics* in the treatment of acute rheumatism, M. Chomel does not speak from experience. Theoretically he condemns them; but we must protest against all theoretical condemnation of any remedy which is spoken well of by many who have employed it; and, although the cases are few in which, from our own experience, we should regard sudorifics as appropriate, we know that they are highly estimated by others. In the Infirmary of Edinburgh they have been long employed; and this would argue somewhat in favour of their utility. Narcotics generally, but especially opium, are commended when the fever has lost its primitive intensity, or if the disease was originally apyretic. But on this point M. Chomel says nothing satisfactory. Of the use of the medicine he knows evidently very little or nothing. In fact, his whole chapter on Treatment is by far the worst in the book. Like many others of his class in Paris, the history, diagnosis, pathology of a disease, with hot squabbles about unimportant matters, absorb all his talents and zeal; and his feebleness shows itself in the discussion of its treatment. The use of opium in acute rheumatism is a matter of considerable importance; and there can be no doubt that it occasionally acts in a way justly entitling it to the name of *heroic*. This result we have ourselves witnessed in some cases; and we think that, if preceded by due depletion, it is often not merely a safe but a very valuable remedy. The attention of the profession in this country was particularly called to this remedy by Dr. De Roches, in a paper published in the first volume of the Ed. Med. and Surg. Journal; although it had been often employed before, in different forms. Sydenham was adverse to the use of opium in this disease, but in his opinions respecting its proper treatment, he appears to have been much influenced by theoretical considerations. Dr. Heberden, in speaking of rheumatism, remarks, "Meo judicio, opium non tantum modo importuni mali præsidium est, sed multum confert ad ipsum malum tollendum." In the paper of Dr. De Roches referred to above, the author states that the induction of perspiration is essential to the success of the remedy, and our own experience confirms this: unless perspiration is excited, the patient's sufferings will be aggravated, in place of being relieved. The fulfilling this indication seemed the chief object of treatment at the Edinburgh school in the beginning of the present century. The late Dr. Gregory recommended, with this view, the administration of Dover's powder in doses of ten grains every two or three hours, with warm diluent drinks and close wrapping in blankets. We are far from recommending the indiscriminate employment of opium, in any form, in acute articular rheumatism, but we are convinced that it as little merits to be rejected as entirely useless.

The moderate use of *purgatives* is not objected to by M. Chomel; but the preference is given to clysters, particularly as they can be administered without requiring that the patient should be disturbed. Indeed, one of the chief objections to the use of purgatives in the disease is, that any movement of the body is attended with so much pain. As usual, M. Chomel misunderstands and misrepresents the views with which calomel is employed in this country in acute rheumatism. He appears to have no idea of its actions, except as a purgative and a sialogogue, and he consequently speaks ignorantly of "cette panacée de la tourbe medicale de l'empire Brittanique." We cordially agree with him in condemning its still too frequent employment; but he must have witnessed the indubitable effects of this mineral, when neither employed to purge nor to salivate, before he can be considered as a competent judge in the matter.

*Colchicum* he used in one case, on which he thus comments: "in short, colchicum appears to us to be a remedy little to be relied on." This very short condemnation is an example of the nature of much of his remarks on the treatment of rheumatism. If, theoretically, he disapproves of a remedy, or if in one case he has found it fail, or if he has not attended to the conditions under which its employment (by those in the habit of using it) is recommended, he does not hesitate to exclude it as a remedy unworthy of consideration. His disapproval does not, in the case of colchicum, appear to be at all influenced by the fact that "it is used with very happy effects in the large hospital of Westminster" (a remarkable instance, by the bye, of his acquaintance with what is going on in this country,) or that his "savant confrère et ami le docteur Magliari" has published instances of "de remarquables succès avec le vin de colchique." It is not necessary for us to point out to our readers the great value of this remedy in rheumatism, and its proper mode of employment, as these are generally well understood and appreciated in this country. For the best information which we know on the subject,—and we may add for much important information of a pathological kind not to be found in the works of MM. Chomel and Bouillaud,—we refer our younger readers to the two admirable articles, On Gout and Rheumatism, by Dr. Barlow, in the *Cyclopædia of Practical Medicine*.

Of *Quinine* and *Digitalis* M. Chomel has had no experience. *Tartar emetic* he has employed, without having any reason to repeat it. A solitary case to which *mercurial frictions* were applied was an instance of failure; "so that we are authorized in considering mercurial frictions as entirely deficient in efficacy in cases of acute articular rheumatism." This most legitimate inference would almost lead us to suspect that these frictions had been recommended by M. Bouillaud. In acute, partial, or apyretic articular rheumatism, *the endermic application of the salts of morphium* is recommended, on the experience of two cases which are quoted.

The *principles of the rational treatment* (as it is called) of rheumatism are, *according to M. Chomel*, established "conformably to an eclecticism counselled by experience;" although it is not easy to gather from the remarks made by him on the remedies above mentioned, whether this eclecticism means the same to him that it does to other people. We should rather think not. His system appears to us to exclude eclecticism; to be founded on his own views of the nature of the disease; a treatment, in fact, based on hypothesis. Be this as it may, the rational treatment of this disease, according to M. Chomel, is as follows:—Bleeding, in young and robust subjects, once or twice, to moderate the fever and the local inflammations, and to prevent metastasis; but this should never be carried to such a degree as to diminish the strength considerably; for weakness lengthens convalescence and predisposes singularly to relapse. Local is less useful than general bleeding; but, if the pain is so intolerable in any joint that there are convulsive movements of adjoining muscles, it must be employed. Leeches are better than cupping, because their application is attended with much less pain. Sometimes emollient poultices are useful. Warm baths, though indicated, give rise, from the lifting of the patient, to great pain. Cooling drinks, such as whey, &c., are pleasant. The apartment should be of moderate temperature; 60° Fahr. The bed should be firm rather than soft; never a feather-bed. When the pains are very acute, and are much increased by motion, a bed should be used where the patient is lifted by means of straps. Position is important, and should be such as to allow the free reflux of blood from the inflamed parts. The duration of the disease is such that a too rigorous abstinence must not be enjoined. Towards its termination, diaphoretics are advantageous: the best of these are vapour baths. The occurrence of pericarditis or pleurisy during the course of the disease, requires, of course, to be vigorously treated. As far as these rules of treatment extend, they quite meet our approval, although M. Chomel does not give us cases illustrative of its advantages. When he adds to his remedies colchicum, and employs it judiciously; when he is aware of the real object with which calomel is employed in this country; and, we may add, when he has discriminated the cases in which sudorifics are apparently useful, we doubt not that he will rewrite his chapters on treatment, and thereby very much increase their utility; and that he will treat his patients with more success, and perhaps be disposed to think that the disease is capable of being abridged by the adoption of an "eclecticism counselled by experience."

*Chronic articular rheumatism* has so great an analogy with the acute that we shall chiefly deal with its points of difference. It has two very distinct forms; the one succeeding to the acute, the other commencing with its own proper characteristics. Each of these may be subdivided into the *mild* and *intense.*

*Mild Chronic Articular Rheumatism.* This differs from the mild acute form only in its minor degree of mobility, and in its duration. Instead of an excess of heat, it produces often a constant sensation of cold in the part. The variations in its course are the same as those of the *intense chronic articular rheumatism.* Here there is complete inability to move the joint, which is generally swollen and deformed,—the joint surrounded by hard and prominent tophaceous concretions. If these concretions are recent, they may be gradually dissipated; if of long standing, this is no longer possible. They gradually render the skin which covers them thinner: they are chiefly observed in the smaller articulations. If limited to a few articulations, the disease does not endanger life: if, however, very intense, attacking many articulations, and giving rise to violent fever, death may follow. Sometimes eschars form, subsequent to an erythema, on those parts on which the body rests; an incurable ulcer follows, the cause of its production effecting its extension, or death takes place from suppuration. Rheumatic hectic fever sometimes extinguishes life. This termination is rare in private practice, but is frequent in hospitals for old men. In chronic articular rheumatism, there are often nocturnal exacerbations; easily calmed, however, by opiates. A damp cold season is most unfavorable; a warm dry atmosphere produces marked relief, if not a radical cure. The duration of chronic rheumatism is from three to four months; but it may be prolonged indefinitely, particularly when tophaceous concretions have taken place.

The *Prognosis* is favorable, and complete recovery may be predicted if, notwithstanding the fixedness of the pain, the articulations are still capable of being moved.

The *Diagnosis* is not always very easy. The diseases termed *white swellings*, and syphilitic diseases of the joints, are those with which rheumatic affections may be confounded. Respecting the latter, M. Chomel says that, in the syphilitic and rheumatic, several articulations may be simultaneously diseased; that, although syphilitic swellings are generally developed in the bodies, and not in the extremities of the long bones, such is not universally the case. Sometimes syphilis attacks the ends of the long bones: this is a rare but an actual occurrence. Hence the importance of diagnosis in such a case. No doubt, the articulation may, as in rheumatism, be painful, swollen, red, and heated; but commonly these symptoms are not present in the entire joint, but are limited to a part of it. Thus, for instance, they attack the acromion, one condyle of the elbow, a single styloid apophysis of the wrist, one condyle of the femur, one malleolus, &c.: but, if the syphilitic affection of the articular extremity gives rise to effusion, then there will be swelling of the entire articulation; but still, in this case, the pain is felt in a particular spot. Again, with the same degree of pain, the rheumatic patient cannot move the diseased limb: whilst, on the other hand, the syphilitic preserves all his power of motion, the pain being little if at all increased by motion. The attendant circumstances, of course, must always be borne in mind. In cases where the constitution has been saturated with mercury, M. Chomel especially recommends "*la tisane de Feltz*," which is indubitably an excellent antiphlogistic for individuals saturated with mercury. It is supposed that the active principles of this ptisan is arsenic in very small quantity, combined with sulphuret of antimony.

*Pathological Anatomy.* If the articulation has not become deformed, there is generally no appreciable lesion. Sometimes, however, curious lesions are found to exist, such as the following:—The synovial membrane was detached, and covered with small holes, the diameter of which varied from a line to a line and a half. In the points corresponding to these holes, the compact tissue of the bone was entirely destroyed; the spongy tissue alone remaining and this was of a reddish colour, but not however softened, as in caries; the medullary cavity of the bone contained a bloody serum. Sometimes, instead of articular cartilages, a cellulo-vascular tissue is found; instead of the normal layer which covers the extremities of the bones, there exist fleshy granulations, which are detached from the bony substance. Occasionally there are no traces at all of cartilaginous tissue. A superficial ulceration, or destruction, of the articular cartilage is also found, without the development of cellular vascular vegetations; the bone is almost or quite naked: this denudation does not generally exist over the whole extent of the articular extremity, but is constituted by small irregular perforations, from a line to a line and a half in diameter. M. Chomel regards this lesion as consecutive to that previously mentioned. In addition to the lesions of the synovial membranes, cartilages, and extremities of the bone, seen in M. Chomel's cases, there was an infiltration of blood into the fibro-cellular tissues external to the synovial membrane, the internal parietes of the articular cavity were of a blackish colour, depending on this hyperæmia; and, at the knee and hip, the interarticular ligaments presented the same appearance. These lesions appear to be of an inflammatory nature, and seem also to belong to chronic rheumatism, but, since they are not constantly connected with this affection, they cannot be regarded as its anatomical cause: they are probably but an effect of it. Blood has been found effused within the articulation, as well as externally to it. Suppuration has never been regarded as an effect of chronic articular rheumatism. The tophaceous concretions are seen only in a comparatively few cases, and appear to require, as a condition of their formation, some special idiosyncrasy, independent of the arthritic or gouty diathesis. These concretions are found in the most various parts of the articulations, from their interior to the layers of the skin; and sometimes, immediately beneath the epidermis, where they appear on the point of escaping. Is it hence to be inferred that this morbid secretion takes place indifferently in all the regions of the joint? Or do they not (formed originally in the fibrous tissues,) irritate and give rise to ulceration of the adjacent tissues; and hence pass, in one direction, into the articulation, in another, towards the skin? It is certain that when, either by incision or ulceration, the tophaceous matter is evacuated, the articulation is not thus necessarily opened and exposed to the external air. Hence it appears that it is not formed within the joint; but that, when it is found there, it has penetrated in the same manner, as it makes its passage through the skin: and this may perhaps be regarded as additional evidence in favour of this disease having its primitive and idiopathic seat in the fibrous tissues. From the various analyses of the secreted matter, it appears to consist chiefly of uric acid, either free or in combination with some base, such as soda or lime.

There is nothing which need detain us on the treatment of chronic articular rheumatism.

ORDER III. VISCERAL RHEUMATISM. This section is perhaps the most unsatisfactory in the volume, requiring, as it does, a considerable exercise of faith on the part of the reader. All the instances, recorded by authors, of gout being transported to the brain, &c., are not considered as belonging to this class of rheumatic affections. M. Chomel only recognizes internal rheumatism where there exists a muscular or fibrous tissue, such as is found in, 1, the diaphragm; 2, the heart; 3, the air-tubes, which present, in the larynx, a complete system of articulations; 4, the alimentary canal, (pharynx, œsophagus, stomach, and intestines); 5, the bladder; 6, the uterus: and, before further considering the rheumatic affections of these various parts, he says, "that in no particular case can the disease be admitted or diagnosticated except on presumptive evidence, not by an absolute demonstration;" although he considers that their existence may be contested "*en thèse générale.*"

1. *Rheumatism of the Diaphragm.* This may be suspected in individuals subject to or actually affected by regular rheumatism, if there be a sensation of painful constriction at the base of the thorax with dyspnœa. The physician called to such a case will be especially struck with the increase of pain at the attachments of the diaphragm, on inspiration, on the passage of food or drink, or on eructation of wind. With these symptoms may be connected cough and hiccough. This last symptom is regarded as important and characteristic. Sauvages speaks of *Singultus arthriticus* as a symptom of metastasis of gouty matter to the diaphragm.

The treatment recommended for this, as well as for other visceral rheumatism is, to recall the rheumatism into parts the functions of which can be disturbed with less inconvenience; such as the articulations, and particularly those previously affected. In cases of danger, the revulsions must be of a powerful and quickly operating kind; such as boiling water and ammoniacal ointment; continuing at the same time more tardy counter-irritation, by mustard, &c.

2. *Rheumatism of the Heart.* M. Chomel thinks that this may be either acute or chronic. Many diseases of the heart, regarded as incurable organic affections, but which have perfectly recovered, it may be suspected, were rheumatic. The muscularity of the heart would lead us to suspect that it might become rheumatic. The above remark on the cures which have taken place of affections of the heart, regarded as organic and incurable, is, we believe, especially appropriate to the young; and it is one which deserves more consideration than our author has given to it. It must have occurred to every one to meet with children, either actually rheumatic or born of rheumatic parents, with signs and symptoms of affection of the heart, such as have led those generally considered good judges of diseases of that organ, to condemn them as incurable, which children have however subsequently lost every symptom of cardiac affection: and these cases are not such as admit of explanation by supposing the occurrence of pericarditis, or indeed of any action leading to organic change. Syncope of short duration, occurring during the course of acute articular rheumatism, a palpitation, more or less considerable, happening under similar circumstances, and disappearing at the end of two or three hours, leaving no traces behind it; in short, some anomalous symptoms referrible to the heart, gone almost as soon as they appear, indicate very probably acute cardiac rheumatism. But its more common form is the chronic. Some gouty or rheumatic people are subject to præcordial pains, not only whilst walking quickly or going up stairs, but suddenly and during the night. There may be commencing anasarca, in consequence of disturbed circulation, and all these formidable symptoms may cease as by enchantment, on the occasion of a regular attack of rheumatism or gout.

These are the symptoms which M. Chomel ascribes to the two forms of cardiac rheumatism. Its differential diagnosis requires great care; and, as examination after death cannot confirm it, it is the more essential to pay great attention to the symptoms existing during life. From *Pleurodynia* it may be distinguished, because, in this disease, the pain is increased by pressure and by inspiration. *Pericarditis* has its dull sound on percussion, excepting at first; and there is fever. *Organic diseases of the heart* have their dependent signs. *Nervous palpitations* occur from a thousand physical or moral causes, in nervous individuals. There has here been no previous rheumatism; and most commonly not the slightest increase of sensibility in the region of the heart precedes the sudden invasion of palpitations. *Angina pectoris* is a disease very analogous to this rheumatism, and it is supposed that it may sometimes be rheumatism. If the above symptoms, evidently attached to the heart, cannot be ascribed to any of the previous diseases, and they occur in a rheumatic subject or in one whose parents were rheumatic, there is some probability of the existence of cardiac rheumatism. The same treatment is required as in the former case.

Under the head of *Rheumatism of the Air-Tubes*, M. Chomel merely alludes to instances of aphonia occurring in the course of rheumatism, which may be of a rheumatic character; the disease existing in the larynx.

*Rheumatism of the Alimentary Canal.* The ancients have spoken of this disease in the stomach and intestines. It has of late been more disregarded, since it has been more the practice to seek for anatomical characteristics of disease. At the beginning of this century, Barthez published some excellent observations on acute and chronic gout of the stomach and intestines; and Rodamel has published facts tending to establish the reality of rheumatic pains in the stomach. M. Chomel believes that gastro-intestinal neuralgia is somewhat rare; and that the affections generally termed gastralgia and enteralgia, in opposition to gastritis and enteritis, should rather be regarded as metastasis of rheumatism, or of affections of the skin (*dartres*); that, in the former case it is the muscular coat, in the latter the mucous, which is the seat of disease. He contends that the structure of the stomach and intestines afford *à priori* evidence of this doctrine, and that it is also confirmed by facts. The *chronic form* is that most frequently observed. If the stomach is rheumatic, the epigastric sensibility is increased, and indeed much more than it is commonly in inflammation of its mucous membrane; and this pain, which affords a singular contrast with the absence or feeble degree of febrile movement, is especially increased by pressure. Besides this, there is nausea and even vomiting; but the vomited matters are not such as are proper to inflammation. A similar pain in the part affected marks rheumatism of the intestines, and there are irregular colic pains. In each case the pain may vary a hundred times in intensity and situation, without, on the whole, being influenced by food. This is contrary to what happens in inflammation: in rheumatism, there is nothing uniform in the effects produced by ingestion of food, nor in the character of the alvine evacuations. Sometimes the pain is increased, at others diminished, whilst sometimes it remains wholly unchanged. It is not uncommon to observe this in many who are said to be affected with chronic gastritis or enteritis. Sometimes such people are better after eating more than usual, or after taking exciting food. This want of influence of food proves that there exists some other affection than inflammation. It is particularly after certain atmospheric conditions that the pains become more acute, and especially when the air is cold and moist, or is stormy and charged with electricity. Patients complain that they have been bled in vain, condemned to abstinence, obliged to submit to a vigorous antiphlogistic regimen, without becoming either better or worse. They have found temporary effects only from calmatives. Previously, most of them have had either articular or muscular rheumatism, or may have it still, or are born of rheumatic or gouty parents. By the aid of these considerations, gastric and intestinal rheumatism may be recognized; confounded, as they have been, by M. Broussais with inflammations, with neuralgia by M. Pinel. Apyretic and frequently transient pains, or pains which last an indefinite period without general *malaise*, are the best means by which the physician may recognize, whether in the acute or chronic forms, a rheumatic affection of the bowels. The prognosis is unfavorable as it regards the duration of the disease and the probability of its relapse. Anatomical evidence of it is sought in vain. The treatment is the same as that already mentioned; but with greater attention to food, which should not be of an exciting character.

We have given M. Chomel's remarks on this subject fully; but, as in the former chapters on visceral rheumatism, they are not quite satisfactory; although they may explain many cases, now termed gastralgia and enteralgia with much probability; and indeed, in this country, symptoms of such a character are ascribed to intestinal rheumatism. But it is probable that M. Chomel may have included cases of a different character, under the same arrangement. The absence of pain in many instances on taking food would make us hesitate whether such cases should be regarded as rheumatic; for the slightest action of a rheumatic muscle is attended with great increase of pain. Distension and certain spontaneous movements of the stomach and intestines are the consequence of taking in food. But we are here required to believe that their muscular tunics may be rheumatic, and yet that they may move without pain. In the absence of anatomical evidence to the contrary, and seeing beside no single objection to such a belief, we therefore think that many of these affections, i. e. "apyretic, and, frequently, transient pains, or pains which last an indefinite period without general malaise," are really neuralgic, whilst others are of a rheumatic character. M. Chomel is too much disposed to be exclusive in his arrangements, and we regard this as an example.

*Rheumatism of the Bladder.* The acute form of this succeeds another rheumatism suddenly interrupted, or it occurs in the course of acute articular rheumatism. Sometimes, there is frequent micturition, and the excretion is very painful; sometimes there is retention. It would thus seem that the disease was at one time in the neck, at another in the fundus of the bladder. Occasionally, in the same individual, there are observed, in the same attack, alterations of dysuria and ischuria. It may also, says M. Chomel, exist in the chronic form. The diagnosis in these cases is to us quite unsatisfactory.

There is nothing that need delay us in the remarks on *Rheumatism of the Uterus.* Rheumatism is believed by M. Chomel to exist *in the Peritosteum,* in those cases where there are fixed pains on a superficial bone, without any trace of swelling, and which appear consecutively to, or simultaneously with, affections which are incontestibly rheumatic. So likewise, some pains in *the teeth* are regarded as *rheumatic;* an instance of which is given in which shifting pains from the upper to the lower jaw succeeded to an attack of lumbago and articular pains. It is conjectured that the *dura mater* may be affected with rheumatism. M. Chomel restricts the application of the term *arthritic inflammation of the sclerotic* more so than is generally done. He recognizes it, when the sclerotic becomes the seat of pain, and the redness of the conjunctiva is consecutive without catarrhal secretion, and when these things take place in an individual previously affected with rheumatism, with which this affection has alternated.

We have already stated that M. Chomel regards gout and articular rheumatism as the same disease, and he now examines the grounds on which the distinction, admitted by others, rests. We will here abridge his opinions. Is the distinction made, he remarks, because gout affects the small joints, and particularly those of the great toe, to the exclusion of other joints? It is evident that the symptoms of articular rheumatism should not be exactly the same at the hip and at the phalanges. In rheumatism of a large joint, there is little or no evident swelling; in that of one of intermediate size, the redness and swelling are more marked; still more evident are they when a small joint is affected. But these, instead of constituting essential differences, are but dependent on the greater or less quantity of tissues interposed between the joint and the skin. Besides, in many *gouty* individuals, both large and small joints are simultaneously attacked: and if, as is indicated by the use of the generic name, a striking analogy is allowed to exist between muscular and articular rheumatism, there is greater reason to admit a relationship between what is termed gout and articular rheumatism; for, between the two last, the resemblance is much more striking than between articular and muscular rheumatism. M. Chomel has found that the digestive organs are not particularly troubled in what is termed gout, more than in ordinary and simple inflammation of the great joints: he has rather noticed the reverse. Rheumatism is hereditary, so that this quality cannot be regarded as peculiarly belonging to gout; nor can its rapid change of seat or the periodicity of its attacks. All the forms under which gout is described apply exactly to articular rheumatism. The *regular gout* is in no way different from it. The *atonic gout* is simply a very natural complication of muscular rheumatism with the gastro-intestinal rheumatism already described; indeed this should rather be termed muscular rheumatism than gout; for in this case neither large nor small articulations are affected: and, in *metastatic gout*, when there is the sudden explosion of inflammation in some viscus, consequent on the delitescence of the articular affection, what is this more than the metastasis which so frequently happens in rheumatism, and which is no less frequent when the large than when the small joints are attacked? There appears, then, to be nothing, as far as it concerns the *seat* of the disease, which separates gout or inflammation of the small articulations, as essentially different from rheumatic inflammation of the large joints. It is said that the poor and labouring classes are spared, and that the rich suffer from the gout. A poor man does not come to an hospital on account of a rheumatic finger or toe; he does not seek medical aid until he is forced to do so, by having lost, more or less, the use of his limbs. But it would be a mistake to suppose that amongst the poor the disease never commences in the small joints. Of this M. Chomel has seen two instances, and some have likewise occurred to M. Requin, the editor of M. Chomel's work, in his dispensary practice. The tophaceous concretions, too, cannot be regarded as signs of gout as a disease distinct from rheumatism; for, if a person is simultaneously affected with the disease in the small and large joints, it is only in the former that these concretions take place. This must be attributed only to a difference in the situation, and not in the nature of the disease: otherwise, how can it be explained that the gout (so called) where it passes from the toes to the knee, so rarely occasions the formation of concretions in the latter? It must therefore be admitted, concludes M. Chomel, that there is no legitimate and fundamental distinction between gout and common articular rheumatism: gout, consequently, in good nosology, should be regarded as identical with rheumatism, or, at the most, as a simple variety of it.

We have given M. Chomel's reasons without thinking that they at all decide the question. In the present state of our knowledge of the diseases termed gout and rheumatism, we are probably not justified in absolutely maintaining their identity or non-identity. As good a case might be made out in favour of the identity of some of the exanthemata, as is here attempted for gout and rheumatism. It is unfortunate that in this instance, as well as in other diseases, we can consider but a few of their phenomena; others, as the state of the fluids, &c., escaping our notice; and that our decision of the question is to be made on this partial knowledge. Hence, the arguments drawn by M. Chomel from what he terms identity in the *seat* of the disease, are uncertain and inconclusive. It is from this circumstance, probably, that M. Chomel's observations, although pointing out more marks of similarity than have hitherto been noticed between gout and rheumatism, are to us quite unsatisfactory. We are still disposed to regard these diseases as distinct, although in many parts, both local and constitutional, strikingly allied. Some of the arguments adduced by M. Chomel will not be admitted as founded on facts; and there are others to which he has not alluded, to which, in approaching to a decision of the question at issue, some weight may probably be attached. Let the reader bear in mind well marked cases of regular gout (as it is termed) and acute articular rheumatism; and we think that he will have no hesitation in recognizing in the former, as a general rule, a state of absolute or relative plethora which is not found in the latter; and, although we are ready to admit, with M. Chomel, that actual dyspepsia is fully more prevalent in rheumatic than in gouty individuals, still we cannot doubt that temporary disorder of a digestion generally healthy, is vastly more frequent as a precursor of gout than of rheumatism. Few experienced practitioners in this country will admit that the poor are as obnoxious to gout as the rich; or, rather, the penurious livers, as the luxurious: and we all know that the facilities of obtaining gratuitous medical assistance in this country would never prevent a poor man's applying for it, if he were suffering from what is termed an attack of gout; a disease quite as disabling for a time as any attack of acute articular rheumatism. There is likewise a distinction in the degree and kind of pain; "sometimes resembling the tension or laceration of the ligaments, sometimes the gnawing of a dog," as Sydenham describes it, from his personal experience. The causes are likewise regarded as in some measure distinct; those of gout being internal, whilst rheumatism more often arises from exposure to cold and wet. The ages at which they occur, notwithstanding the calculations of M. Chomel, (and these calculations, it must be remembered, are made assuming the identity of the diseases,) we still regard as in some degree marking the peculiarity of the diseases. The effect of treatment is likewise somewhat different: colchicum in gout being almost specific; not so in rheumatism. But, notwithstanding all that can be said in favour of the distinctness of the two diseases, they appear very nearly to resemble one another, and to run together in those cases which are termed *rheumatic gout*: and these are the cases which afford the strongest support to M. Chomel's opinions. The general sense of the profession is in favour of the relationship of gout and rheumatism, and it is more accordant with nature to suppose that one runs into the other by insensible gradations, than to believe that there exists any well-defined boundary line between them. Man makes these divisions for his own convenience; they are not the work of nature. Between the animal and vegetable there are intermediate structures, which cannot be referred exclusively to one or the other kingdom; and between the natural families of plants and animals there is the same gradual transition. So it is with diseases; and, in this instance, is not rheumatic gout the union of two diseases which must be regarded as distinct? Can it be the case that, from their different mode of life, or from some other cause, gout is a rare disease among the French, and that consequently M. Chomel is not very conversant with it? He has identified the rheumatism and gout, and considered both under one name; but, certainly, whilst as a treatise on what we term rheumatism, we should highly recommend his book, we should regard it as trustworthyless as a treatise on gout; feeling assured that any practical and experienced physician, who has been in the habit of attending the wealthier classes in England and witnessing their gout, can have no difficulty in drawing the distinction between such cases and the rheumatism of the lower classes.*

M. Chomel terminates his work by some remarks on the nature of rheumatism, regarding it as a disease *sui generis*, which requires to be classed by itself in a nosological arrangement. We will give his reasons shortly. In many cases, rheumatism appears with all the phenomena of inflammation; but it has been seen that this is not always the case. Pain is sometimes the only symptom of rheumatism: to this may be added heat, or, on the contrary, a sensation of cold. The pain, also, is frequently not increased by pressure, but is relieved by rubbing. In muscular rheumatism, no change of structure can be detected after death; and that form of the disease which is most inflammatory is distinguished from simple phlegmasia. The inflammation is not limited to one point, but scattered over several; and, as often as we find on the surface of or within the body many spots *(foyers)* of inflammation,—as, for example, in variola, &c.—our reason is obliged to admit a unique and common cause of all these disseminated inflammations; and it is this cause (call it virus, miasm, or what you will,) which constitutes the essence of the disease, and which should never be lost sight of for the sake of partial inflammations, which are indeed its most frequent manifestation, but which may in fact be absent; the disease remaining essentially the same. So variola may exist without pustulation; typhus fever without inflammation of Peyer's glands: and thus the cause of rheumatism does not always affect the joints, but may disturb, immediately, idiopathically, and independently, certain viscera. The disappearance of rheumatism from a joint, and its sudden appearance elsewhere, is quite unlike a simple phlegmasia. Its want of fixed periods, of determinate duration, distinguish it also from simple inflammation; and, during the course of rheumatic fever, the joints may be freed from disease whilst the fever continues. Hence it follows that inflammation does not constitute the whole of the disease. There is no recorded example of the disease ending in gangrene, and its termination in suppuration is not an established fact. Hence it follows that rheumatism is not to be arranged among phlegmasiæ, properly so called; and that, when it does present itself in an inflammatory form, the inflammation is not idiopathic but symptomatic, and that it has a specific nature. M. Bouillaud's opinion is expressed only on acute articular rheumatism; in favour of the inflammatory character of which he advances the usual arguments. With respect to the multifarious forms of disease included by M. Chomel under the term rheumatism, many doubtful questions might be raised; and the first is, whether there has been more than presumptive evidence to show that what is called muscular is the same disease as articular rheumatism? We have not room at present to moot the question, nor to apply all the arguments which have been over and over again used by the opponents and advocates of idiopathic fevers, and diseases caused by a morbid poison, to this subject of rheumatism. We want more knowledge before such a discussion could be brought to anything like a satisfactory conclusion. At the same time, common sense has long ago convinced us of the inflammatory nature of acute rheumatism, although we qualify it by terming it a specific inflammation; and hence it is not to be judged of according to the rules of common inflammation: the disease must be studied by itself.

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* We would once more refer to Dr. Barlow's article on *Gout*, in the Cyclopædia, for the best diagnosis we know of gout and rheumatism. Vol. ii. p. 364.

For such a study we can recommend both the works before us. M. Chomel's will give many a reader a clearer notion of rheumatism than he previously possessed, although he may think that the term has been allowed too extensive an application. Like all the productions of eminent men of the French school, it is remarkably methodical and free from all obscurity: its fault, which equally belongs to the same school, is too great diffuseness, too much explanation, too little belief in the reader's own powers of reflection and judgment. The fondness for their own exclusive views, also, is an objection to both the works before us: but to the readers of the present article, this, together with the other defects, is of less importance, as the analysis presents most of the valuable matter, and all which can be regarded as novel, contained in the two books; together with (it is hoped) an impartial criticism of those subjects concerning which the authors' opinions are particularly at variance, or where their importance, or novelty, or any other quality, appeared to render them worthy of notice.

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## Art. VII.

*The Transactions of the Provincial Medical and Surgical Association.* Vol. VI.—*London and Worcester,* 1838. 8vo. pp. 621.

We congratulate the members of the Provincial Association on the appearance and upon the quality of their sixth volume. We found little in its predecessor which was of interest; and, if we were consequently disposed to anticipate but little from the present volume, such anticipations have certainly been disappointed.

A Report of the proceedings of the meeting which was held at Cheltenham, July 19th, 1837, is followed by the annual Retrospective Address, delivered by Dr. Bardsley, of Manchester. A Review, one object of which is to supply the most recent intelligence of all the novelties in medicine and the collateral sciences, partakes so much of the character of a retrospective address, that it would be superfluous for us to give any detailed notice of Dr. Bardsley's very excellent performance; in which he has considered the various additions to our knowledge which have been made during somewhat more than the previous year,—has referred to the medical literature, both domestic and foreign, of that period,—has paid a merited tribute of respect to several members of our profession separated from us by death,—and has added some judicious remarks upon medical education, appropriate to our present circumstances. There are one or two facts, resulting from Dr. Bardsley's own practice, to which we may allude. In speaking of a recommendation of Dr. Prichard's, of a method of counter-irritation in certain forms of cerebral disease,—the method consisting in making an incision through the scalp down to the pericranium, in the direction of the sagittal suture, from the summit of the forehead to the occiput, the incision thus made being kept open by one, two, or three rows of peas,—Dr. Bardsley says, "I am happy in being able to offer my own testimony in favour of the practical utility of Dr. Prichard's suggestion; having myself, in two instances of severe cerebral affection, indicated by mental imbecility, stupor, and imperfect vision, adopted the plan, after the failure of the ordinary modes of treatment, and with decided advantage." Dr. Bardsley also describes, as having fallen under his own notice, "a monstrosity, apparently the effect of an abortive effort of nature to effect the formation of twins. This *lusus naturæ*, having only one head, is possessed of four arms and four legs; the sex is masculine, and the organs of generation are double, with several other analogous peculiarities," which are represented in a drawing. The child was born in the latter end of May, 1837, and was alive in July of the present year.

The second article consists of "*Introductory Observations to a proposed Plan for the Reports of Infirmaries and Dispensaries.*" A committee has been appointed by the Association for a consideration of this very important subject, of which Dr. Cowan, the author of the present observations, is the secretary. The object of Dr. Cowan is to recommend some uniform method of reporting cases; such as shall facilitate their arrangement when collected. The important statistical facts which may be obtained in this country by some systematic arrangement of the materials afforded in its hospitals may be inferred, when it is considered that "90,000 cases are annually admitted into the British hospitals, 6,300 of which terminate fatally; and that the dispensary and out-patients amount to at least four times that number." The great utility of adopting some uniform mode of reporting these and other cases, is well shown by the difficulties attending on the arrangement of cases which have not been collected in a systematic manner. Thus, says Dr. Cowan, "M. Louis, when inspecting his materials for his work on the 'Affection Typhoïde,' was compelled to devote three whole months to rewriting his own reports, for the purpose of arranging them on some uniform plan, by which their comparison became possible. Now it is evident that his labour might have been avoided, or materially lessened, had a correct method of reporting been originally laid down." And it is likewise said that "M. Chaponiere, in his inaugural thesis at Paris, 1832, on 'Facial Neuralgia,' spent several months in collecting all the observations he could find in the extensive library of the school, and from other sources; but, of 653 recorded cases, he was obliged to reject 400, in consequence of the irregular and incomplete manner in which they were reported."

Dr. Cowan has suggested a tabular arrangement, which is intended to direct the attention of members more distinctly to the subject. It is not put forward as perfect; as doubtless some members may be able to suggest particulars in which it may be improved: and as it is hoped that whatever arrangement is decided on will be pretty generally adopted, a scheme as perfect as possible is to be desired.

The next paper is "*On the Medical Topography of Exeter and its Neighbourhood; being a Sketch of the Geology, Climate, Natural Productions, and Statistics of that District.* By THOMAS SHAPTER, M.D." We cannot speak too highly of this very elaborate performance: it seems to leave no attainable point unreached. It is in vain to attempt a detailed analysis of Dr. Shapter's memoir, but we cannot deny ourselves the gratification of a brief outline of it.

The portion of Devonshire to which the paper relates is Exeter and its immediate neighbourhood. The river Exe flows through the city, and, between this river and its opening into the sea, various towns, it is well known, are situated. The mean height of the land boundaries to the south-west of the district is 1,782 feet; to the north, 740 feet; and there are also high lands to the east. The general physiognomy is such as is seen in countries with undulating high grounds and luxuriant and highly cultivated valleys. Exeter occupies the flat summit and declivities of a hill which rises from the eastern bank of the river. The summit of the hill is 149 feet above the level of the sea. The hilly character of the situation favours cleanliness and ventilation. The western part of the city is occupied by the great mass of the poor: much of this portion is situated on the steep declivity, running westerly downwards, and thus affording sufficient drainage and ventilation; the remainder of this quarter is a hollow flat. The south-eastern quarter is occupied by the resident gentry, and is open, pleasant, and healthful. The northern and southern quarters are occupied by persons in moderate circumstances. The general character of the surface and soils in the district varies considerably; ranging from the granitic, through the cold slaty soils, to the fine, rich loam lands on the sandstone. The city, by means of its river, wells, conduits, springs, &c., is well supplied with water. Dr. Shapter has mentioned the leading characteristics of all the waters in the district: they consist of common hard spring-water; some of them, however, containing a sufficient quantity of mineral impregnation to entitle them to be called mineral springs; and of acidulous chalybeate, which also, in some instances, might, from their peculiar impregnations, be termed mineral waters. Notwithstanding the general character which Exeter and its neighbourhood have obtained for mildness and equableness of temperature, there is very little certain information recorded respecting these points. The averages from which the following estimates are made "are, in almost every instance, deduced from observations made during a period of ten years,—i. e. from 1825 to 1834 inclusive,—and are contrasted with those deduced from registers kept in the metropolis;" a system of comparison which has been already adopted in the Society's Transactions.

The *annual mean temperature* of Exeter is 51°29; of London, 50°39: and in this calculation Dr. Shapter is of opinion that the temperature of London is too high, since the London average is derived from registers kept in the heart of the metropolis, where the climate is influenced by the circumstances of a great city. Some tables given by Dr. Shapter tend somewhat to support a statement made by Luke Howard, that there is an ascending and descending series of temperature, rising and falling by alternate years, rendering it probable that we have in this island a cycle of temperature, in which the climate becomes warmer and colder by turns, in such a way as to exhibit both extremes in the space of seventeen years.

The *mean temperature of the seasons* is as follows:

| | Winter. | Spring. | Summer. | Autumn. |
|---|---|---|---|---|
| Exeter........ | 41.80 | 49.51 | 62.08 | 51.94 |
| London ...... | 39.1 | 48.7 | 62.3 | 51.3 |

The *absolute range* is

| | Greatest. | Lowest. | Mean. |
|---|---|---|---|
| Exeter........ | 94 | 16 | 70 |
| London....... | 96 | 5 | 91 |

From the recorded tables it may be seen that the climate of Exeter and its neighbourhood, though not so equable as that of Penzance, is justly worthy of its character as to mildness, for which it is so justly esteemed. As contrasted with London, the *equability* of the climate is very marked; since, although the mean temperature of Exeter is greater than that of London, yet the summer of the latter city has a higher temperature, whilst its winter is considerably lower. By comparing his district with other places within the same zone of temperature, and by drawing an isotherial line, and regarding how much the mean temperature of the places it cuts is in excess of that of Exeter, Dr. Shapter has further shown the comparative equability of the temperature of his district.

The quantity of *rain* which falls on the Devonshire coast is almost proverbial; but Exeter itself seems to be unjustly suspected of being very rainy. The mean annual fall, in inches, is as follows:—Exeter, 29.12; St. Thomas, 31.90, (this is a parish in the vicinity of Exeter;) London, 25.00. But the average number of wet days (by which is meant a day on which a fall takes place, however slight it may be,) is in favour of Exeter: e. g. St. Thomas', 162.4; London, 178. The least rainy season is the spring; the autumn is the most so in regard to quantity; but in the winter is the greatest number of rainy days.

Continued *frost* is very rare; although Dr. Shapter states that, about once in five years, "it continues sufficiently long to freeze the river to a state thick enough to sustain large masses of people." In ten years, there were sixty-nine days during which *snow* and *sleet* fell; and *hail* is scarcely more frequent than snow. As compared with London, the relative proportion of *winds* for ten years is as follows:

| | N.W., W. | S.W., S. | S.E., E. | N.E., E. |
|---|---|---|---|---|
| Exeter, 1825-34.. | 131.3 | 86.2 | 71.1 | 75-6 |
| London, 1807-16.. | 100.4 | 104.4 | 53.9 | 74.4. |

The barometer is highest during the N.E. and N. winds, and at the lowest during the S.W. and S. winds. The thermometer is at its maximum average during the S. and S.E. winds, and at its minimum during the N.E.: but this last wind, which has the least elevated temperature during the winter months, becomes the warmest in June. During the winter season, the s.w. wind is frequently accompanied by a warm, thick mist, which has a peculiar relaxing softness in its feel, and is not unaptly styled the true Devonshire weather. The list of exotics capable of cultivation in the open air is further confirmatory of the mildness of the Exeter winters.

Dr. Shapter next gives an account of the supply of the markets; of the natural productions of the district; of its civil and economical history, &c. The agricultural population forms only 1-94th part of the whole, and rather more than 1-6th are engaged in trade and handicraft. There appears to be nothing peculiar in the *occupation* of the inhabitants worth dwelling on. Dr. Shapter thus describes the cider which the Devonshire labourer calls good: "It is full in body, rough in taste, and hard, and has no flavour of the apple remaining in it. The sweetness of the cider prepared for exportation is preserved by a process to which it is submitted during its manufacture, termed 'matching.'" Since lead has been almost entirely discontinued from among the utensils used in the cider manufacture, Dr. Shapter states that the *colica pictonum* is no more frequent in Devonshire than in other countries.

The second part of Dr. Shapter's paper is *Statistical and Medical;* but it is from "the concluding portion of this paper, embracing the general law of mortality, together with the medical history and medical statistics of the city," and which "will follow in a subsequent volume," that we shall expect to derive facts of greater interest to our readers. The statistics here introduced are such as are only illustrative of the effect of climate. Tables which are 'the result of very careful and elaborate examinations of the different parish registers" are subjoined, to exhibit the data whence the average mortality is deduced. As this subject will be further treated of, with the Law of Mortality, we pass it by at present, as well as another series of tables, which "place in apposition the climate, with the mortality of those ten years which throughout this paper have engaged our attention."

We can safely recommend to his fellow associates Dr. Shapter's paper, as a model to be followed in prosecuting the great object of the Association, the formation of a complete Medical Topography of England.

The fourth article is by Mr. Nash, "*On the Medical Topography and Statistics of Cheltenham.*" This paper is not, as the former, the result of long and careful observation; yet it is very creditable to the author. The medical department of its statistics is, however, of very little value. There is more which is interesting in the geological and botanical history of the place, as also in the account of its mineral waters; but our limits prevent us from entering on these subjects. We notice one or two particulars of the climate, for the sake of comparison with what we have recorded respecting Exeter. The annual mean temperature of Cheltenham is 50°.26, calculated on an average of seven years. The mean temperature of the seasons is as follows:—Spring, 49°.65; summer, 60°.80; autumn, 50°.28; winter, 40°.92. The average number of days on which rain has fallen is 110; and the annual fall of rain (during four years) 33 inches. There seems little difference in the character of the winds from that of the more southern and western parts of the island. There are three maps associated with Mr. Nash's paper: a geological map of the country around Cheltenham; a section of the vale of Gloucester, from the Cotswold hills to the river Severn; and a map of the number of feet above the river Severn of Gloucester and the neighbouring country.

The Third Part of the volume, consisting of "*Essays and Cases,*" commences by a most accurate and interesting "*Cursory Examination of the Works of Galen, so far as they relate to Anatomy and Physiology,*" by Dr. Kidd, of Oxford. It is greatly to be wished that Dr. Kidd would take the trouble of making similar analyses of the works of other ancient medical authors: no one is so well qualified for the task. To many practitioners the works themselves are inaccessible; to many more neither time permits nor inclination leads to their perusal; but essays such as the present are at once accessible and acceptable to all. We would gladly transcribe the whole, but must limit our notice to a few extracts.

The system followed by Dr. Kidd is, "after a few introductory remarks, to examine, first, Galen's account of the organs and processes of nutrition, secretion, and reproduction; then of the instruments of voluntary motion; and, lastly, his account of the nervous system." Most of the descriptive terms, both in physiology and pathology, as well as in anatomy, which are now in use, were employed by Galen in the same sense as they are employed by modern authors. He appears to have been possessed of great anatomical dexterity. He regarded the stomach, intestines, liver, and veins as the organs of nutrition. The veins were said to convey the chyle from the stomach and intestines to the liver, by the vena portæ; the formation of which, and its several branches, are very well described. The venæ cavæ hepaticæ were regarded as the commencement or root of the venous system of the body at large. Galen seems to have known the dependence of the condition of ulcers of various parts upon the process of digestion: he observes that, if food be unwholesome, the resulting chyle will not be good, and that from unhealthy chylification, phagedenic and other ulcers will arise. He was not aware of the true nature of the lacteals; but he spoke of the convolutions of the intestines being numerous, in order that the surface for absorption might be extensive. He considered that the blood was perfected in the liver. Whilst he admitted, according to the prevalent theory, that the arteries contained air, he asserted at the same time that they naturally contain blood also; for he says that, if an artery be wounded, blood immediately flows from it, without any previous escape of air; which is a proof that blood was already in the artery: for, had it made its way into the artery from any other source, the air must have first escaped. With reference to the decision of the general question, whether blood is contained in the arteries, he relates the following anecdote:

> "There are some teachers," he says, "who are in the habit of advancing opinions which they are not prepared, and therefore not inclined, to put to the test. Such was the case with a certain teacher of anatomy, who, having declared that the aorta contains no blood, and having been earnestly desired by some of the ardent pupils of Galen to exhibit the requisite demonstration, they themselves offering animals for the experiment, declined, after various subterfuges, to satisfy them without a suitable remuneration: on which the pupils immediately raised a subscription among themselves for the purpose, to the amount of a thousand *drachmæ*, (equivalent, probably, to about twenty-five or thirty pounds of our money.) The professor, being thus compelled to commence the experiment, failed in the attempt to cut down upon the aorta, to the

no small amusement of the pupils; who, thereupon, taking up the experiment themselves, made an opening into the thorax, in the way in which they had been instructed by Galen; passed one ligature around the aorta at the part where it attaches itself to the spine, and another at its origin; and then, by opening the intervening portion of the artery, showed that blood was contained in it."

Galen knew that, notwithstanding the force with which blood is propelled from a wounded artery, gentle pressure of the finger is sufficient to stop its further escape; and that, during the effect of this pressure, the blood soon coagulates, and forms a natural plug within the artery. He regarded the whole arterial system as consisting of those pulmonary vessels which end in the left auricle, together with the aorta and all its ramifications; and he compares these pulmonary vessels, in their supposed office of attracting air from the lungs, to the roots of a tree, which attract nourishment from the surrounding soil; and the aorta and its ramifications, which are supposed to receive the air forwarded by the pulmonary vessels, to the trunk and branches of a tree receiving its nutriment from the roots. The veins are considered by Galen as the efficient nutrients of the body at large. He had a considerable acquaintance with the uses of the valves of the heart: he knew that the heart was the source of pulsation; for he says that, if a ligature be made on an artery, pulsation continues in that part of the artery which is intermediate to the *ligature* and the *heart;* but ceases in that part of the artery which is intermediate to the *ligature* and the *extremities.* But he cannot explain why nature, who does nothing without design, should have made different vessels, arteries, and veins, to contain the same fluid; and he has no knowledge of the circulation, although acquainted with the anastomosis of these two orders of vessels. He describes two kinds of respiration; one by the mouths of the arteries of the lungs, and one by the mouths of the arteries of the skin: in each case the surrounding air is drawn into the vessels during their diastole, for the purpose of cooling the blood; and, during their systole, the fuliginous particles derived from the blood and other fluids are forced out. He supposes that the cuticular respiration is sufficient for animals during hybernation. Galen regards the diaphragm as the chief agent in ordinary respiration, as distinguished from the forced respiration which precedes any violent muscular effort; and he regarded the heat of the blood as in part being produced by respiration. He speaks of a quality in the air which is inimical to life. He describes various effects produced by dividing nerves, and shows the effect of dividing the recurrent branch of his sixth pair of cerebral nerves (the pneumo-gastric of modern anatomy;) explaining why the division of this branch, though it be made on both sides, does not entirely destroy the voice: he also explains how it happens that, if the spinal marrow be divided *beneath* the lower termination of the neck, the diaphragm will still continue to act; because the origin of the phrenic nerve is *above* the lower termination of the neck, In answer to some contemporaneous physiologist, who asserted that urine is only the excrementitious portion of that blood which nourishes the kidneys, Galen gives a good specimen of just physiological reasoning; for, from the very large size of the renal vessels, compared with the size of the kidneys themselves, he argues that those vessels were not intended solely for the nourishment of the kidneys, being capable of affording nourishment to organs of a much larger size; and he therefore concludes that, from the blood contained in them, not only are the kidneys nourished, but the urine also is secreted. His opinion on the nature and source of the catamenial discharge accords generally with that of modern physiologists. The discharge proceeds, he says, from the inner surface of the uterus, through the same orifice in its neck by which the embryo passes. In its character he considers it as somewhat different from the blood; observing that, at the period of the catamenia, the inner surface of the uterus is bathed by a moisture of a sanguineous nature, which has been secreted from the blood. Galen remarks that sensation and voluntary motion essentially distinguish animals from plants; and that, on this ground, nerves constitute muscle an *animal* organ, which, with reference solely to its arteries and veins, &c., is a merely *physical* organ. He supposes, however, that nerves do not possess an inherent and independent power, but that whatever power they have is derived from the brain; from which, though they have no perceptible cavity within them, they are capable of conveying sensation and power of motion to the most distant parts of the body. He distinguishes between the powers of voluntary and involuntary motion; and observes, that those organs of sense which are obedient to voluntary motion, as the eyes and the tongue, require, consequently, two sets of nerves; and, with respect to the tongue, he points out those nerves which belong to it as an organ of taste, and those which render it capable of voluntary motion. His opinion was also that the functions of the nerves arising from the spinal marrow were dependent on the brain. In practice he saw the importance of a knowledge of the parts of the spinal marrow to which ramifications of nerves belonged. Thus, when called to a patient who had for some time lost the sense of feeling in some of his fingers, and had derived no benefit from local applications to the fingers themselves, Galen enquired whether he had ever met with any injury of the spine; and, having been informed that he had fallen on his back some time previously with considerable violence, Galen applied remedies to the part on which he had fallen, and thus restored sensibility to his fingers. He relates experiments which he made in order to prove that, by the division of the spinal marrow in various parts, both the power of motion and sensation are destroyed in those muscles which are connected with the divided parts.

But we must, however unwillingly, terminate our abstract of Dr. Kidd's very interesting paper. As an indulgence of literary curiosity, if no other benefit is to be derived from an acquaintance with them, we should much like to see a similar analysis of Galen's works, "On the Differences and the Causes of Diseases," and "On the Method of Cure," together with other of his productions more immediately relating to medicine.

The next article, "*On the Treatment of Hypertrophy of the Heart, and chronic or subacute Inflammation of the Pericardium, especially in reference to the beneficial use of small Doses of Mercury in those Affections*," by Mr. Salter, of Poole, is one of a purely practical character. It is a most valuable essay, characteristic throughout of a thoughtful and rational mode of pursuing practice, and of the benefits likely to result from the opportunities of communication afforded by the Provincial Association. The paper is so valuable that we scarcely like to abridge it; and it is, therefore, a matter of great satisfaction that, through the medium of the Association in whose published Transactions it appears, it will fall under the notice of a large number of the profession.

The title of Mr. Salter's paper might have been more comprehensive, since the treatment recommended is said to be "applicable to all varieties of chronic diseases of the heart, with the exception of simple dilatation of its cavities, called by Corvisart, 'passive aneurism of the heart.'"

The greatest number of cases treated by Mr. Salter have been instances of hopeless organic disease, where palliation only could reasonably be looked for. Respecting these, however, he says, "I trust that I shall be able to show that we possess the means of staying the progress of morbid action, to an extent that has not commonly been thought possible; and that those persons who have the misfortune to labour under incurable disease of the heart may, notwithstanding, in a considerable degree, be rendered capable of enjoying the pleasures of life."

In considering the pathology of cardiac diseases, it is necessary to bear in mind the congestive states of parts influenced by the impeded circulation, such, for instance, as the lungs and liver; and these require attention in treatment: and it is remarked "that the means which are found effectual in removing the primary complaint are often the best suited for such as are superinduced by it; and this applies more especially to the prudent exhibition of mercurials." The influence of mercury in checking acute inflammations of the heart is well known; but Mr. Salter remarks that "it has remained to be shown that a modification of the mercurial treatment is a powerful means of checking the progress of those slow organic changes, which, if they do not destroy life so rapidly, are nevertheless as certainly fatal as the more active diseases, if allowed to go on uncontrolled by efficient treatment." We believe, with Mr. Salter, that, notwithstanding it has been the practice with some individuals to administer mercury in the forms of diseased heart to which he alludes, it remained to be shown how very extensively applicable it was in this class of affections. Mr. Colles, in his work on the Use of Mercury, alludes to the practice in the following terms, which we are happy to quote as confirmatory of the practice recommended by Mr. Salter. At the same time we are aware that, when Mr. Salter's communication was written, Mr. Colles' book was not published. "In cases of acute inflammation of a joint, or of the dense membranes of the eye, we find" says Mr. Colles, "that the progress of the disease is arrested the moment that the salivary system becomes affected; and even in cases of other diseases, which cannot be considered as purely inflammatory or acute, the same remark will be found to hold good: thus, in cases of orthopnœa, depending on organic disease of the heart, with effusion into some of the thoracic cavities, and in which we commonly prescribe mercury in combination with squill and digitalis, the patient is not at first sensible of any improvement, but almost invariably, as soon as the gums become affected, he experiences relief; and, perhaps the very next morning after this occurrence, he tells us, with joy and gratitude, that he is considerably better, that he has passed a night of refreshing sleep, and that he has been able to do what he could not have done for weeks previously,—namely, rest in the recumbent posture, without any of that alarming and distressing sense of suffocation under which he had previously laboured, and which always supervened the moment he sunk into that position." (pp. 31, 32.) And, again, at page 48, it is remarked that, in "organic affection of the heart, attended with orthopnœa and effusion into the chest, the ptyalism, established on the sixth or seventh day, may be kept up without injury for ten or fifteen days longer, if the obstinacy of the symptoms should require it," &c.

In addition to the acute and chronic forms of *pericarditis* generally described, Mr. Salter infers, from his own observations, that there is a milder or subacute form, consisting in a condition of hyperæmia. This, he observes, is mostly unattended with effusion or sensible thickening of the membranes; is commonly, though not always, a secondary affection accompanying chronic diseases of the heart. The condition above mentioned is regarded as receiving its best illustration from phenomena observed in an organ possessing an analogous structure, the eye. In this, especially in rheumatic subjects, a condition of hyperæmia often exists, which cannot be said to amount to inflammation: and this state may exist for a long time, and, when removed by proper management, will recur from the patient taking cold, or from a disordered condition of the digestive organs. Even this minor degree of congestion existing in the heart will, as is plainly demonstrated in the eye, if uncontrolled, lead to the most serious consequences, and may at any time quickly pass into a morbid action of the vessels of a more active kind. The inflamed state of the pericardium here referred to gives rise to the tenderness between the ribs in the cardiac region and beneath the ensiform cartilage, observed in chronic cardiac affections.

Although we are disposed to agree with Mr. Salter in thinking that there may exist some condition similar to that above mentioned, it is one which is without sufficient evidence. Mr. Salter speaks of this condition as being "mostly unattended with effusion or sensible thickening of the membrane." But these are negative signs; and it is not pretended that any congested state of the pericardium has actually been witnessed. Indeed, we are disposed to think that, in some of the cases in which the mercurial treatment has been so beneficial, it may have been by benefiting a condition of endocarditis, as well as of pericarditis or congestion of the pericardium: and of this endocarditis or (if an inflammatory nature does not belong to the condition to which we allude,) anormal state of the capillaries of some parts of the membrane lining the heart, we have probably evidence in the various changes existing in the valves. But this is a question more of curiosity than of any practical importance.

The treatment employed by Mr. Salter is as follows:—If there appears to be an absolute or relative plethora, some venesection precedes the use of mercury. The blue pill is then employed; the object of its use "being to make the gums slightly tender, as soon as it can be done, and with the smallest possible quantity of the mineral, and to keep up very moderate mercurial action for some little time after the symptoms have been relieved." Of the effect of this treatment Mr. Salter thus speaks:

> "I have seen patients affected with orthopnœa, unable, but for a short period, to bear the recumbent posture, and scarcely able to walk across their chambers, or to stoop to tie their shoes, labouring also under anasarca of the feet and legs, lose in a week all the general symptoms of their disease, walking about apparently in their

former health; and this benefit has been brought about without any evident operation of the medicine, besides its influence upon the mouth." (p. 345.)

Where there is much congestion of the bronchial mucous membrane, preventing the due decarbonization of the blood, and when the circulation of this blood through the brain adds to the nervous irritation, and prevents rest, bleeding is recommended; but in such a posture of the patient that, on the approach of syncope, he may immediately lie down. This depletion alleviates the state of congestion and oppression of the chest, and gives time for the establishment of mercurial action, and it also contributes to remove a disease (bronchitis), which of itself is often highly dangerous. We must quote the following remarks on the connexion between cardiac disease and inflammation or congestion of the mucous membrane of the bronchi in the author's own words, because of their great practical importance, more than on account of any novelty which they contain. (p. 346.)

"I am convinced, from attentive observation, that death is frequently hastened, if not produced, either by failing to notice, or not actively treating, the morbid condition of the bronchial mucous membrane existing in cardiac diseases. Cold, we know, acts as a stimulus to the pulmonary mucous membrane; and the disordered state of this tissue in heart diseases increases its susceptibility to be impressed by atmospheric vicissitudes. Persons, not aware of this fact, by exposing themselves in the winter season, often put their lives in imminent danger. For the same reason, epidemic influenza becomes highly hazardous to individuals suffering from disease of the heart. So decidedly am I impressed with the correctness of these views, that I consider it much more important to this description of patients to be placed, in the winter, under the influence of a regulated warm temperature, than to the truly consumptive."

In the mild cases of bronchitis, complicating organic disease of the heart, Mr. Salter considers that the mercury alone may be trusted to; but he does not wish to be regarded as recommending mercurialization for the cure of inflammation of mucous membranes generally. If the dropsy attending these cardiac diseases do not materially interfere with any vital functions, it may safely be left to the influence of the mercury: this drug, in these disorders, often proving the most efficient diuretic. In worse cases, Mr. Salter has found elaterium preferable to digitalis. Elaterium is most safely given when there is firmness of the anasarcous swelling. If the limbs be soft, doughy, and transparent, something more mild should be tried; and, when the elaterium has carried off the dropsical effusion, the use of mercury should be immediately commenced, and cautiously continued until the gums are slightly sore; and this action should be kept up until the breathing is decidedly relieved, and for some short time afterwards, and should then be had recourse to on a recurrence of the difficulty of breathing. In this manner, by the use of elaterium, mercury, and (if in the winter) confinement to the house in rooms whose temperature should never be below 50° F., the complaint may be kept at bay, and the fatal event warded off for a very long period. The relation of six cases, chosen from among many others, illustrative of the benefits of this treatment, terminates Mr. Salter's communication. They are all well worthy of perusal, and strongly confirm the recommendation of ptyalism as a palliative means in organic diseases of the heart.

The next paper is by Dr. **England**, of Wisbeach, and contains an account of "*two Cases of Gangrene of the Lungs*," which presented nothing very novel. This is followed by a very interesting relation, by Dr. O'Bryen, of Bristol, of "*a Case of partial Ectopia Cordis and Umbilical Hernia*," which, however curious, and indeed, we believe, unique, must give place to matter of a more practical kind. The case is well worthy the attention of both physiologists and pathologists.

This part of the volume terminates with the relation of an interesting and successful case of "*Extirpation of the Eye, on account of a Tumour within the Optic Sheath*," by Mr. Middlemore.

The next two articles are "*Reports of Out-Patients treated in the Birmingham Town-Infirmary*," by Mr. T. Ryland and Mr. S. Berry. These appear to have been drawn up with great care. From the former report we may abridge a case of *Strangulated Hernia*, as encouraging hope under what appears to be the worst circumstances. The case was that of a stout widow, æt. 86, who had been subject to femoral hernia, on the right side, for twelve months. *13th March:* For the first time it gave her pain, and suddenly increased in size. Vomiting occurred, and the bowels became painful and constipated. *17th:* Mr. Ryland first saw the case: the hernial tumour was as large as an orange, tense, rather sore, and quite irreducible. Bowels were not distended, and had been slightly opened on the 14th; tenderness in the right iliac region. A castor-oil enema brought away a large quantity of fæces; everything swallowed produced vomiting.

The female would not submit to an operation, which, although offering comparatively small hopes of a successful issue, it was considered right to recommend. She continued to vomit everything swallowed, together with a large quantity of fæcal matter. In this state she continued for many days, getting gradually weaker, but suffering no pain from the hernia, and only a little occasional uneasiness in the bowels. On the twenty-fourth day of the strangulation she complained of being famished to death; she therefore ate heartily of bread and cheese, and drank beer with much relish. On the following morning she passed flatus, and soon afterwards a large quantity of fæces, which, in the course of the day, amounted to a chamberpot full. She was weak, but had not vomited since the first motion; the hernial tumour was undiminished. She took gin and water in small quantities, and recovered.

Dr. Ogier Ward also gives an excellent "*Report of Cases treated at the Birmingham Dispensary.*" He suggests, as an interesting question, why, in the present report, as well as in all those which he has examined, the proportion of deaths in males is so much greater than in females; while the cases of disease in females are so much more numerous? In speaking of the cases of *Amenorrhœa*, Dr. Ward mentions that they were all attended with more or less headach, and that, in the treament of this symptom, he was guided less by a regard to the constitution of the patients than by attention to the effect of the recumbent posture in alleviating or aggravating the pain: such patients as are relieved by lying down being almost invariably benefited by antispasmodics and tonics, particularly iron; while those who sleep with the head raised, and whose eyes are swelled in the morning, require depletion, local and general. In two cases of *Anæmia*, the *bruit de diable* was observed; a symptom which

Dr. Ward has lately noticed in many cases, not only of anæmia, but of general debility, in which it was conjoined with palpitations, in women past the menstrual period. He has not met with the *bruit de diable* in a male patient; and he alludes to the fact of the British Association having confirmed his opinion as to the venous origin of the sound; though he doubts the correctness of their statement that they have observed it in the arteries also, believing that, in such cases as appear to justify this conclusion, the sound was caused in the veins accompanying the arteries. A case is related (p. 440,) in which it appears that *perforation of the intestines* occurred five weeks before producing fatal effects. The case is very interesting, although not absolutely justifying the inference that the perforation had existed for so long a period; but it is too long to extract, and should not be abridged.

There are many very interesting facts in Mr. MIDDLEMORE's "*Report of Cases, attended during the year* 1837, *at the Birmingham Eye Infirmary.*" One of these is that of an ossified capsule of the crystalline lens, which was removed through a section in the lower part of the cornea. "The anterior hemisphere of the capsule was almost entirely converted into a smooth plate of bone, except near the margin of the union between the anterior and posterior hemispheres of the capsule, where it constitutes a ragged ring of bone." We subjoin the following remarks on "fistulous opening communicating with the anterior chamber." A cork struck a woman in the eye; a few days after which was found a small, nearly transparent tumour, just without the margin of the cornea, which contained a small quantity of aqueous fluid. On its removal by a minute needle opening, it soon reappeared, and the iris appeared somewhat narrower on the side of this vesicular enlargement. No astringents effected its contraction, and, on opening it again with a fine needle, it soon refilled. It afterwards gradually diminished on the application of nitrate of silver, and it has not since reappeared.

Mr. Middlemore has tried the effects of various local means, much vaunted of late, in certain affections of the eyes; and he communicates the results of his experiments as follows:

*Veratrine and Aconitine Ointment.* In only one case of amaurosis, out of eight subjected to the treatment, was the slightest benefit obtained—in the case of a soldier suffering from dimness of vision, which, however, was not so great as to prevent him from walking about the streets of Birmingham, and managing the sale of vegetables at home. The pupil of the eye was rather large and sluggish, and there was just that sort of muddy green appearance within the eye which is noticed where chronic inflammation of the septa of the hyaloid membrane has induced a slightly turbid condition of the vitreous fluid. The pupil (and Mr. Middlemore mentions this as a very common effect of these applications,) became smaller and more active; and he thinks his sight considerably improved. After discontinuing the remedy, his sight was very little better than it was before he used it: (Mr. M. does not say which of the above ointments he employed in this case.) In some cases of neuralgia of the eyeball, he has used these ointments with unequivocal advantage. They consist of four grains of aconitine or veratrine to half an ounce of lard. A quantity, the size of a *small* nut, is to be rubbed above the eyebrow, by means of

Fig. 1.

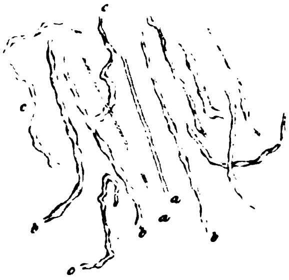


Fig. 2.

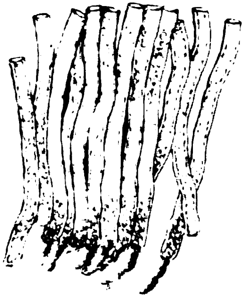


Fig. 3

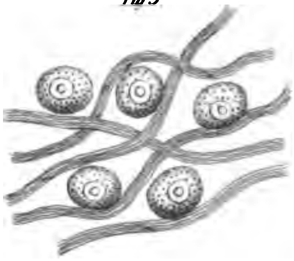
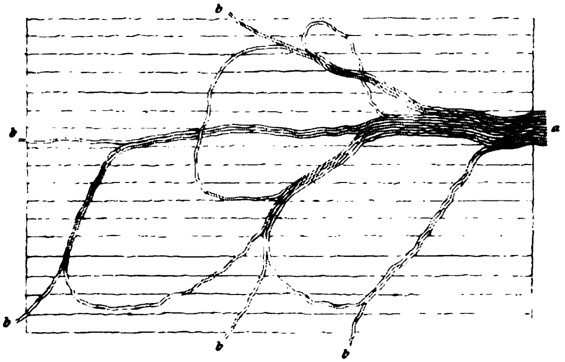


Fig. 4.



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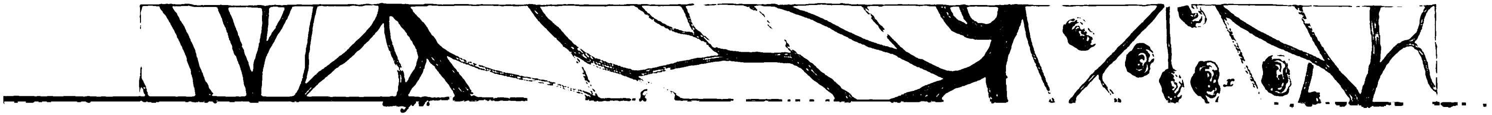
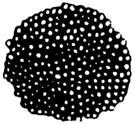


Fig 6



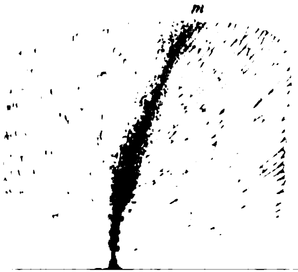


Fig. 8

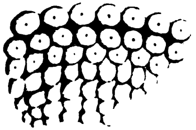


Fig. 9.

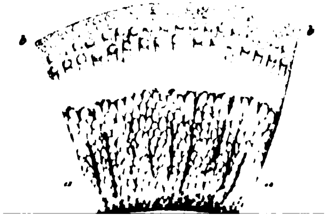


Fig. 10

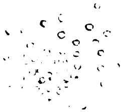
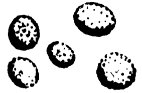


Fig. 11



a bit of sponge attached to a convenient handle, until the skin begins to smart and feel very hot: the rubbing to be practised daily.

As we know that the recommendations lately given to apply the solid nitrate of silver to the eye in some states of disease have led to a destructive mode of using it, and are likely, in the hands of rash practitioners, to lead to still further mischief, we subjoin Mr. Middlemore's mode of using the Nitrate of Silver, in certain forms of amaurotic affections: "Having a portion of nitrate of silver worked to a delicate point, I touch the cornea near its junction with the sclerotica, so slightly as merely to produce a small eschar; on the detachment of which a minute, superficial, and perfectly healthy ulcer remains, which very readily heals and becomes imperceptible; and this I do at about four points, which are comprised within the half of the cornea." *Use of Strychnia:* In two cases of amaurosis, after the failure of the other methods, a blister was placed in the front of the ear, and a small quantity of strychnia was applied to the raw surface left after its removal, with the most satisfactory results; especially satisfactory on account of the success of this after the failure of other methods of treatment.

Our limits will not permit us to notice the last article, which is a "*Statistical Account of the In-Patients of the Medical Wards in the Geneva Hospital, for the years* 1834, 1835, *and* 1836: *to which are added, Documents relative to the Influence of the Seasons on the Development of certain Diseases amongst the poorer Classes of Geneva and its Environs.* By Dr. LOMBARD, Physician to the Civil and Military Hospital of Geneva." It is, however, a very valuable paper, worthy of its distinguished author, and showing the benefits derived by the Association from its corresponding members.

The volume terminates with a "*Report upon the Influenza or Epidemic Catarrh of the Winter,* 1836-7," drawn up by Dr. STREETEN, of Worcester, from the communications of numerous members residing in various parts of England, in reply to a set of queries issued by the Council. This is a paper of so much value, and the subject is so interesting, that we purpose devoting a separate article to its consideration.

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## ART. VIII.

1. *Beobachtung einer auffallenden bisher unerkannten Structur des Seelenorgans bei Menschen und Thieren.* Von C. G. EHRENBERG, &c.—*Berlin,* 1836.

   *Discovery of a remarkable Structure of the Brain and Nerves in Man and Animals.* By C. G. EHRENBERG. *With six Copper-plates, representing the Structure of the Brain in Man and thirty Genera of Animals.* Read before the Academy of Sciences of Berlin, October, 1833.—*Berlin,* 1836. Folio. pp. 57.

2. *Anatomie der mikroskopischen Gebilde des menschlichen Körpers.* Von Dr. JOSEPH BERRES, kaiserl. königl. ordentl. öffentl. Professor der Anatomie an der Wiener Universität.—*Wien,* 1837.—*Anatomia microscopica Corporis Humani.* Auctore Dre JOSEPHO BERRES, Pro-

fessore publico ordinario in Universitate Vindobonensi, &c.—*Viennæ*, 1837.

*Microscopical Anatomy of the Tissues of the Human Body.* By Dr. JOSEPH BERRES, Professor of Anatomy in the University of Vienna.—*Vienna*, 1837. Folio. *Plates.* Part V. *On the Nervous Substance.*

3. *Beiträge zur Aufklärung der Erscheinungen und Gesetze des organischen Lebens.* Von GOTTFRIED-REINHOLD TREVIRANUS. Ersten Bandes, zweites Heft. *Neue Untersuchungen über die organischen Elemente der thierischen Körper und deren Zusammensetzung.—Bremen*, 1835.—Ersten Bandes, drittes Heft. *Nachträge zu den Beobachtungen des Verfassers über den innern Bau der Retina.—Bremen*, 1837.

*Contributions to illustrate the Phenomena and Laws of Organic Life.* By GOTTFRIED REINHOLD TREVIRANUS. Vol. I. Part II., *containing New Researches on the Organic Elements of Animal Bodies and their Combination.—Bremen*, 1835.—Part III. pp. 91-101. *A Supplement to the Observations of the Author on the intimate Structure of the Retina.—Bremen*, 1837. 8vo.; *with Plates.*

4. *Vorläufige Mittheilung microscopischen Beobachtungen über den innern Bau der Cerebrospinalnerven und über die Entwickelung ihrer Formelemente.* Von ROBERT REMAK. (*In Müller's Archiv. Jahrgang*, 1836. Heft II.)

*Provisional Communication of microscopical Observations on the intimate Structure of the Cerebro-spinal Nerves, and on the Development of their elementary Forms.* By ROBERT REMAK. (*In Müller's Archives, for the year* 1836. Part II.)

5. *Ueber den Verlauf und die letzten Enden der Nerven.* Von G. VALENTIN. (*In Nova Acta Cur. Nat., &c.* Vol. XVIII.)—*Breslau und Bonn*, 1836.

*On the Course and Terminations of the Nerves.* By G. VALENTIN. (*In Nova Acta Cur. Nat., &c.* Vol. XVIII.)—*Breslau and Bonn*, 1836.

6. *Ueber die Endigungsweise der Nerven in den Muskeln, nach eigenen Untersuchungen.* Von Dr. FRIEDRICH-CARL EMMERT, Privatdocenten an der Hochschule in Bern.—*Bern*, 1836.

*On the Mode of Termination of the Nerves in the Muscles.* By Dr. FREDERIC CHARLES EMMERT, Private Lecturer in the University of Berne. *With two Lithographic Plates.—Berne*, 1836. 4to. pp. 35.

7. *Beitrag zur mikroskopischen Anatomie der Nerven.* Von Dr. ERNST BURDACH, Prosector und Privatdocenten an der Universität zu Königsberg. *Mit zwei Kupfertafeln.—Königsberg*, 1837.

*Contribution to the Microscopical Anatomy of the Nerves.* By Dr. E. BURDACH, Prosector and Private Lecturer in the University of Koningsberg. *With two Copper-plates.—Koningsberg*, 1837. 4to. pp. 76.

8. *Elements of Physiology.* By J. MUELLER, M.D., Professor of Anatomy and Physiology in the University of Berlin, &c. *Translated from the German, with Notes, by* WM. BALY, M.D., Graduate of the University of Berlin, and Physician to the St. Pancras Infirmary.

*Illustrated with Steel-Plates and numerous Wood-Engravings.* Part III., *containing the Nervous System, and completing* Vol. I.—*London,* 1838. 8vo. pp. 848.

"Das Wissen wird im Suchen sich entfalten,"—Knowledge will unfold herself to him who investigates: such is the motto on the title-page of Ehrenberg's memoir. The career of the esteemed author is a commentary on it. By his patient and persevering researches he has unfolded a wonderfully complex organization in beings previously supposed to be little more than specks of homogeneous matter: nay, as Burdach remarks in the work the title of which we have placed above, he has even shown that certain dead masses of stone consist of antediluvian infusoria.

By the observations detailed in the memoir now under notice, Ehrenberg has certainly not unfolded everything that could be wished regarding the intimate structure of the brain and nerves; still he has cast such a blaze of light on the subject as has already served to guide to more extended and accurate knowledge.

Knowledge will unfold herself to him who investigates,—provided always the investigations be conducted in a proper way. We must enter upon the path of scientific enquiry, as in every other search after truth, in a true spirit of simplicity. Throwing aside all preconceived notions, we must be contented to see only what nature presents. In matters of pure science there is no occasion to hurry on, right or wrong, to some conclusion: it is much better, when we cannot obtain facts enough, either by the observation or interrogation of nature, to suspend our judgment. *No* conclusion is better than a wrong one; and, where judgment is suspended, it is more likely that greater efforts will be made to clear up the subject, than if ignorance were cloaked over by a conclusion of some kind. These reflections will be found to serve in some degree as the argument of the following historical sketch, the materials of which we have extracted from that contained in the first section of Ehrenberg's memoir.

As the structure of the brain and nerves is entirely a microscopical matter, so the history of our knowledge of the subject commences only with the time when the microscope was first applied to anatomical investigation: whatever was emitted on the subject previously was mere conjecture. Although it is curious to observe that what is now recognized to be a fact—viz. that the brain and nerves possess a tubular structure,—is an old and widely spread notion, still those who first promulgated the doctrine had no definite idea of the thing as it really exists. Some admitted it merely to tally with their own wild speculations regarding the function of the brain and nerves; others, though they acknowledged they could not assert the thing as an observed fact, still subscribed to the opinion.

Though the history of our knowledge of the intimate structure of the brain and nerves commences with the first application of the microscope to anatomical purposes, we encounter at first rather errors committed by the employment of that instrument than any important observations made. It is with the microscope as with the eye itself: it is not enough that a man see with it,—he must observe with it. From inattention to this, perhaps, rather than from the badness of the instruments employed, imperfect as they confessedly were at first, arose the errors on the subject, not only of the structure of the nervous substance, but of all other parts of the body. "Hence," says Burdach, (p. 4,) "it may be affirmed that Monro would have come to the same conclusion that he did, even though he had had a microscope by Frauenhofer; and that, on the other hand, Ehrenberg, even with Della Torre's imperfect glass lenses, would have made out the structure of the nervous substance as he has now done." Even at the present day we are not unfrequently met by ill-observed facts, or assertions that the enquirer has not been able to see what others have unequivocally observed.

Every tissue must be examined in as unchanged a state as possible. Alteration by putrefaction, or any other reagent, is sometimes a means of investigation,—a means of leading us to discover the true and natural state of things; but the state of any substance, as changed by such reaction, ought never to be taken into account when the structure as it exists in the living body, comes to be the question. How far, then, must those physiologists have erred who imagined, as Ehrenberg remarks, (p. 37,) that they could discover the true structure of organic substances, too delicate to be examined by them in the natural state, after having been boiled, hardened in spirits, or dried! It is against the abuse, not the use, of reagents in our minute anatomical enquiries that we speak: we therefore consider that Burdach has done well in studying the appearances which the nervous substance presents under different influences, chemical and mechanical, at different ages, after different kinds of death, and at different periods after death, as also in the morbid state. Valentin, too, has investigated the subject more or less in the same way; and Remak has paid particular attention to the appearances presented by the nervous substance at different ages.

An opinion prevails that high magnifying powers are indispensable for minute anatomical investigations: but this we know, that, by properly displaying the substance to be examined, and with a practical knowledge of and dexterity in manipulating on the part of the examiner, not only the structure of the nervous substance, but also many other structures, may be very well demonstrated by inferior magnifying instruments; by much lower powers than a person unacquainted with the subject might be led to imagine. A simple lens, magnifying 150 to 200 diameters, will show very distinctly parts, to see which properly it is often pretended a compound microscope of high power is indispensably necessary. Globules much more minute than those of the blood can be distinctly seen with such powers; and it is to be remembered that it does not require so high a magnifying power to see a fibre or tube as to see a globule of the same diameter. We wish to insist upon this point, and to show that it is not the extent of magnifying power alone which assists the microscopical enquirer. Without properly isolating and displaying the object to be examined, to increase the magnifying power only leads to error. Farther, we venture to assert that the exclusive employment of the compound microscope is not good. On account of its affording a higher magnifying power with a larger field, and enabling us to make exact admeasurements with great facility, it is of the utmost value for the examination and admeasurement of what has been already displayed by dissection under the simple microscope. The compressing instruments, so much used of late, can never serve as a perfect substitute for such dissection. Compression is a very valuable means in many cases, but in others it distorts the thing examined, or destroys it, and prevents it from being seen altogether.

As by the compound microscope we see merely the inverted image of the object, it is a thing next to impossible to manipulate under it. Emmert (p. 8,) appears to have done so: his manipulations, however, must have been very imperfect. In regard to displaying the primitive parts of the substance of the brain and spinal marrow, of ganglions, and the like tender structures, Burdach says (p. 14) that tearing asunder the parts by means of knives or needles is not applicable; "as such an operation, even when performed by the most steady hand, produces too great confusion and destruction of parts: it is, on the contrary, perfectly adapted for the demonstration of the primitive fibres of the peripheral nerves, as the latter preserve in the recent state a considerable degree of firmness." All this is true, if the simple microscope is not used first for the purpose of displaying the parts. Under the simple microscope, even with a magnifying power of 150 or 200 diameters, (though 100 diameters will in general suffice,) it is possible to display the most tender structures: if tearing asunder with fine needles be too rude and violent a mode, the parts may yet be separated and displayed by gentle touches, and by patiently agitating the water. The mere circumstance of putting the object under examination into motion frequently gives us valuable information. In displaying the part under the simple microscope, we can observe what changes take place during the process, and at the same time we can guard against the confusion which is so apt to occur; seeing that, in the most minute state of the manipulation, the eye guides the hand. We hold the use of the simple microscope in this way an indispensable accompaniment of the compound microscope, and we can easily trace many of the oversights of physiologists to the neglect of it.*

But to recur to our historical sketch:

Malpighi's microscopical examinations were confined to boiled brain, which he compared to a pomegranate full of seeds. In these he found glands and vascular saccules, because, says Ehrenberg, (p. 32,) he looked for them; having the idea that all the viscera possessed a glandular structure. After Malpighi, Leeuwenhoek, who may be considered the founder of microscopical anatomy, investigated the structure of the nervous substance, about the end of the seventeenth century; and, though some of the results he came to be not entirely discordant with reality, (less so even than the opinions which until lately prevailed on the subject,) still they are too imperfect to serve as a basis for any physiological deduction. A second series of investigations of the structure of the brain, which he instituted thirty years later, when, as he himself says, he was an old man, is absolutely of no value; as, instead of recent brain, which he had first employed, he took for the object of his second examinations dried brain. Leeuwenhoek was more successful in his investigation of the structure of some of the nerves. As early as 1674, he examined the structure of the optic nerve in the ox, and found it to consist of a filamentous substance, like vessels, filled with slowly moving globules. In the optic nerve of the horse he found globules similar to those larger and transparent ones which he had observed in the brain in the vicinity of the spinal marrow. In 1715, he announced that he had just discovered that the muscular nerves of animals are composed of from forty to twenty filaments; that not only are the individual nervous filaments hollow, but all are provided with many cavities. This assertion, which he put forth as being possibly subject to error, he confirmed, in 1717, by a direct observation of the cavities of the nerves in a cow. There is not in the nerves, according to him, any perceptible motion of fluid or a circulation. Here and there he found the spinal marrow present a structure similar to that of the nerves, but always very confused and obscure.

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* As an example of this may be adduced the fact of Purkinje and Valentin having searched in vain for the germinal vesicle in the mammiferous ovum, and at last detected it only on again resuming their investigations after learning that Coste, of Paris, had affirmed he had seen it. But, even after they found it, these acute investigators missed observing the spot on its side; a part which, in Germany, was shortly after pointed out by Wagner. In contrast with all this, we can state the fact that, in this country, about the same time the observations were made on the continent, the germinal vesicle of the mammiferous ovum was discovered, the spot on its side noticed, and a complete description with delineations of the thing given, by Mr. Wharton Jones, whose observations, we have the means of knowing, were made by the assistance of an ordinary simple microscope only, the highest power of which magnified about 200 diameters. No compression was used, but the parts simply displayed and dissected under the miscroscope, with common sewing needles stuck into wooden handles.

After Leeuwenhoek, Della Torre, Monro, and Fontana prosecuted the enquiry; but, unfortunately, though they examined the self-same substance by similar means, they arrived at contradictory results; a circumstance which materially retarded the farther development of the subject by bringing discredit on the microscope. Of the three, Fontana's observations come nearest the truth.

The lenses with which Della Torre made his observations were small unpolished glass-globules, prepared by himself. Having compressed the substance of the brain to be examined between plates of mica and glass, every organic texture was destroyed. He therefore represented the brain as a pulpy mass, consisting of globules suspended in a clear viscid fluid, and arranged here and there in rows. This view of the subject was supported by Prochaska, who differed only in considering the medium in which the globules were contained a delicate cellular tissue, and the nervous globules of equal size throughout; whereas Della Torre described them as being of different sizes in different parts of the brain and nerves.

Monro, ignorant of the mode of using the microscope, made his observations whilst the direct rays of the sun were transmitted through the substance under examination. The consequence was, as every one at all acquainted with the microscope knows, that he found, not in the substance of the brain only, but in every other tissue,—nay, even in inorganic substances,—solid fibres wound together in a serpentine manner. He, however, afterwards saw his error.

Fontana's observations were much more comprehensive; and he appears to have made out the structure of the brain and nerves such as has recently been demonstrated; though, from not being determined or delineated with sufficient accuracy, his observations led to no results. Hence what he did see correctly was attributed more to optical illusion than to reality.

In recent times two opinions, the converse of each other, and both erroneous, seem to have prevailed. Barba and others maintained the old view of Della Torre and Prochaska of globules in a mucus; other investigators admitted no globules, but fibres as the basis of the cerebral substance. Some persons, again, held a middle opinion. Hence, in physiological writings, we meet with such expressions as *globules of the brain*, *fibres of the brain*, or *cerebral tissue*.

The opinion that the nervous substance consists of fibres has been chiefly founded on examinations made of brain hardened by spirits, or from the existence of the lines and striæ visible to the naked eye and low magnifying powers. The medulla of the nerves has been pretty generally considered as prolongations of the cerebral substance contained in the membranous cylindrical tubes formed by the neurilemma. Among those who held the middle opinion, that the nervous substance consists both of fibres and globules, were Sir Everard Home and Mr. Bauer. Their observations, though made under every advantageous circumstance, are of no value. They appear, indeed, to have seen the structure described by Ehrenberg under the name of varicose or jointed tubes, but to have taken it for venous branches with valves; and, in correspondence with the notion, they have erroneously represented it as branched. Regular rows of globules, such as they represented their fibres to consist of, are not found in nature. Ehrenberg, in his historical notice, has not mentioned Sir Everard Home's doings expressly; and we only allude to them here to show how little worthy they are of being adduced along with those of Ehrenberg, as has lately been done by a contemporary critic.

At page 219 of our fifth volume we gave a short notice of the views of the structure of the nervous substance promulgated by Professor Berres, of Vienna, soon after Ehrenberg announced his discovery. Professor Berres has recently reiterated those views, both by description and delineation, in the fifth part of the work, the title of which we have placed among the others at the head of this article. Although we have done so, we have not thought it necessary to introduce any of his views into our discussions, because they are too little consonant with what is really found in nature. By this we do not mean to say that Professor Berres has asserted anything which he has not seen: on the contrary. But his error is that he has put down everything that he has seen; and, trusting to magnifying power alone, he seems to have forgotten that it is necessary to analyze what one sees; to separate, dissect, display, and to trace under the microscope. "No doubt," Burdach remarks (p. 8), "he has really seen all that he has described and delineated, but has subjected it to a false explanation." Valentin (*Repertorium*, vol. i. p. 61,) says, in speaking of Berres' account of the nervous substance, that, supposing it possible he might have overlooked, in his previous examinations, the relations of things described by Berres, he examined every point of the subject anew, and satisfied himself that most of Berres' assertions are incorrect. The motto affixed to Professor Berres' work is "Non conscripsi ad narrandum sed ad probandum." This we have great pleasure in saying, from our own observation of Professor Berres' injected preparations, is very applicable to that part of his work which treats of the ultimate distribution of the vessels; but the converse would have been more applicable to what he has written on the nervous substance.

In our attempt to collate the different modes of viewing the intimate structure of the brain and nerves adopted by the authors whose works lie before us, and to deduce what may be considered as established facts, we shall distribute the subject into three heads: 1, the intimate structure of the brain and spinal marrow; 2, that of the nerves and ganglions; and, 3, the mode of termination of the nerves.

## I. THE INTIMATE STRUCTURE OF THE BRAIN AND SPINAL MARROW.

A. *The gray substance of the brain.* This consists, according to Ehrenberg,* (p. 19,) "of a close and very fine vascular network." Besides this, he has also observed, "at the very periphery of the gray or cortical substance, a soft mass consisting of very fine granules, in which are deposited, here and there, larger grains in detached heaps or in strata. The larger grains are free, and consist of still smaller granules; the very fine smaller ones of the mass appear simple; and, wheresoever their smallness, softness, and transparency allow us to form an opinion on the subject, they seem joined together in rows by tender threads. In the vicinity of the white or medullary substance, *the fibrous character of the gray or cortical substance becomes more and more evident;* and, in the same proportion, the blood-vessels become somewhat larger and fewer in number."

B. *The White or Medullary Substance of the Brain.* "This," says Ehrenberg, (p. 20,) "presents still more distinctly a fibrous structure. The fibres are, in fact, evidently direct continuations, gradually becoming thicker, of the finer cortical fibres. . . . They are not simply cylindrical, but resemble rather rows of hollow beads, not touching each other, but separated by the intervention of short tubes, or they resemble exactly tubes presenting dilatations at intervals." The tubes under consideration are always straight, and for the most part run parallel to each other; sometimes they intercross. Ehrenberg says that, in his almost innumerable examinations, he has only four times observed a division of the tubes into branches, but he never saw an anastomosis between any. In the bundles of tubes near the base of the brain, as also in the medullary substance lining the ventricles, there are always found tubes thicker than the rest, in which an outer and an inner line, indicating the boundary of the tubular walls, may often be perceived quite distinctly, and consequently the width of the bore or caliber determined. "Hence," says Ehrenberg, (p. 20,) "we can no longer speak of fibres or filaments as composing the brain, but of *tubes or canals presenting dilatations at intervals;* or, in other words, *varicose or jointed tubes or canals.*"

In regard to the spinal marrow, Ehrenberg says (p. 23,) that the gray substance of it, which is known to be situated interiorly, and the white substance, which is exterior, have the same structure as the corresponding substances of the brain.

In short, Ehrenberg gives out in the work before us (p. 37,) as established facts: That the substance of the brain consists of tubes dilated at regular intervals, and which he calls *varicose* or *jointed tubes;* these tubes lie parallel to each other or in bundles, are from 1-96 to 1-3000 of a line in diameter, and run from the surface towards the ventricles and base of the brain, becoming thicker as they converge: they pass into the spinal marrow, the greatest part of which they form: they are not held together by any cement or cellular tissue that can be discerned: and that the spinal marrow of man, and all the great divisions of the vertebrata, consists of varicose or jointed tubes, exactly like the brain: with this difference, that the finer tubes (of the gray substance) lie inside, the thicker ones (of the white substance) outside, and all have a predominating parallelism one to another.

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* In his researches, Ehrenberg always uses the compound microscope, but he has not generally employed the highest powers. All the observations which he details were made by powers magnifying about 350 to 360 diameters. Under these powers, also, he made his drawings. Higher powers he has only used to determine and confirm what he saw with the lower.

These results of Ehrenberg's researches into the structure of the brain, as well as of those into the structure of the nerves, by and bye to be noticed, were read before the Academy of Sciences of Berlin, in 1833, and an abstract of them published in Poggendorff's Annals of Physics for the same year. It was not long before they were put to the test by other observers; some of whom agreed with Ehrenberg, while others raised objections to certain of his views, and that on grounds more or less substantial. All admitted the general results; and, indeed, it could not be otherwise, seeing that they are not very difficult of demonstration.

Of those who have written on the subject, and offered corrections of Ehrenberg's views, since the latter first published his discovery,* the late distinguished physiologist, Gottfried-Reinhold Treviranus, of Bremen, Valentin (now Professor of Anatomy in Berne,) and Burdach, the younger, of Königsberg, deserve to be particularly named.

Treviranus (2tes Heft, p. 28,) distinguished in the gray substance of the brain fine cylinders, closely arranged and interwoven, which he calls *primitive cylinders;* and in the white substance what he calls *medullary cylinders*, each of which derives its origin from the union of several of the primitive cylinders. (p. 40.)

The primitive cylinders and the medullary cylinders of Treviranus are respectively the same things as the very fine varicose or jointed tubes described by Ehrenberg in the gray substance, and the larger varicose or jointed tubes, which, according to him, compose the white substance. So far there is little difference of opinion; but it will be seen that, according to Treviranus, several of the primitive cylinders unite to form a medullary cylinder; whereas Ehrenberg describes the tubes of the gray substance and those of the white as simple direct continuations of each other. The most palpable point in which Treviranus (p. 31,) differs from Ehrenberg is this: that he denies the beaded, varicose, or jointed form to be a constant and essential property of the tubes composing the substance of the brain, but declares it to be merely accidental; the simple effect of putrefaction, pressure, or other reagency after death. This assertion of Treviranus is now recognized to be correct, we believe, even by Ehrenberg himself. But it is to be kept in mind that the readiness with which the tubules composing the cerebral substance assume the beaded, varicose, or jointed form is a remarkable and distinguishing characteristic.

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* Valentin mentions in a paper (in Müller's Archives, 1834, p. 409,) on the primitive fibres of the brain, that, several years before Ehrenberg published, Purkinje used to demonstrate them in his lectures. Valentin says that he himself saw them so demonstrated by Purkinje, in the session 1829-1830.

Valentin (p. 93) considers the beaded, varicose, or jointed appearance as accidental; but, for distinction's sake, he still retains the name of *varicose fibres*, for which, as above seen, Treviranus employs the designation of *medullary cylinders;* a term which we had better not adopt, as it might be liable to be confounded with the cylindrical tubules of the nerves, which contain what Ehrenberg calls *medulla*, hereafter to be noticed. Valentin describes the gray substance as consisting "of an aggregation of globular masses, between which he finds in the yellow substance,—that is, the transition substance from the gray to the white, the numerous but isolated loop-like terminations of the cylinders of the white mass." We shall consider the globules of the gray substance when speaking of the ganglions.

The view given by Ehrenberg as to the course of the tubules of the brain from the surface towards the ventricles and base does not coincide with that of Valentin, who says (p. 92) he has satisfied himself "that the appearance to the naked eye, as if the individual fibres, or bundles of fibres, diverged, is quite unfounded. These bundles of primitive fibres form rather, according to him, the most beautiful and intricate plexuses, which present peculiar characteristics in different places." In regard to this assertion of Valentin's, Burdach, in mentioning that Valentin uses a curved scissors for cutting off pieces of the brain for examination, asks (p. 17) that, as this means is apt, by the pressure necessarily produced, to bring the component fibres into confusion, "might not, perhaps, the plexusf-ormations observed by Valentin in the substance of the brain and spinal marrow, be considered, in part, at least, as depending on the employment of the scissors; seeing that the fibres of a nervous bundle, which certainly run in a parallel direction, may, by pressure and shoving, be so easily changed into a plexus?" At page 24, Burdach again says, in regard to this, "Such manifold plexus-formations, or loop-like terminations of the cerebral fibres, as Valentin has observed and described them, I have indeed never seen; but consider the investigation of the course of the organic elements of the brain so difficult, that I by no means offer my observations, which were merely incidental, in opposition to those of Valentin."

Leaving out of view the differences of detail in the descriptions of the cerebral substance given by Ehrenberg, Treviranus, and Valentin, and the different modes of expression employed, we perceive that, taking them generally, they all agree in this, that the white substance of the brain is composed of fibres or tubes; and that the gray substance also is composed of fibres or tubes, together with granules. Excepting, then, that the varicose or jointed appearance of the tubes is not original and essential, and, even when they exist, perhaps not so regular as Ehrenberg represents them, but that they occur in consequence of post-mortem changes, and with greater or less irregularity, we may remark that the description, as given by Ehrenberg, agrees perfectly with what can be always readily demonstrated. Valentin's loop-like terminations of the cerebral fibres in the gray substance are not of such easy demonstration. As to the union of the fine fibres of the gray substance to form the thicker fibres of the white, as asserted by Treviranus, we think it more doubtful.

It is commonly considered a very difficult matter to demonstrate the primitive fibres or tubes of the brain, and that it is especially necessary to employ for the purpose very high magnifying powers. We can assert, from our own experience, that there is nothing more unfounded. A simple microscope, with lenses magnifying 150 to 200 diameters, will be found to serve the purpose very well. If then, to begin with, the observer will take a minute piece of recent brain at the root of one of the nerves, (say the auditory,) and spread it out carefully in a drop of water on the object-glass, he will not fail to see hundreds of tubes, presenting less or more the varicose or jointed appearance described by Ehrenberg, according to the freshness and undisturbed state of the brain, or contrariwise. Having thus once seen and become acquainted with the appearances, it will be easy to display and examine the tubular structure in all other parts of the brain. (See Fig. 1.) It is, of course, necessary to employ only very minute pieces of the thinnest possible slices of brain; and these will often require to be torn, separated, and displayed by means of needles.*

c. *Organization of the Primitive Fibres or Tubules of the Brain.* The interior of the so-called varicose or jointed tubules is throughout as clear as water: hence, says Ehrenberg, (p. 21,) they must contain some such matter as vapour, water, or clear jelly. Ehrenberg, however, was not able, even with a magnifying power of 3000 diameters, to determine exactly the nature of the contents: at least, he could see nothing of a granulary, or of any other particular appearance; and he is inclined to suppose that the contents are a perfectly transparent and tenacious substance. From our own observations, we can affirm that it is so. We have, in fact, seen it oozing out of the broken extremities of the tubules, and glueing them to the object-glass, so that they adhered like leeches; and the free ends of the tubules moved about when the water under which they were examined was agitated. A microscopical doublet, magnifying 150 to 200 diameters, will be found sufficient to show this, much better, indeed, than a higher compound power; because it is evident that to see a transparent fluid, it is not so much high magnifying power as good light and definition that are required. We shall hereafter see, as Ehrenberg says, that this very transparent and tenacious substance differs considerably in respect to tenuity from that contained in the cylindrical tubules of the nerves.

For the contents of the tubules of the brain, Ehrenberg (p. 22) proposes the name of nervous fluid, *liquor nerveus*, whilst he retains the name of nervous medulla, *medulla nervea*, for the substance contained in the cylindrical tubules of the nerves; and he suggests that the term medullary substance of the brain, *medulla cerebri*, if still retained, should be employed only as a conventional term. He thinks, however, that *white substance of the brain* would be better. We have already seen that Ehrenberg's so-called varicose or jointed tubules are called by Treviranus medullary cylinders, (Markcylinder.)

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* The structure of parts, such as it can be seen by the naked eye, now no longer suffices for physiology; we therefore counsel such of our young friends who would cultivate properly this most important branch of medical science to procure forthwith a microscope, and examine for themselves. It is not necessary to get an expensive instrument. Such a *simple microscope* as may be had of Mr. Ross, optician, in Regent street, Piccadilly, London, for six guineas, will enable any one to make himself acquainted with the intimate structure of every part of the body as far as it is yet known.

In regard to the nature of the walls of the tubules of the brain, Burdach supposes (p. 29) "that the elementary fibres of the brain and spinal marrow are not in general provided with sheaths of cellular tissue, but lie with their own substance in close contact, without the intervention of any other matter. In this case they should consist of a substance, the outer peripheral part of which, being more viscid and of greater consistence, would form a sort of rind or shell; whilst the inner, central part remained fluid. Our own observations lead us to agree with Burdach. The following extract from Wharton Jones, describing the investment of the newt's ovum, illustrates very well what Burdach means: "The ovum of the newt differs from that of the frog, inasmuch as the gelatinous-like matter which surrounds the yelk and its membrane is of an oval form, *and is somewhat hardened on the surface so as to form a kind of shell, inside which is a fluid substance*, in which the yelk and its membrane can freely revolve and glide from one end to the other.*

## II. INTIMATE STRUCTURE OF THE NERVES AND GANGLIONS.

A. *Cerebro-spinal Nerves and Ganglions.* According to Ehrenberg, (p. 24,) "all the nerves examined by him, with the exception of the optic, auditory, and olfactory nerves, and the sympathetic in the middle of its course, consist of cylindrical tubules, of an average thickness of 1-120th of a line,† running parallel to each other, and never anastomosing." The latter fact, indicated by Fontana, and afterwards by Prevost and Dumas, was first demonstrated by J. Müller, who showed that the primitive fibres of the nerves run in uninterrupted continuity from the centre to the periphery, always lying merely alongside each other, and nowhere dividing or inosculating. Kronenberg (*Plexuum Nervorum Structura et Virtutes*, Berolini, 1836, 8,) gives, by his researches, additional confirmation to this opinion.

The cylindrical tubules, united together in bundles, form the nervous cords. Each single bundle, and the whole cord, are surrounded by a vascular fibrous sheath, called *neurilemma*. In reference to the neurilemma, we would here introduce the following observations by Burdach, who, we may premise, has principally occupied himself with the investigation of the structure of the spinal nerves; having directed his attention only incidentally to the study of the substance of the brain and spinal marrow, and that merely for the sake of comparison with the substance of the nerves. The nerves he examined were for the most part those of frogs, and sometimes also of the smaller fishes, birds, and mammifera; as (pp. 9, 10,) "nerves just removed from the living animal are alone fit for the examinations to be instituted." According to Burdach, (p. 18,) when we examine a nerve in the undisturbed state, an undulated appearance is observed in it lengthways. This is owing, *not*, as Valentin supposes, to a contracted state of the neurilemma,—to an alternate elevation and depression of the fibres of the cellular tissue constituting the neurilemma; just, in fact, like the wrinkled state of the investment given to the cords of spiral wire in elastic garters; *but*, according to Burdach, to the primitive fibres of the nerves which lie in an undulated position, the neurilemma being shorter than they. "We have here," says Burdach, (p. 19,) "a wise arrangement of nature, by which, on the occurrence of any pulling and tearing of a part provided with nerves, the neurilemma must first be considerably stretched before the extension can affect the primitive fibres, which are relatively longer, and lie loose in their investment." According to Valentin's view of the matter, on the occurrence of any pulling or tearing, the primitive fibres would be the first to suffer, in the same way that the coil of wire in elastic garters is drawn out considerably before its sheath, being stretched to the full, can prevent further extension or elongation.

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* On the First Changes in the Ova of the Mammifera, &c. (In Phil. Trans. Part ii. for 1837, p. 339.)

† The cylindrical tubes, Ehrenberg says, are found, both thickest and thinnest, in the invertebrata; being sometimes as thick as 1-48th of a line, and in other cases as thin as 1-1000th of a line in diameter.

To return to the cylindrical tubules of the nerves. Ehrenberg says he has satisfied himself that the individual tubules are not invested by any covering of neurilemma. Very frequently the different nervous bundles of one and the same nerve unite by what Ehrenberg calls a false anastomoses: that is, the tubules of one bundle enter into and pursue their course in another, without, however, as has been already pointed out, the individual tubules ever inosculating.

It is a fact established by the observations of Ehrenberg, Treviranus, and others,—and to the correctness of which we ourselves, from our own observations, subscribe,—that the cylindrical tubules of the nerves are direct continuations of those of the brain and spinal marrow. The latter become thicker in caliber, and stronger in their walls in the transition. This transition part of the nervous tubes undergoes less readily the change to the varicose form; some slight dilatations only presenting themselves.

According to Valentin, the primitive fibres of the spinal nerves do not terminate in the spinal cord, but pass on to the brain. The primitive fibres of the nerves which join the extremity of the spinal cord run forward; whilst those of the nerves, entering the spinal cord laterally at its upper part, proceed first transversely towards the interior of the cord, as far, or nearly as far, as the gray substance, and then follow the same longitudinal course to the brain as the others. In the white substance the fibres lie side by side; but, where the white and gray substances touch each other, the fibres are separated by the intervention of the globules of the gray matter, presently to be described, and at last radiate through the cortical substance, where (as we have already mentioned), Valentin says, they form loop-like terminations, by uniting with one another. This is most distinctly seen at the point of union of the white and reddish-gray substances, or in the yellow substance at the periphery of the hemispheres of the cerebrum and cerebellum. (For what we have to say of the ganglions of the cerebro-spinal nerves, see under the head "Sympathetic Nerves and Ganglions.")

B. *Organization of the primitive Fibres or Tubules of the Nerves.* "The purely cylindrical tubules of the nerves," says Ehrenberg, (p. 25,) "differ from the varicose ones of the brain essentially in this, that they have stronger coats, a much larger caliber, and contain in their interior a very evident and less transparent medullary substance, which appears to have been often observed before, though not very accurately." This medullary substance, much grosser than that contained in the tubes of the cerebral substance, can be readily seen oozing out of the cut extremity of the nervous tubule, and diffuses itself in the water under the form of irregular shaped flakes. (See fig. 2.) Remak (*Müller's Physiology*, and also his *Archiv*. 1837, p. iv. *Jahresbericht*,) describes the contents of the nervous cylinder, as Fontana had done before, to be either a perfectly solid fibre, of rather less diameter than the cylinder itself, or a pale flat filament, separable by pressure for a considerable extent here and there from the investing tube, which is prone to become wrinkled or puckered. He could not detect any more minute fibrous structure in this filament, although it sometimes becomes split into several threads. Remak further states, that the contents of the cerebral tube is, as in the fibres of the nerves, a coherent thread; but, like the tube itself, of a much more delicate nature. The simple expression of all this is, as we have already stated,—the contents of the cerebral and nervous tubules is a viscid substance, which, allowed to ooze out slowly, coagulates into an irregular shaped flake; but, if drawn out, whether by pressure or any other means, it is spun into a thread.

As to this subject of the contents of the primitive fibres of the nerves, "I have," says Burdach, (p. 21,) "never been so fortunate, even by examination as soon after death as possible, to find the primitive fibres of a bundle wholly and throughout with perfectly clear contents: there was always in them, here and there, a mass consisting of irregular round particles, which probably, for the most part, gives, by the refraction of the light, a darker appearance to the primitive fibres." The contents Burdach observed to ooze out in the form of a clear, thick, fluid, colourless mass; which was some time after distinctly converted again into a mass of irregular globular particles. Ehrenberg thought that the contents of the cylindrical tubules of the nerves was a granulous fluid; but we agree with Valentin (p. 115) and Burdach, that the substance contained in the primitive fibres of nervous substance in general, both of the brain and of the nerves, is a half-fluid, somewhat viscid, transparent, oily-like matter, which is changed into a granulous mass only by coagulation. Treviranus (p. 38) describes the contents of the nerve-cylinders as a soft matter, in which globules are sometimes seen.

Ehrenberg says, (p. 26,) that he has convinced himself in many ways that the fibres of the nerves are hollow. "In the first place, there are distinctly perceptible in each tube four parallel lines, two of which form the outermost boundary lines, but the inner two indicate the limits of the internal cavity. In the second place, a view of the tubes filled with medulla is very easily obtained by spreading them out with two needles, so as to avoid causing any or much pressure. By now laying a plate of glass over the part, and pressing it slightly, the tubes, which were previously filled with medulla, are seen quite empty, and the medulla forms at their ends a thick protuberance. In the third place," he continues, "I have been able to perceive, during the observation, even the movement of the contained mass caused by gradual and gentle pressure; and, in the fourth place, I have often, in transverse sections, perceived the *lumina* of the individual tubes most distinctly." "Each of these reasons," he adds, "and, much more, all taken together, indisputably prove the fact that the fibres of the nerves are hollow." The transition of the tube of the nerves into the primitive fibres of the brain Ehrenberg adduces as a proof of the tubular nature of the latter. He says, (p. 25,) "All doubt on this point is removed by the fact that the cylindrical tubes of the nerves pass directly into the varicose or jointed ones of the brain."

As to the inner boundary lines of the primitive fibres of the nerves admitted by Ehrenberg, Burdach looks upon them as an effect of the refraction of the light, produced by the fluid contents of the primitive fibres being more accumulated at the edges, causing this part to project above the centre. Burdach enters very much at large into the intimate organization of the primitive fibres of the nervous substance. We shall not follow him throughout all his enquiries; the following only we give. The way he accounts for the production of the varicose appearance which the primitive fibres assume is, (p. 29,) "That the contents of the primitive fibres of the brain and nerves undergo throughout a peculiar tendency to assume the state of globules after the extinction of life, but while they are still fresh; that the sheath of cellular tissue opposes this tendency more or less successfully, according to its strength, and the more so as the contents adhere in some manner to its inner surface by virtue of their viscidity. Hence varicosities are produced in the strong-sheathed primitive fibres of the peripheral nerves only after the action of mechanical or chemical influences, and that but imperfectly. In the same fibres, on the contrary, when, by reason of youth, they are thin-walled, the varicosities are more regular. Further, in the more or less tender fibres of the cerebral nerves, they appear with different degrees of distinctness and constancy; lastly, in the extremely delicate-walled fibres of the brain and spinal marrow, the varicose appearance occurs in the most regular and constant manner."

In regard to the question whether there be any movement or circulation of the contents of the tubules of the nervous substance, Ehrenberg says, (p. 27,) "My investigations hitherto on the nerves of living animals have not yet perfectly shown to me any circulation; and Leeuwenhoek likewise denies it distinctly. But whether Leeuwenhoek, at the place where he speaks of movements seen in the canals of the optic nerve of the eye of the ox, observed a circulation, however indistinct, is uncertain. This subject, as one of the highest importance to physiology, and one the decision of which is within our reach, I recommend to scientific investigators for their cooperation; particularly as, according to my experience, it is not very easy to reduce it to demonstration. Mere hasty assertions, for or against it, deserve no notice." Emmert (p. 30) lays down the following proposition: "The phenomena of muscular activity and loop-like mode of termination of the fibres of the motor nerves render it likely that there is a continuous current; a circulation of nervous fluid in the motor nerves." This proposition Emmert illustrates at considerable length, but does not support by very close reasoning. Burdach's experiments of tying a nerve (p. 42) do not favour the idea that there is any movement of the medulla in the primitive fibres; any real current of it in a particular direction.

Are the fibres or tubules above described really the ultimate elements of the nerves? Müller doubts that they are so, when he considers the great size of the so-called primitive fibres of the nerves as compared with the minute elementary parts of the muscles, the cellular and other tissues. Treviranus appears to think that the cylindrical fibres of the nerves are composed of more minute elementary fibres closely arranged alongside of each other: this he infers from having remarked stripes in the cylindrical tubes, running lengthways. In addition to this, Müller mentions that Schwann saw in the mesentery of the frog nervous fibres, of the thickness of the primitive fibres, from which still finer fibres ran out. The question resolves itself into this: Does the half-fluid contents of the nervous fibres or tubes, above described as primitive, present, in fact, an appearance not homogeneous, but as if composed of minute threads or filaments of half-fluid consistence? Neither Valentin, Burdach, nor we ourselves, have been able to see any such thing in the course of the nerves. We shall recur to this subject when speaking of the terminations of nerves.

c. *Is there any difference of Structure between the primitive Fibres of Nerves of Motion and of common Sensation?*—In the roots of most nerves, where they emerge from the surface of the brain and spinal marrow, Ehrenberg (p. 24) observed among the cylindrical tubes so-called varicose ones, nearly as thick, but which were for the most part likewise filled with distinctly visible nervous medulla. "Whether these mixed nerves, as they may be called," asks Ehrenberg, (p. 25,) "be the sentient, and the purely cylindrical ones be the motor, is a very important subject for further investigation?" In the supplement to his memoir, (p. 43,) Ehrenberg informs us that he examined, with Professor Müller, the sensiferous and motiferous roots of the nerves in the frog, and found no important difference in the microscopical structure. Lauth, and after him Remak, discovered so-called varicose or jointed tubes in different nerves. Remak says (p. 146) he found, in the sciatic nerve of a frog, where it divides into the peroneal and tibial nerves, *a large mass of varicose fibres.* He afterwards convinced himself that there are no spinal nerves without varicose or jointed fibres. All this, however, does not appear to be connected with a sensiferous or motiferous function, but to be merely a stage of development; for the occurrence of so-called varicose or jointed tubes is quite common in young animals; perhaps, simply for the reason we have above mentioned, viz. that the walls of the nervous tubules, being more delicate, they more readily assume the varicose form. Thus, according to Remak, (p. 148,) in a young rabbit, the second day after birth, all the cerebro-spinal nerves, throughout their entire course, consist of *transparent varicose fibres:* in other words, the primitive fibres or tubules of the nerves, being very delicate, like the fibres of the brain, readily assume, after death, the varicose appearance. In young but not very small frogs, Burdach (p. 38) frequently found in the ischiatic nerve, in addition to the cylindrical tubules, still some which had the varicose appearance; whilst, in a completely grown frog, it was always in vain that he looked for varicose tubes. In regard to the *roots of the spinal nerves,* Burdach (p. 10) could only confirm Ehrenberg's statement that both the sentient and motor roots exhibited no actual difference in regard to the appearance of the primitive fibres, except that those of the posterior roots appeared somewhat thinner; a remark which coincides with that already made by Emmert, (p. 9.) Burdach could find no fibres readily assuming the varicose form in the ischiatic nerve of the full-grown frog, even at its division into tibial and peroneal, as Remak had done. He found no so-called varicose tubes in the cutaneous nerves of the frog; only proportionally finer cylindrical tubes. Burdach therefore comes to the fixed conclusion that, in the spinal nerves of the adult frog, in the fresh state, and quite unchanged by compression or any other kind of treatment, there are no varicose fibres. He thinks all that can be inferred from Remak's researches is, that the elementary particles of the nerves acquire their full, regular development only after the complete and perfect growth of the individual; that, on the contrary, at an early period, they are, both in regard to their contents and their investments, more amorphous and more tender, and by handling easily assume an appearance not natural to them. "Hence," says Burdach, (p. 10,) "I have found young frogs, new-born rabbits, and recent human embryoes, absolutely useless for my investigations."

In regard to Remak's researches, it is to be remembered, however, that he distinctly says, in speaking of the greater thickness of the muscular than of the sentient nervous fibres, "These differences are so striking, that, putting out of view altogether the kind of fibres (varicose and such like), we can determine, merely from their thickness under the microscope, whether we have before us a muscular or a cutaneous nerve." Of the three nerves of the tongue, the lingual, according to Remak, resembled a cutaneous nerve; the glosso-pharyngeal contained at most places regular, fine varicose fibres, so that it differed from a mere cutaneous nerve; and the hypo-glossal was quite as a muscular nerve. The above remarks refer to the nerves of rabbits four or five weeks old. In full-grown rabbits, Remak found the varicose fibres,—that is, fibres capable of assuming the varicose appearance,—very rare; and the transition fibres,—that is, fibres somewhat less prone to assume the varicose appearance,—less numerous than in the earlier period. The difference between cutaneous and muscular nerves he found as striking as before; but it consisted, for the most part, merely in the thickness of the fibres, and the greater quantity of fibres without medulla (?) found in the cutaneous nerves; whilst the varicose fibres do not now any longer so generally occur, or are so characteristically frequent in the cutaneous nerves, and much less so in the muscular. Of the three nerves of the tongue, the hypo-glossal continued to present the characters of a muscular nerve, the lingual of a cutaneous nerve, and the glosso-pharyngeal contained the most varicose fibres and cylindrical fibres without medulla.

It thus appears that, with the exception perhaps of a slight difference in thickness, the primitive fibres of sentient and motor nerves have exactly the same appearance under the microscope.

We have already mentioned that we thought Burdach had done well in describing the appearances which the nervous substance presents under different influences, at different periods of life, after different kinds of death, and at different periods after death, as well as in the morbid state. Burdach's work contains intrinsic marks of the observations and experiments having been most carefully made and faithfully reported. Whatever conclusions, therefore, he draws from his researches are entitled to our best attention. We have already noticed some of the most important points of his observations on the structure of the nerves: we shall not follow him more particularly, but simply content ourselves by extracting his own summary of his researches.

"1. The tendinous appearance visible on the surface of whole nerves, or thick bundles of nerves, does not depend on undulating bendings of the cellular tissue forming the sheath, but on a serpentine position of the bundles of the primitive fibres within the sheath.

"2. The nerve appears to retain its sheath even within an organ.

"3. The primitive fibres are not finer within an organ than they are outside one.

"4. The contents of all primitive nervous fibres are, in the natural state, a clear, thick fluid, which is only changed by coagulation into a granulary mass.

"5. The primitive fibres are at first cylindrical, but sink in the middle after death, and when they are laid on a plane surface; whence, by the refraction of the light, they present the appearance of double boundary lines.

"6. The knotted or varicose form is, indeed, peculiar to the primitive fibres of the brain and spinal marrow, but not essential; and depends simply on this, that the contained medullary matter possesses a tendency to assume the globular form, and thereby to overcome the resistance of the sheaths.

"7. There are many circumstances and appearances which render it probable that the primitive fibres of the brain have not a sheath of cellular tissue, but consist of a somewhat more viscid cortical and a somewhat more fluid central substance.

"8. Cold contracts, heat expands, the nervous fibres. Water is indifferent. Vinegar dissolves and softens, first the cellular sheath, then the medulla: potass first the medulla, and then the sheath. Spirits of wine coagulate the medulla and thicken the sheath. Creosote and corrosive sublimate exert a similar action. Alum and saltpetre dissolve both sheath and medulla: common salt less so; and prussic acid appears to render fluid and expand the contents of the primitive fibres.

"9. The nervous fibres attain their full development more slowly than other organic structures: they consist originally of granulous masses, and pass through the varicose* form gradually, but not quite equably, to the cylindrical form.

"10 By age it is only the general nervous sheath and the neurilemma that become thickened; the primitive fibres themselves are not visibly changed.

"11. Decomposition by putrefaction goes on most quickly in the brain and spinal marrow, less so in the nerves of sense, most slowly in the peripheral nerves. Putrefaction takes place in the brain and spinal marrow more quickly when left in the dead body; in the peripheral nerves, more quickly when they are removed, and laid in water.

"12. After hemorrhage, the primitive nervous fibres have a torn collapsed appearance. After death by suffocation, the peripheral nerves are also gorged with blood. After death produced by prussic acid, the fibres of the brain appear in a cylindrical form, and quickly fall down into clear globules.

"13. The blood-vessels going to the nerves *do not penetrate betwixt the primitive fibres, but merely twist round the bundles of fibres, in a net-like manner.*

"14. In dropsy, and perhaps also by inflammation, (dropsy and inflammation in frogs, caused by cauterizing the skin with lunar caustic,) the primitive fibres of the nerves acquire the appearance of transparent tubes filled to bursting with fluid.

"15. Nerves which have been cut do not reunite *immediately*, but only by the *intervention* of cellular tissue: they appear closed at their cut end by medullary matter which has oozed out. No new nervous twigs enter into the cicatrice of healed wounds.

"16. In the primitive fibres of the nerves, there is no movement of the medullary mass in a particular direction." (p. 43.)

D. *Structure of the primitive Fibres of the Nerves of the three higher Senses.* We have seen that Ehrenberg considers the structure of the olfactory, optic, and auditory nerves as different from that of others. He describes them as composed entirely of the so-called varicose or jointed cerebral tubules. On this subject Valentin says (p. 52) the olfactory nerve has, almost along its whole course within the skull, an extremely delicate, fine, parallel, fibrous structure; the elements of which, lying close beside each other, constitute varicose threads. The optic nerve, on the other hand, is divisible into a multitude of thin bundles lying alongside each other, and visible to the naked eye; which bundles consist of a sheath of cellular tissue, and the fine primitive nervous fibres therein contained. The auditory nerve is distinguished by the peculiar fineness of the nervous fibres. According to Treviranus, (p. 36,) "The olfactory nerve consists, at its entrance into the nasal cavity, of bundles of cortical cylinders, uninclosed in any sheath; the optic nerve of medullary cylinders resembling those of the medullary substance of the brain; the auditory nerve of cylinders like those of the muscular nerves, but thinner." Volkmann could not always find varicose fibres in the nerves of special sense.*

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* It would be more correct to say "that state in which the varicose appearance is so apt to take place."—REV.

The above views we consider in some respects right, and in others wrong. According to our observations, the olfactory nerves within the cranium consist of small tubules, similar to those of the substance of brain; but the fibres of the olfactory nerve, where it ramifies in the pituitary membrane, present the thick-walled, cylindrical, tubular structure, though very fine, of other nerves. According to the observations of Wharton Jones, the auditory nerve, from its origin to where it enters the internal auditory meatus, presents most distinctly the delicate-walled, tubular structure of brain; but, within the meatus, it assumes the ordinary thick-walled, cylindrical, tubular structure of nerve. The same physiologist says further, "Examined under the microscope, the cylindrical tubules of the cochlear nerve appeared to me to be larger than those of the vestibular, and to contain, or at least to give out, a greater quantity of nervous medulla."† In regard to the optic nerve, it is composed throughout of the so-called varicose or jointed tubules, or the medullary cylinders of Treviranus, bound together by sheaths of cellular tissue, in bundles visible to the naked eye. The optic nerve thus appears to differ from the olfactory and auditory nerves, and indeed from all other nerves, in nowhere presenting the thick-walled tubular structure: we think, however, that even in it we have observed a slight approach to such a structure, at that place where the nerve becomes somewhat constricted, just before entering the eyeball. Müller says he has found the fibres of the optic nerve in the sheep, when recent, much more minute than they appeared when examined later. We have observed the same thing, we may say, of the cerebral tubes generally.

E. *Sympathetic Nerve and Ganglions.* According to Ehrenberg, (p. 38-40,) the sympathetic nerve has a mixed substance, consisting of so-called varicose or jointed tubes and cylindrical tubes. Valentin says (p. 85) of the sympathetic nerve, that its primitive fibres, examined under the *compressorium* in an uninjured ganglion, exhibit perfectly straight boundary lines, like the peripheral nerves in general. Müller says, "It is well known that the fasciculi of nervous fibres in the sympathetic nerve have, for the most part, a gray aspect, while those of the cerebro-spinal nerves are white: but the latter nerves also contain some few gray fasciculi mingled with the white and in many parts of the sympathetic nerve there are white fibres mingled with the proper gray or organic fibres." The sympathetic nerve having connexions with the anterior and posterior roots of the spinal nerves, the white fibres are derived from that source, and are both motor and sentient. It is a question, however, whether the sympathetic really contains any fibres *sui generis*. Valentin does not think so: he says, "the gray appearance of the cords of the sympathetic does not depend on any peculiarity of the fibres, but on ganglionic globules distributed here and there." Müller is inclined to think that the gray fibres of the sympathetic, after their connexion with the ganglion, are functionally different from the motor and sentient white fibres, which the latter derives from the cerebro-spinal nerves; yet, says he, it would be more simple and agreeable to suppose that all fibres, without exception, were the same, and differed only by the direction of the current or oscillation which took place in them. "However," he adds, "we are not come to that point of theory, and it still remains a remarkable fact that twigs of the sympathetic mingle themselves in other nerves, and still retain their peculiarities."

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* Neue Beiträge zur Physiologie des Gesichtssinns.—Leipz. 1836.

† See article "Organ of Hearing," in vol. ii. of the Cyclopædia of Anatomy and Physiology, p. 540.

Of the *ganglions*, Ehrenberg says, (p. 31,) that they differ in structure. Almost all have this in common,—that they consist of collections of varicose cerebral tubules, which either alone form the ganglion, as is the case with the *chiasma opticum;* or they are mixed with thick cylindrical nervous tubules, as was found in all the ganglions of the sympathetic examined by Ehrenberg. In the case of the latter, the nervous tubules are enclosed in a delicate close vascular network, in the meshes of which are granules, such as are found in the retina and the cortical substance of the brain. In following the course of the varicose tubules of the ganglions, Ehrenberg could perceive very distinctly that they gradually became thicker until they nearly equalled the cylindrical nervous tubes in thickness; still they always retained their peculiar varicose structure. "The idea," says Ehrenberg, "that ganglions may be compared to small brains, is favoured by a knowledge of the structure." Thus, "the substance of the ganglions consists of a mixture of vessels and very delicate, scarcely distinguishable varicose tubes; in fact, of true cortical substance, and of a preponderating quantity of thicker varicose tubes; in fact, of true medullary substance. This cerebral substance lies around cylindrical nervous tubes, which do not become changed in it, but are strengthened by an admixture of varicose tubes in their bundles."

According to Valentin, (p. 78,) the original type of the ganglionic structure consists in this, that one or several bundles of fibres, which enter into the ganglion, form within it a more or less intricate plexus, according to the nature and size of the ganglion. But besides this, individual primitive fibres, or isolated bundles, consisting of very few fibres, run round on all sides of the peculiar ganglionic globules (see fig. 3). These globules are composed of an external fine coat of cellular tissue, within which is a nucleus, in the circumference of which is a second smaller nucleus; they have often also pigmentous deposits on them. These ganglionic globules may be very readily demonstrated in the *locus* *niger*, and other black spots of the brain. The gray substance of the brain and spinal cord is formed wholly of the same globules as the ganglions of the vertebrate animals. The appearance of minute granules is produced by the disintegration of the original globules, which are very soft. The only circumstance in which the globules of the gray substance of the brain differ from those of the ganglions is, that the cellular tissue which invests the former is more delicate. In the white substance of the brain there are no globules or granules; any appearance of granules in it is produced by the nervous fibres being broken up. On the quantity of the deposition of gray globules between the primitive fibres depends the degree in which certain parts of the brain differ in colour from the white or fibrous substance. Where the primitive fibres are in greatest number, the colour is whitish gray; where they are less abundant, it is reddish gray; where, on the contrary, there exist only simple terminating loops of fibres between the globules, the mass is yellow. The darker colour of certain portions of the brain depends on a pigment deposited on the globules.

Farther, Valentin describes the structure of the ganglions of the cerebro-spinal nerves as not differing essentially from that of the ganglions of the sympathetic; but in them the pencil of fibres is more distinctly seen passing through unchanged between the globules of the proper substance of the ganglion.

Somewhat resembling the ganglionic globules above described, Ehrenberg observed, in the interior of the ganglions of the leech, large club-shaped bodies, filled with granules, and presenting a clear nucleus in the centre. Ehrenberg had frequently seen similar bodies in other parts of the nervous substance, but their connexion always remained indistinct to him. He describes the club-shaped bodies in the leech as forming eight bundles, two of which are prolonged into each of the four arms of the ganglion by means of long cylindrical tubes. Valentin has described similar bodies as existing in the ganglions of the abdominal cord of the leech. He saw globules which, like those of the ganglions of the higher animals, contained a nucleus. In this nucleus there was, at a point close to the surface, a reddish body, and sometimes, instead of this, there were several smaller corpuscles. Purkinje saw similar caudate bodies in the yellow mass between the cortical and medullary substance of the hemispheres of the cerebellum. He describes them as having a bright nucleus in the interior, and corresponding to it a smaller one on the surface. They are arranged side by side; their rounded extremities turned in towards the white substance, their tail-like prolongations out towards the cortical substance. As similar to the above, Müller regards certain club-shaped nucleated bodies, which he found in the medulla oblongata of the cyclostomatous fishes, (in a petromyzon preserved in spirits.) But they were in this case of a peculiar shape: their thick extremity was seldom rounded, but for the most part dentated; there being two, three, or four tooth-like processes, the form and position of which varied very much. Emmert (p. 8) mentions his having found, in the upper part of the spinal marrow of a rabbit, below the medulla oblongata, on the posterior surface of the spinal marrow, several club-shaped bodies which lay lengthwise in the spinal marrow beside and upon each other. "At one end," says Emmert, "they are clubbed and bounded by a curved line, at the other end they pass into fibres; their club-shaped swellings are not all of the same size, the line bounding them being sometimes more, sometimes less, distant from each other."*

## III. TERMINATIONS OF THE NERVES.

A. *Terminations of the Nerves in the external Skin and Muscles.* Some years ago Rudolphi, in Germany, and Prevost and Dumas, in France, described what they supposed to be a termination of the nerves in the muscles by loops, but they appear to have referred only to loops formed by the larger nervous ramifications. This is especially the case with Rudolphi's observations which were made by the unassisted eye.

Valentin and Emmert discovered the mode of termination of the nerves in the muscles about the same time, but by a different procedure as regards the manipulation. Burdach confirms the accounts of Valentin and Emmert, and gives, in addition, the mode of termination of the nerves as he has made it out in the skin of the frog.

What we have hitherto extracted from Burdach's work is contained in the first section. Having only incidentally examined the structure of the brain and spinal marrow, he has limited himself chiefly to the investigation of the structure of the spinal nerves. "Although," says he, "these my investigations, so far as they have hitherto gone, have yielded by no means brilliant results, and are to be looked upon rather as a work preparatory for further researches, I have nevertheless been induced to communicate them in the first section of these sheets, seeing that I have been conducted by them to the observations contained in the other two sections; which might, indeed, appear more worthy of being made known."

We shall first consider the mode of termination of the nerves in the muscles, as that was first discovered, and as it is more simple than the mode of termination of the nerves in the skin.

In order to demonstrate the point, Emmert stretches an abdominal or pectoral muscle of a frog on a plate of glass; and, when it has adhered to the latter by drying slightly, he shaves it thin with a cataract knife. The following is his description of the mode of termination of the nerves in the muscles as demonstrated in this manner. "In what way soever the terminating nervous fibrils arise, they have always the same course and the same termination. They go off from the other primitive fibres, in the direction towards which the farther expansion of the nervous cords is carried, consequently, towards the periphery; they run in a serpentine manner over the muscular fibres; and, having described on them a larger or smaller arch, they return to another nervous cord or another nervous bundle, or unite with some other single primitive fibre which has just run a similar course, and accompany it to a nervous cord, to which they apply themselves and in which they then go backwards to the larger nervous stems. The arches of the primitive fibres which are formed in this manner on the muscular fibres, and of which the convexity is directed towards the peripheral expansion of the nerves, exhibit multiplied diversities in regard to the size and form of the curve." (p. 19.)

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* The nucleated structure of the globules of the gray substance of the brain and of the ganglions is not peculiar to those parts: it is found in the corpuscles of cartilage, in the epithelium of the mucous membranes, in the epidermis. Werneck, of Salzburg, has described a new layer within the capsule of the lens, (See our last Number, p. 207,) which, as we can testify from our own observations, consists of small bodies with nuclei in their middle, like the small bodies of the epithelium. The small bodies of the membrane of the pigment of the eye also present the central nucleus. We have found also nucleated bodies in the bulbs of the eyelashes. Valentin communicates observations by Purkinje, which he himself has repeated, on the structure of the choroid plexus; viz. that these shaggy organs are covered by a fine transparent epithelium, the globules of which have a polyhedral circumference. Each globule contains in its middle a dark round nucleus. In man, the middle of each cell has externally a round granule of pigment corresponding to the central point of the position of the nucleus in the inside. The vibratile cilia of various surfaces are connected with the corpuscles of their epithelium.—REV.

Valentin, in his examinations, made use of the recti muscles of the eye of man and the smaller mammifera, the cutaneous muscles of the mammifera, the abdominal muscles of different small animals, all the muscles spread on the inner surface of the cavity of the trunk, and, lastly, the nervus intercostalis internus of rabbits, guinea-pigs, &c. These he examined without any farther preparation than under the *compressorium*. Burdach makes use of a thin muscle, previously laid a few minutes in vinegar, which has the effect of rendering the muscular substance of a horny transparency. During the examination he employs slight compression. By this mode of examination, Burdach always found the termination of the nerves in the muscles as follows.

> "To each individual muscle there goes, in general, only a single nervous stem. . . The nerve, having entered the muscle, first runs downwards in it to some extent parallel with the muscular fibres . . . After this pretty straight course, the nerve begins to split into thicker and thinner branches, which again subdivide into twigs of a few primitive fibres. These branches and twigs proceed over and under the muscular fibres obliquely, or with a slight arch; more rarely quite across; frequently intercross, and then form, as they approach nearer the end of the muscle, a plexus, by frequently joining and separating from each other. This is what Valentin calls the *terminating plexus*. By means of it is produced a frequent interchange of primitive fibres among the twigs of the same branch, or of different branches, or even of different nervous stems, in the case where the muscle possesses several such. From this plexus there come forth, lastly, at a point still nearer the end of the muscle, ramuscules, which, being resolved into single primitive fibres, or bundles of very few fibres, form arches or loops, the convexity of which looks towards the end of the muscle, the concavity towards the stem of the nerve. Thus are formed Valentin's so-called *terminating loops*. After having formed the loops, the single primitive fibres, or bundles of a few fibres, again unite together, and re-enter the plexus, and through this return to their nervous stem." (p.54.) See fig. 4.

Burdach's examinations into the mode of termination of the nerves of the skin were made on the integument of the frog; and for this purpose he found very convenient a circumstance he discovered,—viz., that, by the action of vinegar, the skin of the frog may be divided into three layers.

The integument of the frog covers the animal quite loosely like a sack, and is only here and there connected with the subjacent parts, particularly where blood-vessels and nerves enter into it or pass out from it, and where cutaneous muscles are inserted. Of the three layers into which the skin is divisible, the outermost is the epidermis; the second, the corpus mucosum, or seat of the pigment; and the third, or innermost, is the chorion. This layer of the skin consists of dense cellular tissue. In it we find the cutaneous nerves, blood-vessels, and glandular structures lying spread out before us. With the exception of the part lying on the middle of the abdomen, this chorion, as long as it is in the moist state, is so transparent that, whether the microscope be directed to the inner or outer surface of it, the whole expansion of the nerves can be seen from the cut end of the trunk, lying as yet outside the skin, to the very finest ramifications, without a single twig escaping the eye.

"As soon as the trunk of the nerve enters the skin, it divides into three or four branches, which run in a diverging manner towards opposite sides. The cutaneous nerve first runs to some extent rather parallel with the skin; but after that must make a bend, however slight, in order to penetrate into the skin. . . . . Most frequently the division of the nerve is into three branches: where the trunk divides only into two, (a very rare circumstance,) one of the two, or even both, give off, after a short course, a thick twig which runs in an opposite direction. The branches which go off divergingly now ramify in their slightly serpentine course. . . . The twigs given off sometimes again join the parent branch, or some other branch of the same stem, after a very short course; but more frequently they maintain for a longer time their independent course, in which case they become thinner and thinner by dividing and by the giving off of ramuscules, consisting of more or fewer; sometimes even of only a single primitive fibre. The branches which have become thin by giving off twigs, the thickest as well as the most slender twigs, as well, lastly, as the bundles consisting of a greater or less number of primitive fibres, or even single primitive fibres, arising from these in the shape of ramuscules, form with each other and with the branches, twigs, and ramuscules[*] of other nervous stems, a very complicated plexus. In this they sometimes join each other by apposition; sometimes, again, separate from each other by dividing and ramifying. If we now follow a nervous branch continuously through this network, overlooking for the time all its twigs in their farther course, we find that when, in consequence of giving off ramifications, it has diminished to a few primitive fibres, it again gradually increases in thickness; seeing that new bundles, sometimes thinner, sometimes exceeding itself in thickness, always join it and accompany it. Being thus more and more enlarged, it appears at last as a branch of quite another nervous stem. The same thing is found to be the case in regard to every other, even the smallest of its twigs, and ramuscules. We can trace each of them to a foreign nervous stem, through the most complicated unions and separations of the network. From this it appears that the primitive fibres of the nerves entering into the skin do not lose themselves there *among or in blood-vessels*, as Ehrenberg suspects; nor do they end, as Treviranus thinks, in cutaneous papillæ; nor do they even, as Valentin affirms, and which is really the case in regard to the muscular nerves, go back to their parent stem after having formed terminating loops; *but after they have separated from their parent stem in thicker or thinner bundles, rarely quite single, they form with each other, and with similar bundles of other cutaneous nerves, by alternately uniting and separating, a very close multiplied network; and after that pass immediately into other cutaneous nerves, in order to return with these back to their central organ.*" (p. 47.) See fig. 5.

The following is Burdach's comparison between the mode of distribution and termination of the nerves, in the skin and in the muscles.

1. The cutaneous nerves, immediately on their entrance into the skin, divide into several branches; the muscular nerves, on the contrary, first run a certain distance in the muscles before their division begins.

2. The branches of the trunk of the cutaneous nerve immediately diverge towards different sides, and even their farther subdivisions follow

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[*] Burdach makes use of the words, *branches, twigs,* and *ramuscules,* not in reference to their *comparative thickness,* but only in reference to their relation to the parent stem. The expressions "coming off," "joining," and the like, as applied to branches, twigs, &c., are not to be taken in an absolute sense; for the *branch, twig,* or *ramuscule,* which is described as coming off from a stem, branch, or twig, may in reality as well be said to join it.—Rev.

no definite direction. In the distribution of the muscular nerves, on the contrary, a general direction corresponding to the muscular fibres predominates; although individual branches run across or obliquely over the muscle, in order to spread over the whole breadth of it.

3. The cutaneous nerves, by splitting and ramifying, give off fasciculi which unite with each other and with parts belonging to other cutaneous nerves. These fasciculi form a very multiplied plexus, equally spread over the whole surface of the skin, and representing here and there very regular figures. The muscular nerves form a similar plexus, the so-called *terminating-plexus;* but it is not spread equally over the whole muscle, it is limited to a part of it only: in this place there is also a predominating longitudinal direction, so that it represents only oblique-angled meshes.

4. The individual primitive fibres of the cutaneous nerves, followed through the cutaneous network, pass into another cutaneous nerve, and in this back to its central part; the primitive fibres of the muscular nerves, on the contrary, after emerging from the plexus, return, forming a loop, back to their stem, branch, or even twig.

Valentin has described the loop-like termination of the nerves in the iris and ciliary ligament, and even in parts exclusively sensible; as in the follicle of the teeth, in the skin of the frog, in the interior of the cochlea of birds, in the ampullæ, and in the sack of the vestibule. Breschet has also described the auditory nerve as presenting a loop-like termination; whilst, according to the observations of Wharton Jones, the termination of the auditory nerves resembles more that of the optic.*

A principal result of Valentin's investigations into the mode of termination of the nerves is the doctrine "that the nerves have, properly speaking, no peripheral termination; but that, in their peripheral organs, the centrifugal part passes without any definite change into the centripetal." "This doctrine," says Burdach, "has been proved by Valentin's investigations principally in regard to the motor nerves, and by my own observations in regard to the sentient nerves also of the spinal marrow." He adds, that this principle will probably be found to hold good in regard to all other nerves and in all other peripheral organs; but, as the finest blood-vessels in every organic tissue have a peculiar and characteristic distribution, so surely will the distribution of the nerves within each organ be peculiarly arranged.

The results of Burdach's investigations into the distribution of the nerves in the tongue and mucous membrane of the mouth, (in the frog,) are as follows:

1. The hypo-glossal nerve is distributed to the muscles, and that quite like other muscular nerves, forming plexuses and loops; but differs from them by its ramifications coming off only on one side, and has, with most of the other cerebral nerves, this in common, that the stems of the two sides are not united together by their ramifications.

2. The twigs corresponding to the lingual nerve of the fifth pair belong to the mucous membrane of the mouth and the posterior part of the tongue, and exhibit in these parts an arrangement very similar to that of the cutaneous nerves. They differ from the cutaneous nerves, however,

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* Cyclopæd. of Anat. and Phys., vol. ii. pp. 541, 542.

inasmuch as a greater part of their primitive fibres, after a course of a shorter or longer distance from the stem, return to it, (the transition into the stem of the other side Burdach has not yet completely proved); and, moreover, as they never appear to split into single fibres; and, lastly, as they form here and there small knots, which Burdach conjectures to be ganglions.

3. The glosso-pharyngeal nerve of the frog passes through the muscular structure of the tongue, without giving any branches to it, and without forming any plexus. It forms, close at the surface of the tongue and its mucous membrane, a plexus, composed of its finest ramuscules, remarkable for the looseness with which the primitive fibres lie alongside each other. The nerve resolves itself, lastly, into its elementary cylinders which run quite isolated and form terminating loops.

In regard to the functions of the hypo-glossal, lingual, and glosso-pharyngeal nerves, Burdach remarks:

> "If we suppose the degree of sensibility of a part to depend in general on the peripheral end of a nerve, it cannot be doubted that a nerve splitting into its finest elementary parts, must manifest greater sensibility than another in which the primitive fibres remain in thicker bundles; so that the nerve, forming an extended network, but one not composed of the finest elements, must be less adapted for special sensation. If we compare, therefore, under this point of view, the very fine ramifications of the optic nerve in the retina, on the one hand, and the so-extended network, but in thicker bundles, of the cutaneous nerves, on the other, it would not be too bold to say that, considering the above-described morphological formation of the glosso-pharyngeal nerve, it is a pure nerve of sense; and that, considering the formation, on the contrary, of the twigs representing in the frog the lingual nerve of the fifth, that it is a nerve of common sensation; and, if we add, moreover, that the hypo-glossal, considering its form, so similar to other muscular nerves, must be a muscular nerve, so would it be proved, by my microscopical investigations of the terminations of the nerves of the tongue, what Panizza has by his experiments physiologically demonstrated." (p. 72.)

We have seen that Remak's observations of the microscopical appearance of the component primitive fibres of the same nerves have led him to a similar conclusion. From Dr. J. Reid's experiments* it would appear that the glosso-pharyngeal is a nerve of common sensation, and certainly not the special nerve of taste.

To Valentin's conclusion, "that the nerves, properly speaking, have no peripheral termination, but that, at their peripheral organ, their centrifugal part passes without any definition into the centripetal," Burdach says he would, in order to make the nature of the termination of the nerves completely evident, add, that "the essential character of all pure nerves of sense consists in this, that they form at their peripheral part a very fine plexus, and resolve themselves into their finest elementary parts; the essential character of nerves of common sensation, moreover, whether they belong to the cerebral or spinal system, depends on this, that they form multiplied, wide-extended plexuses, which consist for the most part of nervous bundles, rarely of single primitive fibres. The essential character of muscular nerves, lastly, is, that within the muscle they form a plexus, partly consisting of thick bundles, and then terminating loops, which very rarely consist of isolated primitive fibres."

That the nerves terminate in the manner above described by Valentin, Emmert, and Burdach is not admitted by all anatomists. Thus,

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* Cyclopædia of Anatomy and Physiology, vol. ii. p. 499.)

Treviranus (Heft ii. p. 42,) suspects that, at the terminations of the nerves, the fibres lay aside the sheath in which the cortical fibres are enclosed, to form (as he thinks) a nervous fibre; and that the cortical fibres, thus set free, again separate from each other. This is not the case, he says, with all nerves; but it occurs at some places. These assertions of Treviranus cannot maintain their ground alongside the clear demonstrations of Valentin, Emmert, and Burdach. Müller says, (p. 604,) "it is not very probable that the so-called primitive fibres, which are of considerable size, form the actual termination of the nerves, in parts of which the ultimate elements are much more minute than they. Schwann, indeed, has seen in the mesentery of the frog, issuing from the so-called primitive fibres, numerous finer filaments which here and there presented small knots, from which again several twigs were given off. Further researches on the mode of termination of the nerves in the tail of the larva of the toad have confirmed these observations. The nervous fibrils resulting from the splitting of fibres, of the size of what are ordinarily termed primitive fibres, are excessively minute, and are destitute of the dark tubular sheath which invests the ordinary primitive fibres. The minute knots are almost always present. From the minute fibrils just described, and from the knots on them, still more delicate threads are given off, and terminate by forming a network." On these observations of Schwann, Valentin (*Repertorium*, vol. ii. p. 54,) remarks: "So far as I have been able to observe, these fine fibres have nothing in common with the contents of the nerves, and are merely fibres of cellular tissue, belonging either to the nervous sheaths or accidentally lying beside them. The arched termination of the primitive fibres can be very well observed in the recent conjunctiva of the salamander, in addition to those numerous organs mentioned in my larger work on the nerves."

B. *The Nervous Expansions in which the Optic, Auditory, and Olfactory Nerves terminate.* Ehrenberg first discovered in the retina a tubular structure. According to him, the retina is, in fact, a cerebral substance, formed chiefly by an expansion of the optic nerve covered and penetrated by a close vascular network; in the meshes of which there is found anteriorly a thick layer of free grains, which consist of still smaller granules, and have the greatest resemblance to the globules of the blood.* Ehrenberg describes the substance of the retina as consisting, like the brain, of the so-called jointed or varicose tubular structure in two forms, —viz. 1, of a very fine part, which is, so far as can be made out, *jointed gray substance;* and, 2, of a more distinctly *jointed white substance.* The latter is next the optic nerve itself, of which its fibres are distinctly the continuations. In the retina of the rabbit, the structure consisting of tubes readily assuming the jointed or varicose appearance, is very distinct. The tubules compose, by their aggregation, those two leashes of white streaks seen radiating from the entrance of the optic nerve. The same thing exists, but less distinctly, in the human eye. Ehrenberg mistakes when he says that the tubular structure is in the part which has hitherto been considered the so-called serous layer, or membrane of Jacob. The two parts are quite different: both exist; and each has its own peculiar structure.

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* We pass over entirely Ehrenberg's speculations as to the identity between the globules of the blood and those he describes certain parts of the nervous substance to consist of; as also his views regarding the nature of the thymus gland, as being mere conceits.

In the eyes of some animals, as frogs and fishes, but not in man, Ehrenberg has observed staff-shaped or club-shaped bodies, or papillæ, on the inner surface of the retina; the connexion of which with nerves and vessels, however, remained undetermined. He says, "their connexion with the varicose or jointed tubes of the nerve is not quite clear to me." Ehrenberg says he has convinced himself of the existence of globules in the expansion of the olfactory nerves in the nose, similar to those of the retina. Club-shaped papillæ are found in the olfactory membrane.

Papillæ on the inner surface of the retina are very fully described and well delineated in the work of Treviranus. He considers the tubules as terminating in them. He has observed papillæ in the eyes of all animals. Weber, in a letter to Treviranus, mentions his having seen similar appearances in the human eye twelve hours after death. We have seen the appearance on the inner surface of the retina of the sheep's eye, which Treviranus delineates and describes as papillæ. (See fig. 6.) We have also seen, in the eye of the newt, the structure of the retina exactly as Treviranus delineates it in the frog, in figures 1 and 2 of plate v. Heft iii., and copied in figures 7 and 8 of our plate. Of all animals, he describes the papillæ of the retina of the pike as being the largest. For the purpose of examining the papillæ of the retina, it is necessary to cut off, in a perfectly fresh eye, a small piece of the vitreous humour along with, and still adhering to, the portion of retina to be examined.

The following is a summary of Treviranus's view of the structure of the retina in most mammifera and birds:

> "After the optic nerve has penetrated through the sclerotica and choroidea, its cylinders spread themselves out, either singly or in bundles, on the outer surface of the retina in all directions. Each individual cylinder, or each bundle consisting of several cylinders, at a certain part of its course bends in towards the inner surface of the retina. Immediately after this, it goes through openings in a vascular network which springs from the central vein of the optic nerve. Before it arrives at the inner surface of the retina, it penetrates through a second vascular network, formed by the last twigs of the central artery of the optic nerve. After the passage through the latter, it is received by a sheath-like continuation of the vascular layer of the retina, and, covered by this, it terminates behind the vitreous body in the form of a papilla." (Heft ii. p. 52.)

Hence Treviranus explains the greater thickness of the papillæ than of the tubules which terminate in them. He says, (p. 55,) "the cylinders of the auditory nerve and of the olfactory nerve in the mammifera end in papillæ similar to those of the retina, though somewhat more filiform." The papillæ of the auditory nerve Treviranus observed on the spiral lamina of the cochlea, and that most distinctly in young mice. We have seen a similar, but not so defined an appearance of papillæ in the spiral lamina of the sheep's cochlea, as Treviranus delineates on the spiral lamina of the cochlea of a young mouse. (Fig. 4, plate vi. Heft iii., and copied in our fig. 9.) The papillæ of the olfactory nerve Treviranus observed over the spongy bones and partition of the nasal cavities.

In an appendix on the structure of the retina, contained in Heft iii., Treviranus confirms the above, and communicates a great many most interesting particulars regarding the structure of the retina in different animals.

In reference to Treviranus's views, Müller says, "The termination of each separate fibre of the fibrous layer in a staff-like body seems still rather a postulate than an ascertained fact." He adds, "If every nervous extremity corresponded to a fibre of the optic nerve, the thickness of the retina ought to diminish progressively from the point of entrance of the optic nerve to the border of the ciliary ligament, independent of the varying thickness of the coats of the retina." The observations of Gottsche, he says, favour this. The retina of the cuttlefish is a gross illustration of it.

Müller, in his "Jahresbericht," collates an account of the retina of the cuttlefish, by Wharton Jones, published some time ago, with Treviranus's description of the structure of the retina; in such a way that one would suppose he was referring to a description of the retina of the cuttlefish by Treviranus also: but there is none such in the "Beiträge" of the latter, nor do we know if he has anywhere else published on the structure of the retina of the cuttlefish. We quote Wharton Jones's account, as it corresponds in a remarkable manner with the above description by Treviranus of the retina of the mammifera and birds; and is valuable, as the structure in the cuttlefish, though microscopical in some degree, is not so minute and delicate as that in the higher animals. Wharton Jones's account of the retina of the cuttlefish is moreover valuable, inasmuch as it has explained away a great anomaly, which was supposed to exist in the eyes of the cephalopodous mollusca: we mean the supposed existence of a thick layer of pigment on the anterior surface of the retina.

The fibrils from the optic ganglion cover, to a considerable extent, the posterior surface of the eyeball, and each penetrates singly the thin cartilaginous lamina which corresponds to a sclerotica. "The optic fibrils," says Wharton Jones, "having thus entered the eyeball, expand into a layer of a light reddish-brown tinge, which I shall distinguish by the name of the first layer of the retina. What I call the second layer of the retina is the reddish-brown membrane which I have already mentioned is the part usually considered as pigment. It is situated within the first layer; and between the two there intervenes a pretty thick and dark layer of pigment, through apertures in which the nervous substance passes from the first layer of the retina to form the second. Examined with the microscope, the second layer of the retina, which, as I have said, is of a reddish-brown colour, is observed to be composed of short fibres perpendicular to its surfaces. These fibres, towards the inner surface, end in a delicate pulpy nervous substance, also tinged of a reddish-brown colour, particularly on its inner surface, which has a corrugated or papillary appearance."*

The papillæ of the retina of the cuttlefish rise above the thin deposit of pigment which tinges the inner surface; a circumstance which resembles a remarkable peculiarity in regard to the retina of the *Coluber natrix*, described by Treviranus, (Heft iii. pp. 95-96,)—viz. that, on the surface of the retina, turned towards the vitreous humour, there is a black pigment, which "lies between the nervous and vascular layers, admits of being peeled off without injury to the nervous layer, and presents at intervals round transparent places, where the papillæ of the medullary substance, likewise transparent, project."

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* London and Edinburgh Philosophical Magazine, vol. viii. 1836, p. 2.

Volkmann,[*] Langenbeck,[†] and Gottsche,[‡] have all given descriptions of the retina, agreeing more or less with what has been cited above. It thus appears that the component elements of the retina are tubes or fibres, the continuation of the primitive tubes or fibres of the optic nerve, and globules. The component elements of the nervous expansions in the labyrinth are essentially the same.

We shall now conclude our subject by giving the following account of the intimate structure of the retina in the higher animals, by Valentin, (*Repertorium*, vol. ii. p. 252;) which is the most recent, as it is the most definite and exact, of any that has appeared.

The retina consists, according to Valentin, of three layers, which, proceeding from without inwards, are as follows:

1. The expansion of the primitive fibres.
2. The pavement-like expansion of the globules of the pure overlaying mass, (*Belegungsmasse.*)
3. The layer of proper granules.

The primitive fibres of the optic nerve radiate all over the retina, but with different degrees of distinctness to the naked eye in different animals. The mode of expansion is exactly the same as is the case in regard to the nerves of other parts of the body: that is, the individual nervous stems, or bundles of primitive fibres, do not run simply alongside each other, but interchange their primitive fibres, and thus produce a plexus. The meshes of this plexus have the common character, that they are elongated and pointed at the two ends.

The primitive fibres of the nervous expansion of the retina are remarkable for their thinness. "That they also end in the usual loops is," says Valentin, "considering all their characters, scarcely to be doubted, although their great delicacy renders it impossible to demonstrate this." We mentioned above, in regard to the termination of the auditory nerves, that it resembled more that of the optic nerves than of any other; and we adduced the observations of Wharton Jones in opposition to Valentin's and Breschet's assertions that it has a loop-like termination. We see that Valentin does not venture to assert a loop-like termination of the primitive fibres of the optic nerve in the retina as a thing capable of demonstration: our own observations induce us to consider the primitive fibres of the auditory nerve in the same case.

To proceed with Valentin's description. The middle layer, magnified 240 diameters, and examined with a somewhat shaded light, is seen to consist of whitish, round, granulary globules, arranged by each other in a plane surface. If one of the globules be isolated, and examined under a stronger magnifying power, it is found to consist of an external transparent coat, granulary contents, a transparent vesicular nucleus, and of a simple kernel enclosed in this. These globules lie under the primitive fibres, and fill up the meshes formed by the latter. (See fig. 11.)

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[*] Neue Beiträge zur Physiologie des Gesichtssinns.—Leipzig, 1836.

[†] De Retina Observationes anatomico-pathologicæ.—Göttingen, 1836.

[‡] Ueber die Retina; in Pfaff's Mittheilungen aus dem Gebiete der Medecin.—Altona, 1836. Heft iii. and vi.

The third layer is the part which has hitherto been always described as the retina: we should mention, it is the layer of papillæ described by Treviranus. It consists of granules, which appear round when slightly magnified, but, under a power of 300 diameters, they appear angular. They are coloured yellow, and contain a denser nucleus-like part in the centre. They lie close together, but are not immediately joined to each other, and are only loosely connected to the middle layer. They form, at the least, looped continuations of the primitive fibres of the optic nerve. (See fig. 10.)

Valentin says, (p. 256,) "Abstracting the peculiar layer of blood-vessels and cellular tissue from the innermost granulary layer of the retina, we have the most complete analogy of conformation with the auditory and olfactory nerves. These also form, by their stems and twigs, plexuses, the meshes of which are of a rhomboidal form, pointed at the two opposite ends, at first more spindle-shaped, and contain between themselves overlaying globules (*Belegungskugel,*) in great numbers. The optic nerve consists of very thin and fine primitive fibres; so also the retina. The retina possesses in the innermost layer a peculiar structure.* (Fig. 10.) This granular layer is wanting at the entrance of the optic nerve, which is a circumstance worthy of notice; for, in consequence of it, this point, being still like pure optic nerve, (only with numerous overlaying globules,) is merely a light-conductor, not an organ impressible by light.

We have thus endeavoured to lay before our readers as clear and complete an exposition as possible of our present knowledge of the intimate structure of the brain and nerves. It will be seen that whatever has any pretensions to exactness has been all accumulated within these five years; and for it we are almost wholly indebted to the labours of German anatomists. Ehrenberg struck out the path, and many have been found to beat it down.

It is commonly considered that the more accurate and complete our knowledge of anatomy is, the more accurate and complete will be our pathology and physiology. This is quite true as a general axiom; but, when we descend into particulars, we find that it does not hold in many instances; for there are parts the anatomy of which we know well, but are still only partially acquainted with their pathology and physiology. Again, there are parts the anatomy of which we know pretty well, and the physiology also pretty well, and yet our knowledge of the latter has been gained in some degree independent of our knowledge of the former. Such is the case with the nervous system. Notwithstanding the great strides recently made in our knowledge of the intimate structure of the brain and nerves, that knowledge has added little to the physiology. The latter, in fact, has been founded on other than mere anatomical data. With the following remarks on this subject by Müller, as being very apposite, we will now conclude; only remarking, that the independency of muscular irritability of nervous influence, which has been repeatedly advocated in this Review, for reasons drawn from the result of experiment, is also favoured, as far as may be, by the mode of termination of the nerves in the muscles.

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* The papillary structure of Treviranus.

"That observers," says Müller, "have not inferred too much from the microscopical relations of the nervous fibres, for the purpose of illustrating the physiology of the nervous system, is a circumstance, in my opinion, to be mentioned only with approbation. Our opinion is further, that the results of microscopical observations will have more influence on the future than on the present state of the physiology of the nerves. Microscopical investigation cannot be carried too far in the domain of micrology. . . . This is the point which we keep in view in our own investigations of microscopical objects; and it is with pleasure we remark that most naturalists are of the same mind. The additions to the physiology of the nerves have, for the most part, been made independently of the more recent progress of microscopical research. The only fact by which microscopical anatomy appears to illustrate the physiology of the nerves is that of the isolated course of the primitive fibres in the nervous stems, branches, and plexuses. Even although it should be established as a peculiarity of the nerves of sensation, that the primitive fibres terminate in loops, still this fact is, as we have already mentioned, without any influence on the theory of the phenomena, as the nerves continue to manifest the same phenomena of sensation, even after their ends have been cut away, and consequently the terminating loops removed. Again, suppose every two nervous fibres form a loop with each other at the surface of the brain, still this circumstance, if the continuation of the nervous fibres should really be discussed, is without influence on the explanation of the known phenomena in the muscular nerves: as a cut muscular nerve continues, when irritated, to manifest the same action on the muscle as when the communication with the brain was entire. The physiology of the nerves must, therefore, be developed in an independent manner. Even the isolated course of the nervous fibres might have been, at least, inferred from the phenomena already known to be manifested by the nerves. As to the peculiarities of the different roots of the nerves, we should never have been led to a knowledge of them by microscopical observation. The facts of reflexion, sympathetic sensation, and motion, have been explained by the phenomena themselves, without the necessity of supposing that, in the brain and spinal marrow, there must be a continuation of fibres. The principle for the advancement of the physiology of the nerves thus remains the same,—viz. experiment on the living nerves. And it is only in this way that we shall distinguish the various peculiarities as regards quality in the uniform disposition of the nervous system; particularly as the facts obtained are, in part, more to be depended on than the results of microscopical investigation have hitherto been."—*Müller's* "*Jahresbericht*" *for* 1836, p. xxi. *In Archiv. &c. for* 1837. Heft iii.

## *References to the Figures.*

Fig. 1. Isolated primitive fibres or tubules from different parts of the brain; exhibiting the various transitions from *a*, *a*, the almost perfectly cylindrical, to *b*, *b*, *b*, the decidedly varicose form. *c*, *c*. Tubules crooked by the same cause which produces the varicose appearance.

Fig. 2. Primitive fibres of the nerves. At one extremity the nervous medulla is seen oozing out.

Fig. 3. A coarse diagram, illustrating Valentin's account of the structure of ganglions. Deposits of pigment are seen on one side of the ganglionic globules.

Fig. 4. (From Burdach.) This exhibits the fundamental form of the terminating loops of the nerves in the muscles: *a*, a bundle from the terminating plexus; *b*, *b*, *b*, fibres which escape the eye without exhibiting terminating loops.

Fig. 5, (also from Burdach,) represents a piece of the skin from the back of a frog, in which four cutaneous nervous stems, A, B, C, and D, spread out in a net-like manner. *a*, *b*, *c*, is a dichotomous nervous bundle, which is chosen for the illustration of the different and indeterminable directions in the course of the nervous fibres; *d* and *e*, two bundles intercrossing, and partly exchanging their fibres; *f*, a bundle which rises perpendicularly on another, and gives off its fibres to the latter in divergent directions; *g*, a square formed by two such perpendicularly rising bundles; *h*, an oblique-angled quadrangle; *i*, *i*, triangles formed by the meeting of a split bundle with another running horizontally, the fibres of the former running on the latter in different directions; *k*, a square formed by the meeting together of two bundles, split at a right angle; *l* and *m*, rhombus and parallelogram, formed in a similar manner, only the bundles have split at an acute angle; *n* and *o*, a pentagon and a heptagon, produced by the regular meeting together of several bundles, dividing at an obtuse angle; *p*, a dichotomous bundle, with a rounded angle at the place where it divides: at the angle may be seen the transition of the fibres from one branch into the other; *q*, a small stem, formed by the meeting together of several bundles in the middle of the nervous plexus, and soon dividing again into several twigs; *x*, several cutaneous glands; *y*, trunk of a blood-vessel, with deposits of pigment in its walls; *z*, several deposits of pigment found free in the skin.

Fig. 6. (From Treviranus.) The papillæ of the retina of the eye of the sheep, seen from the inner surface, or that turned towards the vitreous body. Magnified 300 times.

Fig. 7. (From Treviranus.) Piece of the retina of a frog, seen from the outer surface. *m*, *m*. Dark depressed stripe, from which the very broad and rigid cylinders running on this side proceed. Magnified 300 times.

Fig. 8. (From Treviranus) Papillæ of the retina of the frog, seen from the side turned towards the vitreous body. The dark spots in the centre of these papillæ appear to be the places at which they are connected with the vascular layer covering the inner surface of the retina. The four undermost rows of papillæ are seen sideways. Magnified 300 times.

Fig. 9. (From Treviranus.) Papillæ of the auditory nerve, on a section of the spiral lamina of the cochlea of a young mouse. *a*, *a*. Osseous part of the lamina, which is quite covered with conical papillæ lying close to each other. *b*, *b*. Membraneous border of the lamina, on which there are small papillæ of a hemispherical shape, and placed in rows.

Fig. 10. (From Valentin.) Corpuscles of the peculiar granulary layer of the human retina.

Fig. 11. (From Valentin.) The overlaying globules (Belegungskugel) of the human retina.

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## ART. IX.

1. *Practical Remarks on the Diseases of the Skin, on the external Signs of Disorder, and on the Constitutional Peculiarities during Infancy and Childhood.* By WALTER C. DENDY, Surgeon to the Royal Infirmary for Children, &c.—*London*, 1837. 8vo. pp. 153.
2. *A short Treatise on the external Characters, Nature, and Treatment of the different Forms of Porrigo, or Scalled Head, and Ringworm.* By WALTER DICK, M.D.—*Glasgow*, 1838. 8vo. pp. 58.

BOTH these works profess to be rather practical than scientific; and, considering the great and often superfluous learning which we have to encounter in the volumes that come before us, we are not sorry to meet with books containing nothing which has not a direct relation to therapeutics; a branch of medicine which, strange to say, certain medical writers think beneath their notice. The public, however, take a more utilitarian view in this case; and we believe that few medical men have ever obtained a really beneficial reputation by writings which did not comprise valuable therapeutics, or discoveries from which improvement in the treatment of diseases could not be very directly derived.

I. Mr. Dendy has drawn from his opportunities as surgeon to the Royal Infirmary for Children valuable materials for various disquisitions on the diseases of infancy; a subject, of which we all feel the difficulty and importance. The first essay in the present volume, on the Constitution of Infants, contains, as might be supposed, much that is familiar to the experienced practitioner: it also comprises some remarks and suggestions, which, if not quite original, are at least less generally known, and are well worthy of being recorded. The following observations are of this kind:

> "When the physiologist begins to explain the phenomena of organic life, he selects the animal in which the organization is the most simple; so the constitutions of young children are the most favorable for the study of disease, because they are usually marked by greater simplicity, unmodified by alarm regarding the result of their disease, or by sexual influence, or by mental emotion or disquietude, or by the physical changes resulting from the wear and tear of body. They are, above all, uninfluenced by the alterations of structure produced by repeated and varied disease; for acute attacks, if not checked very early, soon disorganize and change the normal condition of some important organs, forming thus what is termed 'a weak or delicate point;' becoming often, in after life, the focus or seat of disease." (p. 3.)

Notwithstanding this greater simplicity in the form of diseases in infants, compared with adults, our want of that portion of symptoms (and we all know how considerable a portion it is,) derived from the patient's description of his own feelings, renders them difficult of investigation. Mr. Dendy appears to us to estimate far too low the verbal information derived from patients, when he speaks of it as "expressions, which, from the imperfection of language, and the adoption of figurative terms, are not always so decisive and illustrative as we think." Admitting this, we still think that, by a little cross-examination, the physician need never have any difficulty in reducing the patient's language to its just value: for a vague term, such as "*bilious*," for instance, he may obtain a precise description of the patient's feelings, and be enabled to correct such errors as are conveyed by the word "weakness," when employed to represent obscure local pain, or to describe febrile oppression.

To obviate the difficulty arising from the want of articulate speech, the author endeavours to make us correct interpreters of the natural signs of disease—the instinctive language of complaint during pain, the expression of the features, the attitude and action of the limbs. The varieties of these natural signs are drawn concisely, but with graphic power; and we earnestly recommend a perusal of the sketch. The following extract is part of the description of the language of complaint during pain:

> "*Screaming* is a violent effort, indicating vigour, and is usually heard in the early or acute period of disease. The face is flushed and the veins turgid, as the effort impedes the return of blood from the head. When it is protracted, becomes shrill and piercing, and the heat of skin is increased, inflammatory action has probably commenced.
>
> "In inflammation of the gum during dentition, &c., the scream will be more or less protracted; but in inflammation of the chest and belly, in which are seated the organs of breathing, the effort of screaming, induced by continual suffering, so much increases pain, that the child controls for some moments its expression; the screaming is by fits and starts. Local symptoms will direct us to the seat of pain. Intolerance of light and tossing of the head, to the brain; quick breathing, or panting, or cough, to the lungs; palpitation, to the heart; constipation or diarrhœa, to the bowels; nausea, to the stomach; and crowing, to the larynx." (p. 15.)

The section on Diseases of the Skin is by much the longest and most important of the work. The author, in a very sensible introduction to his classification of these diseases, illustrates from them the remark of Sydenham that "a disease is no more than a vigorous effort of nature to throw off morbific matter, and thus recover the patient." He disclaims, indeed, the implicit adoption of a humoral pathology; but is of opinion that cutaneous affections are the outlets and safety-valves of serious internal disorders, and that many resist our means of cure from an endeavour of nature to perpetuate relief to the inward disease causing the eruption. On the other hand, he states the fact, that the cure of these affections, when of long standing, often proves detrimental to the constitution. These opinions are confirmed by striking examples from other works, and supplied by the author's own observation. He admits, however, that the establishment of disease of the skin often reacts perniciously on the system, and that its extension to important parts may prove destructive.

Impressed with the truth of the connexion between certain internal diseased conditions and the outward and visible sign existing on the skin, the author has essayed to form what he terms a practical system of classification, founded on the causes of these diseases. He adopts the descriptions of Willan and Bateman; but employing (to use his own expression,) their delineations as an alphabet, and the classification according to causes as a language, he conceives he has thrown into a concise form all the information that science at present offers regarding cutaneous pathology.

The following is the arrangement according to causes:—The first division comprises diseases symptomatic chiefly of disorder of the alimentary canal, marked by increased cutaneous action, often by subacute or chronic inflammation. The diseases comprehended in this division are subdivided into those occurring during dentition or suckling,—viz. strophulus, lichen, prurigo, crusta lactea, impetigo; and those dependent chiefly on gastro-enteric irritation, which are roseola, erythema, eczema, urticaria, erysipelas, phlegmon, herpes, lepra, psoriasis, crinones, verruca follicularis, acne, sycosis, and porrigo. In the second division are placed diseases indicative of debility, marked by languid cutaneous action, often the sequelæ of acute disorder. These, again, are subdivided into affections depending on original debility of the system,—viz. pityriasis, icthyosis, elephantiasis, alopecia, shrivelled skin, ephidrosis; and those arising from derangement of the chylopoïetic function, which we learn are aphthæ, miliaria, ecthyma, rupia, pemphigus, anthracion, anthrax, purpura, nomé, struma, onychia maligna, cloasma. Of diseases consequent on specific infection we have likewise two subdivisions: the first the febrile, comprising rubeola, scarlatina, varicella, variola; the second the non-febrile, comprehending vaccinia, scabies, and syphilides. The fourth division contains diseases consequent on external and common irritation; which are, encausis, vesication from external irritants, pernio, paronychia, pterygion, verruca, clavus, intertrigo, rhagades, condyloma. The fifth division, or Maculæ, comprises nævus vascularis, lentigo, and tinge of argenti nitras; by which is meant the cerulean hue, deepening to purple or purplish black, produced by the *internal* use of this medicine.

It will be obvious that the propriety and value of this arrangement depend entirely on the correctness of the reference of the diseases comprised in the first two divisions to the causes assigned by Mr. Dendy. We are as much averse as possible to the practice, too common among certain writers, of emptying their case-books on the public: not only, however, would we have excused a few cases in the present instance, but we consider them absolutely necessary to justify the altered classification; for without them the profession, in adopting it, would be surrendering their conviction to Mr. Dendy's opinion instead of his arguments, which it would be quite unreasonable in him to expect. We think the juxtaposition of diseases in a nosological system, because of a resemblance in external character, without a correspondence in their essential nature, a matter of very trifling value: where the two conditions exist, as in the pyrexiæ of Cullen, they constitute a natural class. The latter condition, however, is infinitely the more important; and, had the author proved by the evidence of facts that his collocation of cutaneous diseases reposed on this basis, he would have received thanks for rescuing this branch of our art from the confusion and quackery in which, notwithstanding the labours of many eminent men, from Plenck to Rayer, it is still involved. We not only find a lack of credence on this point, but, as we read the book, positive objections to the classification occurred to us. Purpura, for instance, we find under the head of "diseases indicative of debility, marked by languid cutaneous action;" whilst the author, when mentioning the treatment of the disease, says, "where indications of congestion, of local pain, or *high action* especially, prevail, a small quantity of blood may be drawn in the recumbent position." Not only do we agree in this statement, but we may add, that we have as frequently found purpura associated with a sthenic as a debilitated state of the system, and have been compelled to resort not to small but copious bleedings, and every part of the antiphlogistic regimen, to subdue it.

Notwithstanding our distrust of Mr. Dendy's classification, we have been much gratified by the perusal of his remarks on many of the diseases. His practical good sense is throughout conspicuous, and nowhere more than in the following remark:

> "The adoption of one principle in treatment will often relieve us from this dilemma. I mention it here generally, because, as a preliminary step, it is applicable to almost all cutaneous diseases, especially in childhood, whether common or specific, idiopathic or symptomatic: I allude to local depletion. By subduing common inflammation, which is concomitant with so many even contagious diseases, we produce almost a magical effect, by rendering a disease, before stubbornly resisting, directly yielding to local and even constitutional remedy." (p. 24,)

At the end of the work there is a formulary of local and constitutional remedies, which cannot fail to be valuable to the young practitioner, to whom we can conscientiously recommend this unpretending volume, as calculated to remove many obstacles in the difficult path of early practice.

II. Dr. Dick's work is of more limited range than Mr. Dendy's, but is written with the same laudable object, of disembarrassing the confused and illustrating the obscure in one of the most difficult departments of practical medicine. We know that professional reputations have sustained considerable injury in private families, and especially in schools, from erroneous or undecided opinions regarding the troublesome and loathsome disease which forms the subject of Dr. Dick's "Short Essay." We have seen no treatise upon it so well calculated to impart precise ideas to the young practitioner as this, and consequently to give decision and accuracy to any opinion he may express.

The author thinks, and very correctly, that the genus Porrigo should embrace only species displaying what may be termed the important characteristics of this troublesome family,—contagion, difficulty of cure, and alteration of the appearance of the hair. He, therefore, removes those comparatively slight diseases, Porrigo larvalis and P. favosa, from this genus, and places them with the Impetigines; and, consequently, the student of the disease has only to deal with P. lupinosa, P. furfurans, P. scutulata, and P. decalvans. As, moreover, he agrees with Mr. Plumbe in considering P. furfurans and P. scutulata one and the same disease, the number of species is thus still further reduced to three. This is the simplest classification of the disease that we have as yet seen; and, should general observation confirm the accuracy of Dr. Dick's opinion, he will deserve credit as a reformer in this department of dermatology.

A valuable portion of the treatise is the descriptive anatomy of the parts on which the author considers the *real* forms of porrigo to depend, —the hair and the sebaceous follicles of the scalp. This, however, we leave to be read in the work itself. To the former of these structures he refers ringworm, or P. scutulata and P. furfurans; to the latter, P. lupinosa. His reasons for referring the former disease to an affection of the pilous structure are thus stated:

> "That ringworm consists primarily of subacute inflammation of a specific character, affecting the pilous cysts, and structure secreting the hair, we infer from the following circumstances:
>
> "1st. The thin, bran-like incrustations so frequently seen surrounding the roots of the hairs, and consisting evidently of a morbid secretion from the pilous cysts.
>
> "2d. The falling-off of the hairs from affected parts, and the towy, unhealthy appearance of those which remain.
>
> "3d. In pulling out the hairs from affected parts, we have sometimes brought the bulbs along with them; and these never presented their usual or normal appearance.
>
> "The disposition of morbid action to spread from the parts or tissues in which it originates to those adjacent, is well known; and in ringworm, as in other diseases, this extension of morbid action always occurs, to a greater or less extent. After the disease has continued for a considerable time, the whole thickness of the skin or affected parts is undoubtedly affected, or in a morbid state; but the arguments above adduced appear to me sufficient to authorize us to fix upon the pilous cysts and hair-bulbs as the parts primarily affected in ringworm.
>
> "There seems to be something like a reciprocity of action, in health, between the vessels or organs secreting the cuticle and those secreting the hair; as we see the cuticle thin and delicate where the hairs are plentiful and strong, and *vice versâ*. I have been rather inclined to believe that the epidermic scurf of ringworm might partly result from an over-active state of the vessels secreting the cuticle at affected parts, in consequence of the check given by the disease to the secretion of healthy hair. In other and more extensive diseased states of the system, we see one organ performing, as it were vicariously, the action of another. But I must confess that I am not so much attached to the above notion as I was some time ago; and perhaps the phenomena of Porrigo decalvans are rather opposed to it." (p. 42.)

The author coincides in the opinion of Murray, and dissents from that of Duncan, Underwood, and Luxmore, in regarding the sebaceous follicles as the seat of the P. lupinosa. Baudelocque conceived that he had settled the question by adopting the opinion of Bichat, Meckel, and others, and denying the existence of sebaceous follicles in the scalp. Dr. Dick, however, cannot concur in this view, but describes the sebaceous follicles as really existing, placed at the exit of the hairs from the skin, and surrounding the summits of the pilous cysts. He derives a confirmation of his opinion regarding the seat of the disease from a preparation, which he thus describes:

> "The preparation to which we above alluded, as confirmatory of the opinion of the disease being seated primarily in the sebaceous follicles, consists of a small portion of the scalp, inclosing an incipient lupine-like scab, taken from a subject who died of fever, and who had, for a considerable period before death, been affected with P. lupinosa. The scab is seen to be still covered by the cuticle: inferiorly, it appears to be inclosed in a membrane, and a hair enveloped in its double cyst, apparently in a healthy state, is seen passing through its centre. In short, the scab occupies exactly the situation of a sebaceous gland, and indeed appears to be merely a sebaceous gland distended by a concrete secretion. This view of the pathology of the disease appears to be confirmed by the fact that, when a lupine-like scab is removed, it is not succeeded by a similar one, but by one of an irregular form, without the peculiar cupped appearance. This arises, as we conceive, from the sebaceous follicle having been destroyed during the development of the primary scab." (p. 26.)

Dr. Dick's pathology of P. decalvans is not so distinct as his explanation of the symptoms of the other two species. He supposes that the principle which produces it may act upon the organs secreting the hair in such a manner as to produce a temporary atrophied or inactive state of them. But its true nature, he admits, is obscure; for it is often seen to continue a long time and afterwards disappear, either spontaneously or during the application of stimulant liniments, the hair sprouting up strong and healthy over the previously bald patches.

The author gives clear descriptions of the different forms of porrigo, but these we do not consider it necessary to copy, as they do not essentially differ from those found in Bateman and other elementary works. Regarding the causes of the disease, he thinks that it may arise spontaneously in the ill-fed and filthy; but contagion most commonly produces it. His own experience establishes the fact, that all the forms of real porrigo may arise from the same contagious principle; for when from one case of the disease other individuals of a family are infected, it not unfrequently happens that they are so in forms differing from the original one. The variety most frequently observed in this country, is the furfuraceous or true ringworm of the scalp; whilst in France, the P. lupinosa is the most common form.

The therapeutical suggestions of Dr. Dick are judicious. He is very anxious that this disease should be withdrawn from the domain of quackery, a feeling in which we concur with him; but till more decisive principles of treatment are laid down than are to be found in the pages of writers on this subject, those of Dr. Dick included, we fear that our practice must continue to be at least *empirical*. The vexed question of plucking out the hair he endeavours to settle in the negative, opposing to the authority of Plumbe and others the reasoning that the disease is not seated in the hairs themselves, but in the organs secreting them, and that no considerable benefit can accrue from their evulsion till the parts primarily affected are restored to a healthy state. The success of the depilatory ointments of the Frères Mahon he ascribes, not to their destroying the hair, but to their influence on the structure secreting it and on the cutaneous vessels. He recommends that loose hairs should be removed, that shaving the head should be practised where the condition of the scalp will allow it, and otherwise that the hairs should be cut short. The practice of Mr. Plumbe, which though severe we regard as founded on rational principles, smearing the affected parts with strong sulphuric acid, allowing it to remain for a minute or two, and then washing it off with tepid water, is approved of, and proposed to be extended from the form of the disease in which it was first adopted, ringworm, to porrigo lupinosa.

Dr. Dick extols poultices, and says that many cases have been entirely cured by them. This we can readily believe, and we are somewhat surprised that this result or other circumstances did not suggest to him the adoption of the practice so judiciously recommended by Mr. Dendy, that of subduing common inflammation. The quackery of which the author complains has consisted in treating this disease exclusively by irritants, the only question having been which of the tribe, tar, sal ammoniac, mercury, or sulphur, should predominate in the village nostrum. The name not the nature of the disease has been the object of the prescription. Only by more attention than has hitherto been paid to the local pathological condition, and to the constitutional state with which it is not unfrequently associated, a point which Dr. Dick too much neglects, can this disease be transferred from the domain of quackery to that of regular medicine. By leeches and poultices to the affected parts followed by some mild unctuous application, such as olive-oil or zinc ointment, whilst due attention is paid to the state of the digestive organs and the general system, more may sometimes be accomplished in a few days than by the entire class of irritants employed for months. There are, however, states of the disease for which stimulants are required; and the method of Mr. Plumbe we consider the most simple and efficacious representative of the class, and well suited to supersede the *farrago* by which medical men have been bewildered and patients tortured.

In conclusion we must express the opinion that Dr. Dick's "Short Essay" is the work of an industrious and enlightened physician, and a valuable contribution to this department of medicine.

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## ART. X.

1. *Etudes Chimiques sur le Sang Humain.* Par LOUIS-RENÉ LE CANU, de Paris, D.M.—*Paris*, 1837.

*Chemical Researches on the Human Blood.* By L. R. LE CANU, D.M.—*Paris*, 1837. 4to. pp. 128.

2. *Nouvelles Expériences sur le Sang.* Par M. P. DENIS, de Commercy. (*Archives Générales de Médecine, Fevrier*, 1838.)

*New Experiments on the Blood.* By M. P. DENIS, of Commercy. (*Arch. Gén. de Médecine, for February*, 1838.)

THERE are few subjects which have more deservedly engaged the attention of modern chemists than the analysis of the blood in health and disease. If the results hitherto obtained, are of too vague a nature to allow them to be applied to the improvement of the practice of medicine in the way anticipated by the experimenters, this is rather to be ascribed to the extreme difficulties which beset the investigation than to any want of zeal or skilfulness on their parts. Ample materials are before us; but these materials require a master-hand to mould them into a proper form. The differences in the results of different chemists must be reconciled; in making extensive and important generalizations, we must not be content to proceed with mere average ratios; and, above all things, the characters of blood in its most normal or healthy condition must be indisputably settled. M. Le Canu, the author of the thesis placed at the head of this article, whose name is already well known to the chemists and physiologists of this country, appears to be fully aware of the difficulties which remain to be overcome. To make our knowledge practically available, we must determine, in the healthy adult, the number, nature, and proportion of the constituent principles of the blood which enters into, as well as of that which passes out of the lungs. The liquid transmitted through the jugular, femoral, and subclavian veins, in other words, that which comes from the head and extremities, should be attentively compared with the contents of the vena portæ, the hepatic veins, and of the left subclavian vein, after it has received the chyle from the thoracic duct. The arterial must be compared with the venous blood in individuals of the two sexes, of different ages and temperaments. A similar series of experiments should likewise be instituted on the blood of persons labouring under different forms of disease. It is easy to understand that all the conditions here mentioned may give rise to important modifications in the composition of this fluid.

M. Le Canu has endeavoured in his thesis to give us a true picture of the present state of our chemical knowledge on this subject. Before we express an opinion of the manner in which he has executed this laborious task, we consider it necessary to lay before our readers a general outline of his researches; premising that, in doing so, we are furnishing them not only with the most recent, but with the most trustworthy account of the properties of the blood.

The thesis is divided into four parts: in the first part, we have an examination of the nature and mode of distribution of the immediate principles of healthy venous blood; in the second, the determination of the proportion of these principles, under the different conditions of age, sex, temperament, and kind of food; in the third, the arterial is compared with the venous blood; the properties of this liquid as it is found in the capillary system, vena portæ, and placenta, are then examined. Finally, in the fourth, we have a summary of the pathological changes which it undergoes in certain states of disease, more especially in icterus, cholera, chlorosis, and affections of the heart.

Chemists have generally selected blood for analysis from the veins at the bend of the arm; since, as the arterial blood of the hand traverses no secreting organ before arriving at this part, it may be taken as the type of venous blood in its greatest possible state of simplicity. No less than *forty-five* different constituent parts are enumerated as entering into the composition of this liquid; and, to facilitate the labours of those who are disposed to follow out its minute history, copious references are given opposite to each constituent. But, as the author wisely observes, the existence of this large number of bodies in venous blood is far from being satisfactorily established. The only chemist of repute who has met with manganese is M. Denis, to whose experiments we shall presently have occasion to refer; but he himself admits, what all will easily believe, that this metal may have been accidentally introduced during the analysis. M. Le Canu places more confidence on the discovery of copper by M. Sarzeau, (*Journal de Pharmacie*, t. xvi., p. 505;) but, at the same time, he does not seem disposed to admit it as a regular constituent. Notwithstanding the acknowledged ability of M. Sarzeau as an analyst, since no other chemist has found copper in the blood, we are justified in thinking that he may have been deceived: for the discovery of copper even in the small proportion of $\frac{1}{100000}$th part of an *organic* liquid containing it, is not attended with any great difficulty. This experimentalist stated that he found copper in a great number of organic substances, as in the varieties of corn and grain, in flour, bread, cheese, meat, and most kinds of food, a statement, which has been more industriously circulated than closely examined. Our own experiments, both with regard to the blood and some of the other substances mentioned, lead us to declare, either that some unperceived error in spite of the greatest precautions must have crept into them, or that the blood and the other bodies mentioned, do not contain copper. The hydrosulphuret of ammonia, the acetate and benzoate of soda, discovered by Prevost, are properly considered to have resulted from the decomposition of the liquid under analysis, and have no claim, therefore, to be regarded as independent constituents. The hematic acid, which, according to Treviranus, is combined with iron, has been proved by Engelhart to be the hydro-sulphocyanic acid: and that this was plainly a result of decomposition, is proved by the fact that it was only discoverable after the carbon of blood had been heated to redness with soda. The osmazome of Denis, the *extractive* matters of Berzelius, and the muco-extractive matter of Marcet, are undefined and complex products: besides, these different names appear to be applied to substances essentially the same. The older chemists thought that gelatin was a constituent of the blood; but the researches of Berzelius, Brande, Bostock, and Marcet, have proved that this is an error.

> "The combination of albumen and soda, usually known under the name of serosity, was formerly mistaken for gelatin: but there is this difference between them. A solution of gelatin, exposed to a voltaic current, undergoes no change; while the gelatinous-looking serosity is instantly separated into albumen and soda. Besides it is now generally acknowledged that gelatin is not a distinct animal principle; but a mere product of the reaction of boiling water on skin, cartilage, and other substances." (*Le Canu*, p. 14.)

Some dispute has existed as to the presence of urea in the blood. The real state of the question seems to be this. MM. Dumas and Prevost, in the first instance, demonstrated that urea existed in the blood of those animals whose kidneys had been extirpated; but neither they, nor Tiedemann and Gmelin subsequently, could detect the smallest trace of urea in the blood of animals placed under ordinary circumstances; although, according to the latter chemists, urea may be detected in the proportion of $\frac{1}{250}$th even in operating on a very small quantity of blood. M. Le Canu observes:

> "I have not succeeded in discovering urea in one thousand grains of human serum; but I have observed that an aqueous solution of an alcoholic extract of this serum, first dried in a vapour-bath, and then digested in ether, gave out during the evaporation a strong urinous odour; still the solution evaporated to the consistency of a

syrup, cooled and mixed with an equal bulk of strong nitric acid, did not furnish a trace of the nitrate of urea." (p. 15.)

This we think is conclusive of the absence of urea from *healthy* blood: for it is impossible to rely upon an odour resembling that of urine as evidence of the presence of urea. This principle is well known to be inodorous.

Passing over a few other unimportant substances, the existence of which as constituents is doubtful, M. Le Canu arrives at the conclusion that there are at least twenty-six bodies present in the blood. These are:

> "Oxygen, nitrogen, carbonic acid, iron, the muriates of soda, potash and ammonia, the sulphate of potash, the *sub*-carbonates of soda, lime and magnesia, the phosphates of soda, lime, and magnesia, the lactate of soda, fixed fatty acids combined with soda, a salt formed by a volatile fatty acid, fatty phosphorescent matter resembling the cerebral substance (cerebrine), cholesterine, seraline, fibrin, albumen, yellow, and red colouring matters, extractive matters, water." (p. 16.)

Under the very indefinite term of "*extractive* matters," are comprised: the osmazome and cruorine of M. Denis, the extractive matter of Berzelius and Marcet, the serosity or compound of albumen and soda of Brande and Le Canu. It is very properly remarked, that until the true nature of these substances has been determined, no mischief, but much good will arise, from considering and classifying these nominally different substances under one head.

Probably the long list of substances above given might be advantageously reduced. Thus, with regard to the salts, we doubt whether any with an ammoniacal base can be truly said to be a constituent of healthy blood. Is it not much more probable that this alkali should have resulted either from spontaneous decomposition before analysis, or have been actually produced in the processes required to determine its presence? Again, how can it be said that the sub-carbonates (carbonates) of lime and magnesia, exist free in the blood? If we admit that the serum owes its alkalinity to the presence of carbonate of soda, we cannot extend this admission to the insoluble carbonates of lime and magnesia; and we think it a point far from being satisfactorily settled, as to how far the different salts of the blood have really an independent existence in it. Where, as in general, the process by incineration is followed for their detection, the fact that sulphur, phosphorus, and carbon exist in the organic principles of this liquid, must tend to render it probable that the acids of these salts may be, if not entirely, at least in part, accidentally produced.

Our author does not propose to examine the chemical properties of the different bodies which he enumerates as constituents: for an account of these, he refers to the different and well known writings of other chemists. Nevertheless, the state of the iron in blood, and the singular but little understood connexion existing between it and the red colouring matter are points which are sufficiently important and interesting to call for a few remarks.

The presence of iron in blood is now placed beyond all dispute; but its proportion has been considerably exaggerated. Thus, Menghini, who discovered it, supposed that at some future day, nails, swords, and all kinds of instruments might be manufactured from it. "Non desperaverim ex humano sanguine et clavos et enses, et instrumenta omni genere cudi posse." One of our own countrymen, Dr. Good, asserted, that there was enough iron in the blood of forty men to make a good ploughshare: (*Book of Nature*, vol. i. p. 361;) and Deyeux and Parmentier broached the idea that medals might be struck to the memory of celebrated men from the iron obtained from their blood! We shall see, however, that the quantity is in by far too small a proportion to realize these ingenious speculations. One fact seems now to be well established, that that portion of blood which is commonly called the clot alone yields this metal. Brande asserted that he obtained an equal portion of iron from serum: but Berzelius and Engelhart subsequently proved that there must have been some error in his experiments. Berzelius, however, admits that there are faint traces of iron in the ash obtained by the incineration of fibrin and albumen. But is the iron inherent in and intimately combined with the colouring matter, or is it merely loosely mixed with it in the state of oxide, phosphate, or in the form of an organic compound? On this point, some of the best authorities are at issue. Berzelius, Engelhart, and Le Canu, separate the colouring matter from blood by different processes; but each has found in his experiments that the iron came exclusively from the colouring matter, and they consider that it is in an intimate state of combination with it. On the other hand, Brande, Vauquelin, and Sanson, by processes peculiar to themselves, assert that they succeeded in separating from blood colouring matter free from iron; they therefore contend that the iron is not combined with it. It will not be difficult to reconcile these discrepancies. Berzelius has shown that the colouring principle, as obtained by Brande, does yield peroxide of iron on incineration; and indeed this chemist himself admits, that he always found *traces* of that metal in the incinerated residue, although, at the same time, he denies that it is essentially united to the colouring matter. The inferences of Vauquelin and Sanson are set aside by the fact, that they did not operate on the calcined residue. In taking this precaution, Berzelius and Le Canu both detected iron in a proportion equal to that obtained from their own experiments; and by pursuing the process of Sanson for extracting the colouring matter, our author invariably succeeded in demonstrating in it the presence of the metal. We can then come to no other conclusion, than that the iron and colouring matter of blood are intimately combined; and cannot, by any processes short of the utter destruction of the organic matter, be separated from each other. The author then details six experiments in which he extracted the colouring principle from blood by six widely different processes; and yet in each case there was no difficulty in determining the presence of iron in it. We have only room for two of these, which we quote, as they appear to us to show in the most satisfactory manner the intimacy of the union of these two bodies.

"*Experiment* 1. Warm alcohol was digested on dried blood. The alcohol became strongly coloured; and, on cooling, an abundant red deposit took place. This deposit washed, dried, and calcined, yielded an ash very rich in peroxide of iron.

"*Experiment* 3. The blood of the ox was boiled with alcohol containing a few drops of sulphuric acid. I obtained, 1st, an abundant colourless residue, the ashes of which were entirely free from iron; 2d, brown-coloured solutions, which an excess of ammonia changed to a red colour without precipitating a particle of oxide. In these solutions none of the reagents for iron indicated the slightest trace of that metal;

while by evaporating them and incinerating the residue, it was clear that they contained it in large quantity." (p. 23.)

But there is great reason to doubt, as the author justly remarks, whether by any of the processes usually resorted to the colouring matter of blood is obtained in a pure state. Thus, according to the method of Berzelius, namely, that of drying slices of the clot on bibulous paper, and the processes proposed by Brande and Vauquelin, the colouring matter must be always united to more or less fibrin and albumen. The plan adopted by Engelhart was to dilute the concentrated solution of colouring matter with so much water, that on heating it to a certain temperature this would alone separate in coagula; while the albumen from the large quantity of water present would, it was supposed, remain uncoagulated by heat. But Berzelius proved that a portion of albumen always separated in a coagulated form with colouring matter; and if any albumen were to be detected after the process in the residuary liquid, this was to be ascribed to the dissolving power of the alkali set free during the experiment. This fact is certain: we can never by the mere washing of a clot in water obtain colouring matter without albumen; therefore experiments, performed, as they usually are, upon such a compound, are not to be relied on as setting forth the properties of the pure colouring principle of the blood. The presence of albumen in the colouring matter, obtained by the different processes above mentioned, was easily demonstrated by Le Canu, in simply washing them with hot alcohol acidulated with sulphuric acid. (p. 26.) Even the principle which he had previously described as *globulin* was thus proved to be a mixture of colouring matter and albumen, but the latter was in less proportion than usual. The colouring principle which was procured by Sanson, although free from albumen, he regards as an altered product, from the means used to procure it. From these facts, it is easy to understand why the properties of this important constituent of blood have been so differently described by different observers. Indeed, in comparing the accounts of some chemists, it would hardly be supposed that they were describing a substance which, like other independent animal principles, should possess certain distinctive and uniform characters.

M. Le Canu proposes to substitute the name of *hæmatosine*, which was first given by Chevreul to the colouring matter of blood, for *globulin*, subsequently suggested by himself. That is the term which is now, indeed, generally applied by chemists to designate this principle; but we must here premise that the hæmatosine of Le Canu differs widely in its properties from the hæmatosine of other experimentalists. We must refer to the work itself for a full description of the process for obtaining pure colouring matter. The principle of separation is simply this: The blood, deprived of its fibrin, is treated with sulphuric acid until the whole sets into a brown mass. To this alcohol is added, and the mass compressed in linen, by which the water of the blood is removed. The brown residue is now digested by divided portions in boiling alcohol, slightly acidulated, if necessary, until the last portions of alcohol are no longer coloured. By this we obtain a dense white residue,—a compound of sulphuric acid and albumen,—and several alcoholic solutions of a reddish brown colour, holding, among other matters, the red colouring principle. The alcoholic solutions are supersaturated with ammonia, filtered and evaporated to dryness. The residue is a mixture of colouring matter with saline and fatty substances; these latter are separated by repeated digestions in water, alcohol, and ether. The chief properties of hæmatosine thus obtained are the following:

> "It is solid, inodorous, insipid, of a dull brown or a reddish black colour, with almost a metallic lustre. It is insoluble at any temperature in water, alcohol, ether, and oil of turpentine, unless these menstrua be combined with a few drops of ammonia, potash, or soda, when it forms a solution of a blood-red colour. By long contact with alkalies, or by boiling in water, it becomes changed in colour and properties; for it is now no longer dissolved by ammoniacal alcohol. Chlorine passed through water holding it mechanically suspended entirely destroys it. White flocculi subside insoluble in water, but soluble in alcohol, while the supernatant liquid will be found by the application of reagents to have become impregnated with iron. The sulphuric, nitric, and muriatic acids act powerfully on it, and entirely alter its properties. When a portion is deflagrated with nitrate of potash, neither sulphuric nor phosphoric acid can be detected in a solution of the residuary salt; facts which prove that hæmatosine contains neither sulphur nor phosphorus. When incinerated, it yields a reddish-coloured ash destitute of alkaline reaction, forming a yellow solution without residue in muriatic acid, on which the ferrocyanate of potash, the hydrosulphuret of ammonia, and the infusion of galls, act as upon a solution of permuriate of iron. The muriatic solution evaporated to dryness yields a solid hyacinth red substance, not a white residue as it would do if it contained phosphate of iron. These experiments show that the ashes of colouring matter consist exclusively of peroxide of iron." (p. 30, *et seq.*)

The author found that one hundred parts of hæmatosine obtained from the blood of two females, the one aged twenty-eight and the other eighty-three years, as well as of two male subjects aged twenty-nine, yielded in each case ten parts of peroxide of iron, which are equivalent ($\frac{28}{40}$) to seven parts of metallic iron. The hæmatosine procured from the blood of mammalia, birds, reptiles, and fish, was found to possess the same physical and chemical properties as that derived from the blood of man. M. Le Canu thence takes occasion to animadvert, and in our opinion very justly, on the absurdity of attempting to distinguish the blood of various species of animals effused on linen and clothes from that of a human being. (p. 38.) Although there was no difference in the general properties of hæmatosine, whatever the animal from which it was taken, yet the proportion of iron yielded on incineration was found to vary. Thus while, as we have seen above, one hundred parts of human hæmatosine yielded ten parts of peroxide of iron, the same quantity of hæmatosine of the ox yielded in one experiment 12.85 parts, and in a second 12.67: while this quantity of hæmatosine from chickens' blood gave only 8.34 parts of peroxide. Thus the quantity of iron, although it seems pretty uniform in regard to individuals of the same species, is subject to variation in individuals of different species, and, *à fortiori*, of different classes.

But in what state is the iron combined with the colouring matter? This is a question which has for some time past exercised the ingenuity of chemists, and the answer to which still remains in a great degree a matter of speculation. Deyeux and Parmentier considered that the metal was in the state of peroxide, suspended or rendered soluble by free alkali. Fourcroy and Vauquelin afterwards imagined that the peroxide was combined with phosphoric acid as a basic phosphate: but Berzelius showed that this was not the case: since the subphosphate of iron is not soluble in serum or any modification of albumen, whether an alkali be added or not; and that the colour of the blood could not depend upon the presence of subphosphate or peroxide of iron, was proved by the fact that artificial mixtures containing either of these bodies possessed only a rusty colour, and by no means the bright red tint of the blood. Le Canu is inclined to adopt the opinion of Berzelius, that the iron is in the state of *pure metal* in the blood forming one of its elements, just as phosphorus united to oxygen, hydrogen, and carbon, forms one of the elements of the fatty matter of the brain. (p. 36.) The grounds for this opinion are: if the iron were in the state of oxide, it ought to be separable from the hæmatosine by the muriatic and other mineral acids, which it is not. It ought also to be susceptible of detection by the common and delicate reagents for iron, whereas we know that in a strong solution of hæmatosine these are without effect. With perhaps only one exception the iron cannot be detected in blood prior to incineration. This exception refers to the experiment of Engelhart of passing chlorine gas through a mixture of hæmatosine, in which case the colouring matter is destroyed while muriate of iron is formed and held in solution. It has been contended that this is an additional proof of the iron being in the state of metal since chlorine has no tendency to unite to the oxide of a metal; but Fromherz answers this statement by supposing that the chlorine, in destroying the organic matter, passes to the state of muriatic acid, sets the oxide of iron free, and then combines with it. Rose made artificial mixtures of oxide of iron with different kinds of organic matter; and proved that in these combinations, the tests for iron wholly failed to detect the presence of the metal. Therefore, he argued the iron in blood might be in the state of oxide combined with organic matter, and not be susceptible of detection by tests, owing to that very combination. There is no doubt of the correctness of Rose's experiments: but Berzelius overthrew the conclusion derived from them, by establishing that acids had the power of readily dissolving out the oxide of iron from all such artificial compounds; whence it followed that the analogy between the state of the iron in them and in hæmatosine, wholly failed. Although it is not capable of demonstration, there seems therefore to be every reason to believe that the iron in blood is united in the form of *pure metal* to the ultimate elements of hæmatosine, just as it is united to carbon, nitrogen, and hydrogen in ferrocyanic acid. In both cases it is withdrawn from the operation of ordinary reagents, while in each it is easily susceptible of detection after incineration.

But in whatever state we may presume the iron to exist, there is no reason to suppose that the colour of the blood is due to that metal either in the form of oxide or salt. It is an essential constituent of hæmatosine, and forms, as we have seen, about seven per cent. of that principle; but the colour of the blood is wholly different from that which any salt of iron is capable of giving, even supposing that it were in sufficient quantity. There can be no doubt that the colour of the blood is due to an organic red colouring principle, similar to that of madder or cochineal, which forms the great bulk of the hæmatosine.

Some chemists have thought that there existed a strong analogy between hæmatosine and albumen, but our author shows that there are differences, of which the following are the most important: "Hæmatosine is soluble in ammoniacal alcohol, albumen is perfectly insoluble in this menstruum. Hæmatosine is insoluble in the acetic, muriatic, and sulphuric acids, while albumen is dissolved by them. Lastly, hæmatosine leaves one-tenth of its weight of an ash consisting of the peroxide of iron, while albumen leaves on incineration no (?) traces of iron." (p. 37.)

After some general remarks on the manner in which the chief constituents of the blood are disposed, as well as on the variety of opinions existing respecting the form, size, and disposition of the globules, the author proceeds to detail his own observations on the fatty principles contained in the serum, chiefly the margaric and oleic *acids*, and on the constitution of the globules.

M. Le Canu adopts the old opinion of Home, Prévost, and Dumas, that fibrin has no independent existence as such in the blood; but that it is an essential constituent of the globules, which he believes to be formed externally of a transparent envelope of fibrin, and internally of a mixture of albumen and hæmatosine. (p. 53.) Thus then, according to this view, the blood is divided into two parts, serum and globules; but the whole of the crassamentum is not formed of globules. This mass locks up within its spongy texture a quantity of serum which must be drained away from it by means of bibulous paper. We then have a residue representing the weight of the globules. The mean proportions in 1000 parts of healthy blood are thus given:

| | Le Canu. | Prévost and Dumas. | M. Denis. |
|---|---|---|---|
| Serum . . | 869.1 | 870.8 | 876.9 |
| Globules . . | 130.9 | 129.2 | 123.1 |
| | 1000. | 1000. | 1000. |

These, it is to be observed, are only average results. In no two experiments will the proportions be probably exactly the same; since the degree to which the crassamentum is drained, and the difficulty of separating from it the whole of the loose serum must make a difference. This will likewise account for the above numbers not corresponding: but the difference there is not very material, although in *individual* instances M. Denis found it very great. Thus it is stated in his paper (p. 179,) that the maximum proportion of globules in 1000 parts of blood reached 173.1; while the minimum proportion was so low as 64.4. We must refer to a table by M. Le Canu (p. 62,) to show that there are great variations in the proportion of the globules, depending in some degree upon the *age* of the subject from whom the blood is taken.

The proportion of hæmatosine in the globules is determined by the process already given. M. Le Canu has not found it to form more than 2 per cent. of the globules; and therefore the colouring matter of blood forms very little more than 2 parts in 1000 of that liquid. This will appear extraordinarily small, especially when we compare it with the statement of M. Denis, (*Archives*, p. 173,) that the hæmatosine forms 18 parts in 1000 of blood; but the discrepancy is removed when we bear in mind that the colouring matter of M. Denis contains albumen, while that of Le Canu is free from this principle.

M. Le Canu analyzed the globules by receiving fresh blood in a saturated solution of sulphate of soda. This prevented coagulation, and kept the globules in his view with their envelopes entire. "After a few hours, the liquid will be found separated into two parts, the upper having a faint reddish tint, while the lower is thick, of a blood-red colour, and by slight agitation rises in the form of small pearly looking globules." (p. 50.) By filtration the supernatant liquid is easily separated; and is then found to consist of sulphate of soda and serum. It has all the characters of a solution of albumen in that salt. "The deposit when quite drained has the consistency of honey. When digested in alcohol, acidulated with sulphuric acid, it surrenders a very small portion of hæmatosine and leaves an abundant white residue, having all the characters of sulphate of albumen." (p. 51.)

This then proves that the globules contain a large quantity of albumen. The next observation seems to bear out strongly the view of the author, that the fibrin is not a substance freely diffused or dissolved in the blood, but really a constituent of the globules. "The deposit treated with a saturated solution of the sulphate of soda is not dissolved, nor is the saline solution in the least coloured by it. But if the mixture be strongly agitated, a blood-red liquid is obtained; and there is a residue formed of a white membranous looking substance." (ib.)

That this substance is not albumen is proved by its entire insolubility in water: that it is really combined with the colouring matter, either as nucleus or envelope, seems to be well established by the fact that they are not separable from each other except through mechanical agitation or trituration; although the colouring matter itself is proved by this experiment to be easily taken up by the saline solution. For this reason the author considers that fibrin must form the envelope of the globules; since if the colouring matter (hæmatosine and albumen) were external, why should not the solution dissolve it? The fibrinous envelope protects it until this is mechanically destroyed; then it escapes and is dissolved. This, it is true, is an assumption, but it is one which appears to us to be well warranted by the results of the experiment.

The effect of water is singular:

> "When water is added to the blood red deposit it is absorbed; and the deposit swells out to a dark red jelly or clot. If the proportion of water be increased, the jelly disappears entirely; and while the liquid assumes and retains a red colour, there slowly separates from it a white membranous matter, which is known to be fibrin by the following characters: It is insoluble in hot and cold water, in alcohol and ether, but little soluble in ammonia and acetic acid, insoluble in water saturated with sulphate of soda, but taken up, on the contrary, by a solution of nitrate of potash." (p. 52.)

We do not consider our author quite correct here: fibrin is remarkable for its solubility in acetic acid; and so far as our observation extends, it is taken up in only very small proportion by a saturated solution of nitrate of potash. He admits that he owes a knowledge of this last property to M. Denis, who, as we shall have occasion to see, is remarkable for the boldness of his assertions.

M. Denis, who holds that there is no difference between albumen and fibrin, contends that fibrin is held dissolved in the blood; and he considers that he has discovered the secret of the solubility of this principle. He asserts that the salts in the serum render it soluble, so long as it circulates through the living vessels. (p. 173.) Thus then in the view of this gentleman, the serum of blood is nothing more than a saline solution of fibrin, and fibrin and albumen are identical. (p. 176.) Insolubility in water, he observes, is the natural character of albumen, and when we find it dissolved this must be ascribed to the presence of saline matter. We shall now be prepared for M. Denis's explanation of the cause of the coagulation of blood when removed from its vessels.

> "The serum while within the vessels is naturally supersaturated with albumen, which it is continually depositing in the round of circulation. For the same reason, it must deposit that principle equally when removed from its vessels. It is in this that essentially consists the phenomenon of coagulation." (*Denis*, p. 180.)

M. Denis is the first chemist whom we have met with who has contended that fibrin was identical with *uncoagulated* albumen. Whether we admit that fibrin is held dissolved in the blood of the living body or not, it is impossible to consider that substance as identical with the albumen of serum, or to suppose for a moment that the small quantity of saline matter united to albumen gives to it its property of solubility in water, and constitutes the difference between them. Many chemists have occupied themselves in endeavouring to establish the identity of fibrin and *coagulated* albumen; but with singular inconsistency they have, at the same time, in other parts of animal chemistry, divided what might be fairly, and is generally, regarded as one substance into many others. We ought to remember that a similarity of properties does not constitute identity, and that these attempts to enlarge or diminish the number of bodies where there is so little necessity, are always attended with considerable mischief. We do not mean to assert that there are any striking chemical differences between *coagulated* albumen and fibrin; but in our view, the fact of albumen existing in two states both in and out of the living body, i. e. as solid and liquid, while fibrin is only known to us as a solid out of the body, as also that acetic acid has a very different solvent action on fibrin and coagulated albumen, are differences far greater than exist among many organic substances which are commonly regarded as distinct. M. Denis proceeds to prove his propositions by experiment. He says that "if the fibrin of blood obtained by washing the clot in linen be digested in a solution of nitrate of potash for twenty-four or forty-eight hours or even longer, according to the proportion of salt present, it will be dissolved. The new product will resemble serum or white of egg. It will be precipitated by bichloride of mercury and alcohol, and will coagulate at a temperature of 170°. If this saline solution of fibrin be diluted by a large quantity of water, the fibrin will gradually be precipitated, and reacquire its orginal properties." (*Denis*, p. 174.)

Being struck with the novelty of this experiment we resolved to repeat it, and employed recently prepared fibrin of blood with a pretty strong solution of nitrate of potash. Although the author says nothing about temperature we employed both a *hot* and *cold* solution, digesting and macerating for four days to be quite certain of the result. The liquids were then filtered, and the greater part of the fibrin employed was unacted on and left on the filter. A solution of chlorine which we find to be the most delicate precipitant of fibrin dissolved in acetic acid, gave a faint opalescence, showing that a very minute portion had been taken up; but neither the bichloride of mercury nor a boiling heat had any effect on the filtered liquid. Alcohol certainly gave a precipitate as it always will in a pretty strong solution of nitre, a fact not noticed by M. Denis, although it could not fail were his statements borne out by experiment to complicate the result. In another trial we allowed the fibrin to digest in a cold solution of nitre after very gently heating it for *fifty days;* but *it was not dissolved.* We therefore think ourselves warranted in rejecting both the experiment of M. Denis, and the hypothesis which he has formed from it. But as nitrate of potash is not contained in the blood, he felt himself obliged to advance a step further than this. We are given to understand that he actually incinerated a portion of blood, then obtained from the carbonaceous residue a solution of the saline matter, concentrated the solution by evaporation until the salts amounted to about from $\frac{7}{1000}$ to $\frac{10}{1000}$ths; and by means of this liquid dissolved about $\frac{15}{1000}$ of fibrin, thus forming an artificial serum. (p. 175.) For reasons above given we put no confidence in this statement.

Thus then, if fibrin be dissolved in blood while circulating in the living vessels, this is certainly not proved by the experiments of M. Denis; far less is it established that the albumen of serum is identical with the solid fibrin obtained by washing the clot. M. Denis's explanation of the cause of coagulation cannot then be admitted; but even allowing to him all that he requires it is impossible to adopt his view of this phenomenon. What physiologist will admit that the influence which leads to the deposit of albumen or fibrin by this liquid in the *living* body, is identical with that which leads to the separation of these two principles in blood after it is drawn from the body? According to this view, life takes no share in the phenomena of separation in the living system; but it is a simple mechanical result of the supersaturation of the blood with albumen (fibrin)!

To return to M. Le Canu. Many of our readers may have observed that the liquid obtained by washing a clot of blood in an excess of water, is in the first instance clear and of a deep blood-red colour; but after a time a whitish-red deposit takes place in it. This cannot be albumen, since that is suspended with the colouring matter; but it must be the fibrin separated during the process from the globules, and which Le Canu believes to form the envelopes. This will explain the effect of water on the red deposit obtained by our author. The globules are destroyed by the process of washing, or even by simple digestion in an excess of water, and the fibrinous envelope being insoluble falls to the bottom of the vessel after a longer or shorter time.

M. Le Canu, it will be perceived, is at issue with other chemists chiefly on the point whether fibrin be held dissolved in the blood or not. His opinion of that principle being an essential constituent as a solid envelope of the globules, certainly appears to us to receive support from his observations and experiments. Thus he contends, the globules unless agitated are permanent in saline solutions but are destroyed by water. Hence the globules are suspended in serum without rupture because it contains saline matter. They disappear on the addition of water to blood because the strength of the saline solution is thereby diminished; the envelopes are ruptured and the colouring matter and albumen taken up by the water. Again, it is impossible to obtain separately serum and globules from blood which has once been violently shaken, and this liquid does not coagulate when it has been well beaten or agitated. In both of these cases the facts are explained by the constitution of the globules being destroyed. One point here occurs to us which seems to require explanation. The author employed in his experiments a *saturated* solution of sulphate of soda; but the serum is far from being a saturated solution of saline matter. Besides the most abundant salts in the blood are, upon his own showing, the muriates of potash and soda; and he states he found that a solution of common salt tended to *dissolve the colouring matter* of the globules much more rapidly than the sulphate of soda. Is there not some discrepancy here? Is the author at least not bound to show that an artificial mixture, resembling serum in the *nature* as well as in the *proportion* of its saline contents, has no more influence upon the globules than ordinary serum? The permanency of the globules in this liquid cannot obviously be referred to its saline contents before some such experiment as this has been successfully performed. We put it to the author, whether it be physiologically or chemically accurate to draw an inference respecting this assumed property in serum, from an experiment made upon a *saturated* solution of a particular salt which does not exist in that liquid?

M. Le Canu, although he believes fibrin to be solid in the blood, acknowledges that a coagulable principle occasionally separates from that liquid in a solid form. The nature of this he does not attempt to explain; but he imagines that, under certain conditions of the economy, albumen may acquire the property of becoming spontaneously coagulable. The formation of a buffy coat appears also to offer a difficulty to his view of the constitution of the globules. He considers that this may be due to a similar cause; to the production of a substance analogous to a false membrane on the surface of the crassamentum, or to an increased formation of fibrin. We do not think, however, that this explanation removes the difficulty; for the section of a clot with a buffy coat shows that the colour is gradually shaded off, from the almost colourless surface to the under part, where it is most intense: a fact which seems to prove that the colouring matter is distinct from the fibrin, and is capable of falling through it while consolidating, if the process take place slowly. Besides, those lengthened masses of pure fibrin often found in the heart and great vessels after death cannot have their origin thus explained.

For the method of analysing the blood, we must refer our readers to the work itself; but we think it proper to subjoin the mean result of the analysis of venous blood by the author, as his views are somewhat peculiar respecting its constitution:

*Analysis of Venous Blood.*

| | |
|---|---|
| Water . . . . | 790.3707 |
| Saline and fatty matters . | 10.9800 |
| Albumen . . . | 67.8040 |
| Globules . . . | 130.8453 |
| | 1000. (p. 59.) |

Our readers will here remark that neither fibrin nor colouring matter is mentioned as a constituent of the blood, while the quantity of albumen is less than we find commonly stated. These facts are explained by referring to the author's views on the composition of the so-called *globules*. These are formed of albumen in very large, and of fibrin and hæmatosine in very small, proportion. Thus the following is his analysis:

| | | |
|---|---|---|
| Fibrin | 2.9480 | |
| Hæmatosine | 2.2700 | |
| Albumen | 125.6373 | |
| | 130.8453. | (p. 60.) |

Without this index, we should be at a loss to reconcile the author's analysis with the results obtained by other chemists. From it we learn that the total quantity of albumen in blood amounts to nearly one-fifth of the whole; that the fibrin forms about $\frac{3}{1000}$ths, while the hæmatosine, or *pure* colouring matter, forms little more than $\frac{2}{1000}$ths of the mass. We have already seen that this yields $\frac{7}{100}$ths of its weight of metallic iron; whence we infer that the proportion of iron in blood is no more than about $\frac{1}{7000}$th. Le Cann makes it $\frac{1}{4405}$, (p. 65;) but, allowing for the decimals, he appears to have calculated for the proportion of oxide of iron, and not of the pure metal. At the same time we fully agree with him that no great importance is to be attached to calculations of this sort; and the less so, since we do not find that there is much agreement in the reports of any two observers. Still, by showing how minute the quantity of metal is, they set at rest two points: they refute the exaggerated statements of the older chemists, and they serve to establish that the colour of the blood cannot depend upon the mere presence of this metal.

In regard to *Sex:* it is found that, while the proportion of albumen is about the same, the blood of the male contains less water and more globules than the blood of the female. We are to understand by the albumen here, and in all future observations, that portion only which enters into the composition of serum, and not that which has been described as forming part of the globules. The chief modifications which the blood undergoes in persons of different temperaments and ages are observed to relate chiefly to the relative proportions of serum and globules; the quantity of albumen being subject to little variation. Thus, the serum is in less and the globules are in greater proportion in individuals of a sanguine than in those of a lymphatic temperament; in adults than in infants or old persons; in those who take much nourishment than in those who take little. Thus the author remarks, "the proportion of globules in the blood would seem to be an index of the vital energy." (p. 69.) So again: "By a singular coincidence, every cause that tends to diminish the mass of blood in the body tends at the same time to diminish the relative proportion of globules, by increasing the quantity of water; so that those causes which diminish the fulness of the vessels at the same time impoverish the blood, and render it more fluid. Uterine hemorrhage in the female, and copious bleeding in either sex, are well known to have this effect." (ib.)

The author next describes the differences between arterial and venous blood, and he arranges in order the observations of different chemists, which are in some instances of the most conflicting character. By sifting the statements of some, and testing the correctness of others by experiment, he comes to the conclusion that the following are the only appreciable differences between the two kinds of blood:

"*Arterial* blood is of a brighter vermilion colour, and possesses a stronger odour, than *venous*. It has a greater tendency to coagulate, furnishes a larger and firmer clot; facts which show that it has a greater proportion of globules and less serum. It contains more fibrin, and the solid matter being in greater proportion, its density is higher than that of venous blood. The albumen, saline and fatty matters, are about equal in the two kinds of blood. Arterial blood is richer in oxygen, in proportion to its carbonic acid, than venous; and, by ultimate analysis, it yields less combined carbon and more combined oxygen." (p. 86.)

The properties of the blood of the *capillary* system are given on the authority of Pallas; of that of the vena portæ, on the authority of Prévost and Dumas; while the characters of placental blood are derived from the observations of Denis. M. Le Canu here presents us with nothing original. The blood of the capillaries was not found to differ sensibly from that of the veins; and, although the blood of the vena portæ seemed to offer a smaller proportion of globules, yet this was only in comparison with the *mean average* of venous blood: the quantity of globules was within the ordinary range of variation of healthy blood. The blood of the placenta Denis found to contain less water and more globules. It is singular that the blood of the fœtus should differ in the same respects from that of the mother; for the proportion of globules in fœtal blood is far greater than in that of the mother. This is precisely the change which the lungs work after birth; and it is a proof, therefore, that the placenta, in the unborn child, really occupies the place of lungs.

We now arrive at the last part of the work, namely, that which refers to the changes which take place in the blood under certain *pathological* states of the system. This is obviously the end to which all researches on the blood ought to be directed; for our knowledge of the chemical properties of this fluid will be valueless, unless made auxiliary to the purposes of medical practice. We wish we could look forward to the realization of the anticipations of the author, as to the good likely to be wrought on medicine by the pursuit of these enquiries; but we fear a very long period must elapse before this end will be attained: nevertheless, we willingly agree with him that, when we compare our knowlege of this important liquid with that which existed but half a century since, we have indeed reason to congratulate ourselves on having made more than one step towards the desired object. The following observations are very just:

"Our knowledge of the pathological changes which take place in the blood must be necessarily imperfect, until we know exactly, and in the minutest details, the composition of normal or healthy blood. At present we are not acquainted with all the immediate principles of the blood in health: for instance, we know nothing of those numerous substances commonly confounded under the name of *extractive matters;* we cannot determine exactly the proportions even of those principles with which we are best acquainted; and, lastly, there is not a single principle in the blood which may not undergo important changes, without these becoming appreciable in analysis." (*Le Canu,* p. 93.)

We should recommend M. Denis to adopt the philosophical caution implied in these sentiments, and not suppose, as he seems to do, that he has left future chemists nothing to occupy themselves with in relation to this subject. For instance, we are told by him "that chemical experiments are everywhere at hand to assist us, and that these must take precedence of all explanations," (p. 180;) while, immediately afterwards, he furnishes us with thirteen conclusions, in as many paragraphs, which, for crude hypothesis and hasty generalization, we have seldom seen surpassed. It is strange that one generation will not profit by the errors of another. We laugh at the false reasoning of the older chemists, but we do not hesitate to plunge into the very system which misled them. It is fortunate, however, that, by a strictly inductive method of research, it is as easy to expose such absurdities as it is to invent them. We shall dismiss this gentleman's "new experiments" on the blood with one quotation, showing the application of his views to therapeutics. It appears that, among other cases, he has endeavoured to cure the croup *on chemical principles*.

> "The croup," he says, "consists physically in a false fibrinous membrane lodged in the trachea. M. Denis conceived that it might be attacked directly by the introduction of substances exerting a solvent action over it, and indirectly by introducing into the blood, through the medium of absorption, other substances possessing a similar power. He asserts that he has *successfully* employed for this purpose salts resembling those of the blood." (*Denis*, p. 184.)

But to return to the more cautious views of M. Le Canu. We do not find that the author has advanced much that is original on the pathology of the blood; but he has carefully collected, arranged, and commented on the statements of other experimentalists respecting its abnormal conditions in numerous diseases. He presents us, indeed, here with a good summary of all that is known on this interesting subject.

The menstrual secretion appears to differ from blood in containing more water, less albumen and globules, and an admixture of variable proportions of mucus.

In inflammatory affections, the quantity of globules increases; or, in other words, the proportion of water is diminished.

Dr. Rollo announced, now many years since, that in diabetes the blood contained sugar; but the researches of numerous able experimentalists have rendered it probable that he was deceived. Vauquelin operated on the blood of a diabetic patient, whose urine contained one-seventh of sugar, without finding a trace. The only well-ascertained fact appears to be, that in this disease there is a diminution of the proportion of globules.

The state of the blood in icterus has given rise to considerable discussion. Some have contended that this liquid always contains bile; others that it does not contain any; and a third class of pathologists have admitted, a *mezzo termine*, that it receives only the colouring principle of that secretion. This last opinion appears to be correct: for, while no one has succeeded in detecting all the elements of the bile in the blood of icteric patients, many have found in it yellow, green, and blue colouring principles, resembling, if not identical with, those present in bile. Le Canu observed that the globules were also in much less proportion in these cases. But we must not consider it positively established that the bile never finds its way into the blood. How are we to discover all the complex principles of the bile, more especially picromel, in a still more complex liquid like the blood? There are assuredly no tests at hand to assist us; and, even if there were, it is more than probable, from an experiment of Thénard's, that they would not be available. This distinguished chemist could not find a trace of bile in the blood of an animal, into the veins of which a large quantity had been purposely injected. It had probably been decomposed or altered in the round of circulation. This preliminary experiment should be tried by all those who undertake the pathological analysis of the blood. There is no doubt that the bile has been pronounced, in many instances, to have been present both in the blood and urine, merely from the circumstance of a yellow colouring principle, capable of being affected by mineral acids, like that of bile, having been found in those liquids: but it is quite certain that the discovery of this colouring matter in either case is no proof of the presence of the whole hepatic secretion.

The author had frequent opportunities of examining the blood in malignant cholera, and his results correspond with those obtained by good observers in most countries where this scourge has prevailed. Leaving out trifling differences, which may have depended on purely accidental causes, the most striking and constant deviation appears to have been that the solid matters were in very considerable proportion. At the time that the cholera was most severe in Paris, the following results were obtained from the analysis of the blood of three persons labouring under it.

| | 1st An. | 2d An. | 3d An. |
|---|---|---|---|
| Solid matters (globules) | 340 | 251 | 520 |
| Water (serum) | 660 | 749 | 480 |
| | 1000. | 1000. | 1000. (p. 106.) |

By this it will be seen that the solid matters were in some instances twice as great as in healthy blood; a condition which did not arise from any increase in the solid, but from a diminution in the liquid contents of the vessels. We agree with the author in thinking that this state of the blood might, perhaps, be made to serve as a criterion of the nature of the malady. The intestinal discharges seem to acquire what the blood loses: these have a powerfully alkaline reaction, and contain albumen with extractive matters, resembling those of the blood.

In typhus fever, the author has found the globules to be below the mean, and even below the minimum proportion in healthy blood. Bonnet observed that hydrosulphuret of ammonia was present in these cases; but this may have depended on the blood having rapidly putrefied after abstraction. This substance is a product of putrefaction; and it is hardly likely that it should be formed within the blood while circulating through the living vessels.

In disease of the heart, the quantity of serum was found to be large, and the clot small. Repeated venesection seemed to have the effect of increasing the proportion of globules.

After the discovery of iron in the blood, it was supposed that the pallor peculiar to chlorosis was due to a diminution in the quantity of that metal: and this seems to have been the foundation of the use of ferruginous preparations in its treatment. Le Canu found that chlorotic blood contained much less than its healthy proportion of globules, and consequently less iron: but the disease cannot be owing to this deficiency, since the same diminution is met with in other and widely different diseases. Other experimentalists have corroborated these results.

In some pathological conditions, as in diabetes, dropsy, peritonitis, nephritis, and hepatitis, the blood has been observed to assume a creamy appearance, just as if milk had been mixed with it. This appearance has been proved by the author and other chemists to be entirely due to the suspension of an excess of animal fat or oil in the serum, and not to casein, as it was formerly supposed. There is at the same time a considerable deficiency, or almost entire absence, of red colouring matter.

Such is an outline of what we believe to be the principal facts connected with the healthy and morbid condition of the blood, so far as the observations of chemists have yet extended. The chief fluctuations in disease relate to the increase or diminution of the globules; and we cannot hesitate to assent to the proposition of M. Le Canu, that many morbid states in the system may be due to insidious changes in the nature and proportion of the constituents of this important liquid. Without adopting the extreme views of humoral pathologists, there is nothing to prevent us supposing that fatal impressions on the system may also operate by altering the blood. Many poisons may in this manner destroy life, although chemistry has not yet succeeded in establishing their presence in this liquid.

We shall conclude our notice of M. Le Canu's thesis by saying that it reflects the highest credit on the author. There is much that is original; and this, together with what he has compiled from numerous sources, forms, in our opinion, the best modern history of the blood.

## ART. XI.

*Notes on the Medical History and Statistics of the British Legion of Spain; comprising the Results of Gun-shot Wounds, in relation to important Questions in Surgery.* By RUTHERFORD ALCOCK, K.T.S. &c.; Deputy Inspector General of Hospitals, with the auxiliary Forces in Portugal and Spain.—*London*, 1838. 8vo. pp. 101.

THE opening words of this volume, in which the author sets forth the abundant censure and scant praise,—the toil, peril, and privation,—the hard blows, and still harder words, received in the service of which the medical results are here given to the public, are calculated to disarm the most ruthless critic. We could not have helped receiving the circumstances under which the materials of the work were collected and arranged as an atonement for many imperfections and errors, had such existed: when the book, however, proves to be absolutely a good one, we must consider the credit thence accruing to the writer to be enhanced by those circumstances, which, had they acted on an ordinary mind, would have rendered the work erroneous, confused, and uninstructive.

Had we a hope that any medical work would prove a lesson to governments, we should pronounce Mr. Alcock's likely to answer this purpose; but we can entertain no such hope. How many expeditions have quitted our shores to return thither overwhelmed with disaster, because the page of some well-known medical writer had not been consulted! In the case of the ill-omened expedition which is the subject of the present work, hundreds of living witnesses were accessible who, on the best evidence, could have assured those who planned it that, if the difficulties of the country, climate, and, above all, the government of the Peninsula, were such that they were scarcely surmounted by a well-organized and highly disciplined army, commanded by the greatest general, and supported by the most powerful government and richest treasury in the world, they must prove destructive to a very inferior army, however ably conducted, which received but a *quasi* recognition from British authorities, and was thrown for its supplies on the tender mercies of those of Spain. It is a pleasure to be able to state that Mr. Alcock's work contains indubitable evidence that the superior officers, civil and military, discharged their duty with honour, talent, and energy, and failed only where the very bravest and the very best must have failed.

These remarks may wear the aspect of being extra-professional: they are, however, not so in reality; but have a direct relation to the medical condition of the Legion, so ably depicted by Mr. Alcock. The English and Scotch soldiers of this army were drawn principally from an enervated, oppidan population; whilst the majority of the Irish had been accustomed to spend their days in the open fields, and live more hardly. The effects of this difference in early training was visible in the numbers which these classes respectively furnished to the hospitals at Vitoria; the English being swept in very rapidly, in the proportion of one-third of their number, the Scotch of one-fifth, the Irish but of one-eighth. So long as this miscellaneous and probably ill-organized body remained stationary at Bilbao, the sickness was moderate, and the cases were slight; being such as are ordinarily found in military hospitals in a healthy season. During the march to Vitoria, which commenced on the 29th of October, and terminated on the 3d of December, the troops remained healthy, though the weather became, before it was completed, very severe, as it generally is, during the winter months, in the mountainous district of Biscay, and on the table-land of Old Castile. Sickness assailed them in quarters at Vitoria. The causes are referred to the following heads:—1. An over-crowded state of the town. 2. An inclement winter. 3. Rations bad in quality, deficient in quantity, irregular in delivery, and imperfectly cooked. 4. The state of the hospitals and convalescent depôt generating and aggravating disease, and rendering impossible the adoption of proper means of treatment. 5. Ultimately, deficiency of medical aid.

A portion, at least, of the evils they suffered might have been obviated or removed by good will, exertion, and honesty on the part of the Spanish agents and contractors; but it need not be remarked to those who know anything of Peninsular warfare that of these qualities there was found a total deficiency; whilst their place was supplied by backwardness, malignancy, and treachery, without stint; and that, consequently, the sufferings of the British were frightful. Over-crowding, deficient and bad food, want of blankets or any covering to shield the troops at night from the inclemency of the season, speedily engendered a pestilence, which, of fifty-one medical men, destroyed eleven, and disabled seventeen; out of 7000 troops, sent 5000 to hospital, and carried off a fifth of the force in five months.

The type of fever which committed these ravages is thus described by Mr. Alcock:

> "On examining the patient, the medical officer would often find a full pulse, flushed face, dry hot skin, violent pain of the head, a crusted or loaded state of the tongue, with total prostration of strength; at other times greatly diminished vitality, purple and livid countenance, or skin shrunk. Delirium very quickly supervened; the tongue and fauces became dry and hardened, the lips loaded with sordes. Dysentery became developed; the rapid decline of the pulse and all the powers of life followed, and from the tenth to the twentieth day the patient died. During this period the feet, partially or entirely, sometimes including the whole of the legs, would run rapidly through all the stages of gangrene. In others, again, dysentery and its train of symptoms presented the leading feature, with the same fever quickly supervening, running a still more rapid course. If, on the contrary, the patient's state improved, one of his first complaints, on recovering from his delirium, was the tenderness and insupportable sense of heat and pain in the feet; for the relief of which neither bland nor stimulating fomentations, nor friction, nor opiates, general or local, availed. The feet would feel cold to the touch, but present no external appearance of disease or inflammation whatever, and were exquisitely tender: at last, either these symptoms would gradually disappear after a menacing inflammation, or finally separate the tips or the whole of the toes; but, in the worst cases, often involving feet and legs before the line of demarcation indicated its cessation." (p. 20.)

The author says that, if asked to give the chief feature of this fever, he would say "the inflammation and gangrene of the lower extremities." He bestows laudable pains in investigating the causes of this curious symptom, which, if we understand him rightly, occurred under two different sets of circumstances; either in the course of such a fever as has been described, or in emaciated and weakly patients suffering from dysentery, in whom, on their first admission into hospital, the toes of one or both feet were found in a state of gangrene, which was ascribed by the patient to exposure to cold, on some particular occasion specified. When it occurred under the former circumstances, the author referred it "to the near approach to annihilation, in the powers of life continuing at the lowest point consistent with existence for many days, and most fatally experienced at the point furthest from the centre." When the gangrene occurred more suddenly, we should think that there could be little difficulty in ascribing it to frost-bite, (as the patients themselves virtually did;) to which ill-fed, ill-clothed, and weakly persons would be peculiarly prone. We happen to know that this was by no means an unusual occurrence in the mountainous regions in the north of Spain, among the soldiers of the Duke of Wellington's army; especially among those on night-guard or picket. We likewise think it probable that the tendency to gangrene in those affected with fever was connected with the low temperature, from which they were so irregularly and ill protected; for, if the *mere* "approach to annihilation, in the powers of life continuing at the lowest point consistent with existence for many days," could produce it, why is it not observed in the protracted collapse of cholera, which has been known to continue for many days? Again, the author quotes a report from Dr. Neale to Sir James M'Grigor, in which it is stated that, in January 1812, when fever was prevailing in all the general hospitals, the cases at Ciudad Rodrigo had almost universally mortification of the lower extremity, with livor and mortification of the nose. Ciudad Rodrigo is in the north of Portugal; but it consists with our own knowledge that the fever which was prevailing at the same time in the hospitals in the south of Portugal, near the Tagus, was unassociated with any such symptom. The bread given to the men of the Legion was said to have been intentionally mixed with deleterious matter. This the author, like a man of sense, rejects as an absurd rumour; but he describes the bread as having been wretchedly manufactured and of unsound flour; and he asks the very reasonable question, "Knowing, as we do, that diseased rye will produce dry gangrene, why may not some deleterious quality of bad corn produce a similar disease—moist gangrene?" It is not, so far as we know, consistent with medical experience that it does so *directly*; but we have little doubt that the defective nutritious quality of this bad bread was one of the means of inducing that impoverished condition of the system which rendered the soldiers peculiarly liable to the action of cold and other morbific causes.

The treatment adopted consisted in unloading the alimentary canal by an emetic and a purgative; blisters to the nape, to relieve pain of the head; mild diluents, saline effervescents, and gentle diaphoretics; cold immersion and affusion during the hot stage; and, in the latter stages, means suited to support, and even stimulate, the vital powers. The wretched state of the supplies, however, rendered this treatment, simple as it was, always difficult, and sometimes impossible.

A summary of the mortality at Vitoria is furnished by the following table:

*A Return of Admissions, Discharges, and Deaths, from January* 1*st to April* 15*th*, 1837.

| | Febris. | Dysenteria | Varia. | Total. | |
|---|---|---|---|---|---|
| Remained, Jan. 1st | 136 | 5 | 48 | 189 | |
| Admitted............... | 329 | 26 | 163 | 528 | |
| Discharged ........... | 350 | 21 | 152 | 543 | |
| Dead.... ............... | 81 | 9 | 21 | 111 | |
| Remained, April 15th | 44 | 1 | 38 | 83 | Proportion of deaths, 1 in $6\frac{41}{111}$. |

To the mortality stated in the table are to be added 187, who died out of the 787 remaining under treatment, and seventeen deaths during the removal to Santander. When are added to these 400 deaths in December, and as many more at Trevino and other cantonments in the neighbourhood of Vitoria, a total of 1223 is formed. The further additions being made of 500 men incapacitated for future service, and of a mortality of nearly 200 at Santander and Bribiesca, a loss of the services of 2000 men to the Legion (the shocking inhumanity being left out of the question) is to be ascribed to the defective arrangements of the Spanish government or its agents, and the hostility or inertness of the provincial authorities.

Mr. Alcock states the total proportion of mortality in the Legion, during twenty-three months of its service in Spain, thus:—During the first four months, it was about one in forty of the number treated; from the end of the fifth to the ninth month, (the period of its residence in Vitoria,) one in five; and from the tenth to the twenty-third, one in $13\frac{88}{270}$. He is naturally desirous of comparing the mortality of the Legion with that of the British Peninsular army, and finds, from Sir J. M'Grigor's Medical Sketches, and the statements of Mr. Guthrie, that, in the case of regiments composed principally of recruits, which may be considered as forming a fair parallel with the Legion during its disastrous stay at Vitoria, the difference in the loss from sickness is by no means considerable.

The second part of the work is surgical, and contains ample evidence of the professional zeal, method, and activity of the writer. We feel confident, in perusing it, that, in the field and the hospital, he is an excellent military surgeon. Justice, however, compels us to withhold our praise from one section of the book, which has probably cost its author much labour,—the numerical estimates of the results from injuries received in action; most of which are arranged in a tabular form. Some might feel disposed to question the applicability of numbers for any useful purpose to matters affected by circumstances so infinite and minute as this class of injuries. This question we do not feel disposed to argue at present at any considerable length. We agree with the author that, to form a correct average, it should be drawn from a very large number of cases; and think that his statements and tables, correctly calculated, might have constituted a valuable contribution of such an average. On examining them, however, with some care, we find them destitute of that accuracy without which they must be valueless, or worse than valueless. Some of the errors may be typographical; but, even in this case, a list of *errata*, which we have sought for in vain, should have corrected them. Most of them, however, evidently do not belong to this class; and we exceedingly regret that a work, the general execution and tendency of which impresses us very favorably, should be disfigured by them.

There are many parts, however, of the same department of the work, the value of which is much less questionable. Two precepts delivered, as it would appear, by Mr. Guthrie, in his oral lectures, and in the truth of which every skilful military surgeon will concur, receive ample confirmation from the experience of Mr. Alcock. The first is, "Never to be alarmed at hemorrhage, when the finger can reach the bleeding point;" the second, "Be sure to cut the bone short." Mr. Alcock's first operation confirmed the value of both these aphorisms. The very last operation which he performed was, we will not venture to say, unexampled in military surgery, but we must think infinitely rare. The decision in this difficult case was very creditable to his judgment. The humerus was shattered, and the arm was removed at the shoulder-joint. The patient was of a sanguineous and nervous temperament, falling into nervous paroxysms. The divided artery retracted so deeply, that it was impossible to reach it without a second operation for taking up the axillary artery. This was not done; and the stump was lightly dressed, that any further operation might be easily resorted to, should the necessity for it occur. None, however, did occur, for there was no secondary hemorrhage; and the stump healed soundly and rapidly.

The question of primary or secondary amputation, or rather as, following M. Boucher in his Memoir to the French Academy, Mr. Alcock would term it, primary, intermediary, and secondary, is discussed at considerable length. This portion of the work displays less of decision and of reliance on surgical statistics than generally characterize the volume; but it manifests what is of more value, a clear perception of the difficulties of the subject, and a candid statement of the sources whence they arise.

> "Unfortunately we are met on the threshold with a great, if not an insurmountable, difficulty. In considering the subject of amputation, we find so many causes influence the result; some, it is true, evident, easy to observe and define; but others difficult to appreciate, although not the less certain in their action: and hence all tables are more or less inconclusive. Thus there are several classes of causes which exercise an important influence over the results of amputation, whether primary or secondary; each branching into a multitude of ramifications, uniting, separating, and again interlacing, bearing upon each other, and finally, by their combination, producing the results upon which alone the reasoning on both questions and the deductions are chiefly drawn." (p. 67.)

After stating his hopelessness of supplying a definitive solution of these questions, but mentioning his intention, in a future work, of furnishing a detailed tabular abstract of between one and two hundred cases; which, though leaving much to be desired, may not prove uninstructive; Mr. Alcock says,

> "It is not enough, in my opinion, to say here are two sets of operations, primary and secondary,—(the greater mortality is in the latter, therefore the former is most advantageous;)—a summary mode of settling the question, which would be most desirable, were it just, and as conclusive as the numerical difference is distinct. There are at least certain classes of causes, the influence of which should be correlative and equal in the two opposing sets of operations, before the numerical comparison of results can be final. These I consider to be: 1. The nature of the original injury. 2. The general constitution depending upon the organization and previous health and habits of the patient, and his particular condition at the period referred to. 3. The mode of operation, (in which the time occupied is included,) and a great variety of circumstances, some easily and others with difficulty appreciated or defined. 4. The after treatment, into which the *locale*, the state of the atmosphere, and many other features, enter. 5. The complexion of the mind, and the consequent mental influence which may be beneficial or deleterious to a high degree.
>
> "Unfortunately, a rare combination of favorable circumstances is required to render the observations and registry of a sufficiently large number of facts, thus accurately and fully defined, possible." (p. 68.)

If the question of primary or secondary operations is to remain undecided till their respective merits are tried on two numerous groups of cases, presenting a perfect equality in all the circumstances specified by Mr. Alcock, we need scarcely remark that the decision is adjourned for ever: but we do not consider the authorities in military surgery as having left the question to be settled by Mr. Alcock's impossible experiment. Beside the recorded opinions of Guthrie and Larrey, (they men of experience in the best sense of the term, having combined ample opportunities of observation with the talent required for profiting by it,) it is familiarly known that the practice of primary amputation was that adopted by the surgeons generally in the British Peninsular army, in all cases in which the condition of the patient rendered it admissible, and there was no reasonable prospect of saving the limb. This is not the place for a full disquisition on the grounds of this preference, but the thought would naturally arise in the mind of any one that the repair of a clean incision, to which healthy integuments are carefully adapted, must be a source of much less suffering, and a less serious tax to the patient's constitution, than the sloughing of contused integuments and muscles, and the extensive suppuration occasioned by the presence of comminuted bone, and it may be of foreign bodies; and that, on the first infliction of such an injury, the patient will generally be found in a state more suitable for bearing amputation than after his health and spirits have been broken by confinement, and his constitution has been irritated and drained by the circumstances above mentioned. This plain reasoning appears tolerably conclusive; but still more reliance is to be placed, in a matter of observation and induction, on the universal decision of a body of men whose talent and zeal, and, above all, whose experience, Mr. Alcock, we feel sure, would never think of questioning. The preliminary questions—"Is there a reasonable prospect of saving the limb?" and "Is the shock to the nervous system of the party such, that to inflict immediately the additional one of an amputation would be dangerous?"—it is obvious, demand, in many cases, on the part of the surgeon, a good deal of accurate and prompt thought: in short, of the quality called *tact*, which intelligence and experience alone give, and which no written rules can as yet render medical men independent of.

Mr. Alcock is not opposed to the primary as compared with the secondary operation; but his expressions on this subject are very remote from the decisive tone which pervades the rest of the book: "he hints a fear, and hesitates dislike." The student of military surgery, anxious for information on this point, would rise from a perusal of his volume embarrassed and confused. Some portion of this undecided tone is apparently ascribable to the nature of the force and service with which he was connected. He describes the soldiers of the Legion as in a state of despondency or despair at the thought of primary amputation, preferring even death to sustaining a loss which would reduce them to mendicancy or the workhouse; whilst the experience of weeks of suffering having convinced them, in the case of the secondary operation, of the hopelessness of saving the limb, they submitted with resignation to a loss which they had become convinced was unavoidable. It is obvious that men, certain of being compensated for the privation of a limb by a pension from their country, would have felt differently. There is a degree of fallacy in all reasonings from the class of men composing the Legion to the more energetic soldiers of the regular British army. The author's tables show that, in the case of the former, there was a trifling advantage in secondary as compared with primary operations: the mortality being in the latter as 1 to 2, in the former as 1 to 2¼ nearly. In explanation of this he makes the following very judicious remarks:

> "As it was only from some accidental circumstance of rare occurrence, after the first action, that cases which, from the nature of the injury, required amputation from the first were deferred, it followed that those who were the subjects of secondary amputation had already proved, by the very fact of their arriving at the second period, that they were among those best capable of bearing the effects of a shock and subsequent disease, and the original injury was generally less severe. It should ever be borne in mind that a cause of fallacy pervades all the returns of secondary amputation given by the advocates of that practice; viz. that the cases are altogether lost sight of which never arrive at the second period, many of which would have been

saved by operation at the first, and which, in my opinion, ought strictly to be added to the losses from that practice; an arrangement which would materially affect their tables of results." (p. 98.)

A proportion of the mortality from primary operations was observed to arise from a peculiar febrile condition, which occurred at a period varying from two to three days to ten or twelve after the amputation, and which the author ascribes to the quick succession of two shocks to the nervous system. At least, he found this condition less liable to occur after secondary amputation, when the shocks were necessarily separated by a much longer interval.

Towards the conclusion of the volume there is an abstract of the cases, seventeen in number, of the traumatic tetanus which occurred in the Legion. The first six were treated by bleeding, opiates, and calomel combined with opium; none of the remedies being carried to any great extent: all these cases were fatal. Of the other eleven, six arose from simple flesh-wounds: of these, one recovered, which was treated by carbonate of iron; the others, in which bleeding, acetate of morphia, calomel and opium, and tartar emetic were the remedies, were fatal. The remaining five furnished one case of recovery, though the patient subsequently died of irritative fever with affection of the knee-joint. These cases were treated with opiates, bleeding, and tartarized antimony, all employed to a moderate extent. Amputation after tetanic symptoms had occurred was found, as usual, fruitless. Two cases of recovery out of seventeen is a more favorable result than is generally experienced in this obscure and untractable disease. We regret that a more extensive trial was not given to carbonate of iron, as it was successful in the only case in which it was employed. We have witnessed its failure, but this was only in a single case; whilst the medical officers of the Legion had a comparatively extensive field for trying its powers; and no compunctious visitings need have been felt for withholding the bleeding, mercury, opium, &c., of which the experience of many years has so clearly displayed the inutility.

We have already expressed very strongly our sentiments of the general merit of this work; and we would wish the reader to understand, notwithstanding the character of some of our comments on certain passages, that a minute scrutiny of its contents to the close, has left our first favorable opinion totally unchanged. The military surgeon especially will find it a very valuable, and, what under certain circumstances is by no means immaterial, a very portable addition to his library.

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## ART. XII.

1. *Inaugural Dissertation on the Presence of Air in the Organs of Circulation; submitted to the Medical Faculty of the University of Edinburgh, &c.* By JOHN ROSE CORMACK, President of the Royal Medical Society of Edinburgh, &c.—*Edinburgh*, 1837. 8vo. pp. 56.
2. *Remarks on a Case of Suicide, published by* Dr. P. D. HANDYSIDE, *in No.* CXXXIV. *of the Edinburgh Medical and Surgical Journal.* By JOHN ROSE CORMACK, M.D.—*Edinburgh*, 1838. 8vo. pp. 15.

3. *Bulletin de l'Académie Royale de Médecine.* Tome II. No. 12. *Rapport de* M. BOUILLAUD *sur les Expériences relatives à l'Introduction de l Air dans les Veines, faites par* M. AMUSSAT.—*Discussion sur l'Introduction de l'Air dans les Veines. Ib.*
4. *Lettre sur l'Introduction de l'Air dans les Veines de l'Homme.* Par M. VELPEAU. *Gazette Médicale,* 29 *Fev.* 1838.

THE question of introduction of air into the veins is not less interesting to the practical surgeon than to the physiologist. If any proof were required of the interest which this subject is calculated to excite, it might be found in the appointment, by the Royal Academy of Medicine of Paris, of a Commission of Inquiry; in the report of the commissioners; and in the protracted discussion to which that report has given rise. In the present article we propose giving a careful analysis of the labours of the commission, and of the facts advanced during the discussion; we shall also examine the paper of M. Velpeau, as well as a case recently published by Dr. Handyside, with Dr. Cormack's remarks upon it, and the prize essay of Dr. Cormack. As some arrangement is necessary in order to place the several facts derived from these sources in their proper light, we shall, in the first place, analyze the principal experiments which have been made on animals, and then proceed to detail the cases in which air has been introduced into the veins of the human body.

I. The forcible introduction of air into the veins has been long practised by veterinary surgeons, as a prompt and certain means of destroying life; and the experiments made by physiologists previously to the labours of the Commission have also consisted in the forcible introduction of air, either by means of a syringe or from the mouth of the operator. In the experiments made by Nysten, in 1811, and cited at length in the Report of the Commission, air was injected by means of a syringe; in those of Cormack, it was blown through a tube. We shall, in the first place, state the principal results of these experiments as briefly as possible.

In four experiments made by Nysten, a quantity of air, varying from thirty-five to fifty-one cubic inches (English), was injected into the jugular vein of spaniels, in separate quantities, varying from eight and a half to eleven cubic inches, in a period of from five and a half to twelve minutes. Death followed almost immediately on the last injection of air. In one instance, death was instantaneous; in two instances, it occurred at the end of two minutes; and in the fourth case, though the exact time is not stated, it was evidently very short. In the fifth experiment, thirty-seven and a half cubic inches of air were injected at once into the jugular vein of a small dog: death ensued in a few seconds. Cormack relates three experiments, in which death followed promptly on the forcible injection of air into the veins.

*Experiment* 1. Horse, seventeen hands high. Two full inspirations blown into the left jugular vein, through a tube a quarter of an inch in diameter. Duration of the experiment, one minute. Death in three minutes from the commencement of the experiment. Quantity of blood lost not greater than in an ordinary venesection.

*Experiment* 2. Rabbit. Three or four full inspirations introduced into the right jugular vein, through a tube the size of a crowquill, in the space of two minutes. Death in a few seconds after the termination of the experiment.

*Experiment* 3. Dog. A large quantity of air introduced suddenly, and with great force, into the jugular vein, in the space of six seconds. Death in six seconds from the end of the experiment.

From these experiments of Nysten and Cormack, it appears:

1. That considerable quantities of air may be injected into the veins, without proving instantly fatal.
2. That death follows the introduction of air more promptly as the quantity injected at one time, and the force employed in injecting it, are greater.
3. That the sudden and forcible introduction of a large quantity of air proves almost instantaneously fatal. (Experiment 5, Nysten. Experiment 3, Cormack.)

With regard to the quantity of air necessary to cause death, the report of the Commission has the following note:

> "Each time that Nysten injected a large quantity of air into the veins of animals with one stroke of the piston, death took place in the same manner as in the fifth experiment. When the animals were small and weak, as was the case with some spaniels, forty to fifty cubic centimètres (about fifteen and twenty cubic inches English) sufficed to cause almost instantaneous death. When large dogs, above the average size, were submitted to experiment, it was necessary to inject at once 100 to 120 cubic centimètres, (about forty and forty-eight cubic inches English.) Quantities much more considerable were required to destroy larger animals, such as oxen and horses, which are frequently killed in this way by veterinary surgeons."

The effects produced by the injection of air were not always equally marked. Thus, in the second experiment of Nysten, eight cubic inches of air, injected five times in succession, at intervals of less than two minutes, produced no severe symptoms; and a further introduction of twelve cubic inches did not cause instantaneous death. In the first experiment, on the contrary, in which the same quantity of air was injected into the jugular vein of an animal nearly twice the size of the one employed in the third experiment, the first injection of eight cubic inches of air was immediately followed by acceleration of pulse, and *a sound was heard analogous to that which is made by beating together white of egg and water: this sound came from the region of the heart, was synchronous with the systole of that organ, and depended on the mixture of the blood contained in the right cavities of the heart with the air injected.* It ceased entirely at the end of two minutes, and the pulse regained the frequency which it had before the experiment. A further injection of ten cubic inches of air was thrice repeated, and the animal died almost immediately after the last injection.

The symptoms produced by the injection of air into the veins vary in degree and in kind, in different experiments. Cormack briefly describes them thus: "The animal suddenly falls down, utters some cries, and speedily expires in convulsions." The symptoms observed in the experiments which have been cited are the following: Cries; frequent and violent efforts to inspire; feeble, fluttering, and often imperceptible action of the heart; convulsions; tetanic spasms; involuntary ejection of the urine and fæces; a sound resembling the beating up of white of egg and water, coming from the region of the heart, and synchronous with the systole of its ventricles.

Though these symptoms occurred in one or more of the instances alluded to, they are by no means constant. The tetanic spasms, and the involuntary discharge of urine and fæces, are mentioned only in the fifth experiment of Nysten; whilst the peculiar sound coming from the region of the heart was observed only in his first experiment. The effects produced by the sudden injection of thirty-seven and a half cubic inches of air are thus described: "After some seconds, cries, imperceptible pulse, violent agitations, opisthotonos, ejection of urine and fæces, some deep inspirations, and death."

*The post-mortem appearances* are not minutely detailed in the experiments of Nysten. The right cavities of the heart are stated to have been greatly distended by a mixture of air and frothy blood, while the left were entirely free from air. In the experiments of Cormack the air seems to have been more carefully traced through the circulating organs; it was not confined to the right side of the heart, but found its way in smaller quantity into the left; it existed in considerable abundance in the large veins near the heart, and in those of the abomen; and in one instance (Exp. 1) "air was found in every visible vein over the whole body." In the second and third experiment, no air was found in the left cavities of the heart; in the first experiment, however, "the left auricle contained frothy blood, and some coagulated masses. In the left ventricle the quantity of air was just sufficient to make its existence appreciable; but there was a great quantity of blood both fluid and coagulated in this cavity." (p. 11.) In the third experiment air was found in the coronary veins, and it is stated "as a circumstance worthy of note, that there was but little frothy blood in the pulmonary artery." No emphysema of the lungs existed in any of Cormack's experiments, and in the only one of the five experiments of Nysten in which the state of the lungs is noticed, emphysema was also absent. In detailing the post-mortem appearances in the third experiment, Cormack, after alluding to the distention of the right cavities of the heart, vena cava, abdominal veins, and those of the lower extremity, and remarking the absence of air from the left cavities of the heart and from the pulmonary arteries, says, "The obvious explanation of these phenomena is, that the air had been sent directly through the right auricle into the inferior vena cava and coronary veins; for its absence from the left side of the heart clearly indicates that what was found in the veins had not made the round of the circulation." The post-mortem appearances in the subject of the first experiment, seem to indicate the possibility of the air going the whole round of the circulation, although it finds its way into the left cavities of the heart in small quantities. Great venous congestion is a constant appearance.

In the experiments which have been quoted, the forcible injection of air into the veins caused death. We have still to notice those in which either no fatal consequences followed, or the animal survived the experiment some hours or even days. Two such experiments of Nysten may be briefly stated as follows:

*Experiment* 1. About seventy cubic inches of air injected into the jugular vein of a dog, in separate quantities of eight or twelve cubic inches. Eight injections were made occupying sixteen minutes, with intervals of two or three minutes between each two injections. The first injections caused a slight increase in the frequency of the pulse, without signs of pain. An injection of twelve cubic inches of air caused a slight increase in the respiratory movements. At the end of half an hour the animal presented no symptoms of disease; on the following day it ate its food as usual, and continued to enjoy perfect health till the tenth day from the experiment.

*Experiment* 2. One hundred cubic inches of air, injected in separate quantities at eleven times during the space of twenty-eight minutes, produced increased frequency of the pulse and respiration. When loosened after the last injection of air the animal lay on its side breathing naturally, and only appearing somewhat debilitated. An hour after the last injection a cough came on, and was renewed at intervals. The following day the cough was augmented, the breathing became laborious, and the pulse feeble. The dog was stretched out on the ground, and a transparent frothy liquid flowed from its mouth. Death thirty-five hours seven minutes after the last injection of air. Post-mortem inspection discovered the lungs gorged with blood, and with a large quantity of frothy mucus. Not a bubble of air was found in the heart or large vessels.

Nysten states that he repeated these two experiments frequently with the same results; that is to say, when he injected but a moderate quantity of air the animal soon recovered its health, but when he injected a large quantity of it, for instance, from eighty to one hundred cubic inches according to the size and strength of the animal, it was soon seized with an obstruction in the bronchi, producing cough, expectoration of frothy mucus, mucous rhonchus, and death. In some cases the animal did not die till the third day.

Three experiments of the same kind are related by Cormack. In one of these "*a large quantity* of air was injected into the jugular vein. Very violent effects were produced, so violent indeed, that the animal seemed for twenty minutes to be in a moribund, or at least in a very critical state. To our astonishment, however, it gradually came round, and in about an hour the inspiration, which was at first very laboured, became tolerably natural, and the rabbit was soon little out of sorts." This animal was killed after sixteen days; its lungs were quite healthy, and there was no air in any of the blood-vessels. In the two other experiments, "six or seven full expirations were injected through a small tube into the jugular veins of two dogs. A little difficulty of breathing ensued, but this soon passed away. At the end of half an hour the animals were killed, having up to that time seemed pretty lively, and exhibited almost no symptoms of uneasiness." On post-mortem inspection, lungs collapsed, no emphysema. "In both instances, when the scalpel was thrust into each of the cavities of the heart, a little air unmingled with blood rushed out with a whizzing noise."

In the two last experiments, the animals were killed so soon that no time was allowed to the lungs to become affected, as in the experiments of Nysten; the first experiment, however, more closely resembles those of Nysten and did not lead to the same results. It is impossible to establish an accurate comparison between the experiments of Nysten and Cormack, and we know not in which to place most confidence: Nysten's appear to us to have been made with the greatest care.

The experiments which we have just detailed, coupled with those first cited, establish the following results:

1. The forcible and sudden injection of considerable quantities of air into the veins proves almost instantly fatal.

2. The more gradual introduction of considerable quantities of air may either prove speedily fatal, (by embarrassing the action of the heart?) or cause death after an interval of many hours or even of a few days by exciting pneumonia, or may be productive merely of passing inconvenience.

3. When the injection of air proves *instantly or speedily fatal* the right cavities of the heart, and especially the right auricle, are found distended with air and frothy blood. Air is also to be found in all the large veins near the heart, particularly in the ascending cava and abdominal veins; it may also find its way into the veins of the extremities, and in rare instances is contained in every visible vein throughout the body. Air is rarely found in the left cavities of the heart, and then only in small quantities. When the injection of air has *not proved speedily fatal*, air is not to be found in any part of the circulating system.

4. Death, where it occurs soon after the injection of air, may be attributed to distension of the right cavities of the heart, and consequent impediment to the circulation: where death does not take place within some hours or days it may be referred to pneumonia.

These experiments clearly disprove the opinion of Bichât, that an extremely small quantity of air introduced into the veins is sufficient to cause death, as well as that of Leroy, that death is due in many instances to emphysema of the lungs.

Other experiments, both of Nysten and Cormack, still remain to be noticed, but these may be reserved till we come to discuss other questions connected with the introduction of air into the veins.

II. The researches of Nysten, in 1811, had no directly practical object, but were merely intended to solve a question of much interest to the physiologist. Cormack's treatise, published in 1837, throws little new light upon this subject, though it has now become as practically important as t was before physiologically interesting. Beauchêne, in the year 1818, first invited attention to the possible introduction of air into the veins during surgical operations; and, since his time, as many as thirty cases of real or supposed accidents of that kind have been published. In some of these cases the true cause of death is doubtful; and men of a sceptical turn of mind might impute the whole of them to other causes than the introduction of air into the veins. Hence a new class of experiments was obviously called for: the forcible injection of air into the veins could no longer serve any useful purpose beyond that of destroying life, and the duty of the experimenter was to teach the surgeon under what circumstances air *could introduce itself* into the veins; by what symptoms he might be warned of its admission; and to what remedial measures he ought to have recourse. These desiderata are supplied by the experiments of the commission appointed by the Royal Academy of Medicine.

Of these experiments, which are forty in number, twenty-nine were made on dogs, ten on horses, and one on a mule. The veins which were opened were the external jugular, subclavian, and axillary veins. In some instances these vessels were left in their natural position; in others they were put on the stretch by extension and separation of the limb. In some experiments the horizontal position was employed, in others the vertical; in some the animals were strong and healthy, in others purposely debilitated. The introduction of air was in some cases allowed to take place spontaneously; in others it was injected by a syringe or by the mouth.

We shall, on the present occasion, state as briefly as possible the chief results of these experiments, referring to the Third Part of this Number for an abridged abstract of the whole. We have thought it advisable to give this abstract, not merely because the experiments are in themselves interesting and valuable, but because, in consequence of the want of systematic method in their performance, it is almost necessary to examine them individually in order to appreciate the results deducible from the whole. In the following summary we shall first give the results of the forcible injection of air by a syringe or the mouth, and then examine the so-called spontaneous introduction of air.

A. As regards the forcible introduction of air the two following points seem clearly made out.

1. The injection of a considerable quantity of air is necessary to cause death or even to produce marked uneasiness.

2. The severity of the symptoms produced increases with the quantity of air injected, and with the force employed in its introduction.

It seems also probable, although not actually proved, that the injection of air which has been respired proves more speedily fatal than that of pure atmospheric air. As we have no means of instituting a strict comparison between the experiments performed with the two kinds of air, this statement must remain open to doubt. In two out of three experiments (20, 21, and 23 of the commission,) in which pure atmospheric air was injected into the jugular vein of dogs the animals recovered, whilst three experiments in which air was *gently* breathed into the vein through a tube, proved fatal. In these instances death took place in half a minute, one minute, and two minutes, respectively, from the first introduction of air. Three similar experiments made on horses proved fatal in five, five and a half, and six and a half minutes, respectively. In one of Cormack's experiments a dog was killed in twelve seconds, and in another a horse was destroyed in three minutes. Three other experiments, however, made by Cormack, prove that the insufflation of air is not necessarily fatal, for seven or eight inspirations did not destroy the life of two dogs, (they survived half an hour, and were then killed;) and a rabbit also escaped, though a large quantity had been thus introduced. Though it is highly probable, therefore, that the injection of respired air proves more fatal, and destroys life more speedily than that of pure atmospheric air, the confidence of the reporter of the commission (Bouillaud), as to this point, seems scarcely warranted by the experiments adduced. If the fact were fully established the explanation given by Bouillaud might be admitted as the true one, viz. that the more fatal effects of respired air are due to the change effected by respiration, and not in any degree to the mode in which the air is introduced. This point, it would seem, might be easily determined by injecting with a syringe a moderate quantity of atmospheric air having the same admixture of carbonic acid as it acquires in respiration. The cause of death, as well as some other questions suggested by the experiments in which air was forcibly injected, will be best discussed when we come to consider the so-called spontaneous introduction of air.

B. The experiments made to determine the conditions under which air may enter the veins, the symptoms produced by its admission, and the best means of remedying this accident, constitute the most original and valuable part of the labours of the commission. The practical bearing of these experiments will justify a more lengthened and careful analysis than we have thought it advisable to make of the experiments in which air was forcibly introduced. We propose to examine in succession each of the following points:

1. The conditions under which air may enter the veins.
2. The signs indicative of the admission of air.
3. The symptoms produced.
4. The period at which death takes place.
5. The post-mortem appearances.
6. The proximate cause of death.
7. The remedial measures which may be resorted to.
8. The application of these experiments made on animals, to the human subject.

1. *Conditions under which Air may enter the Veins.* In every instance in which an opening was made into that part of the external jugular vein in which the venous pulse, or the phenomenon of flux and reflux of blood, was observed, the introduction of air took place. When the opening was made into a part of the vein above the site of the venous pulse, no air was observed to enter (Exp. 1 and 30 of the commission); but if a tube was introduced into the opening and advanced as far as the site of the venous pulse (Exp. 19 and 29 of the commission), the entrance of air took place just as if the vein had been opened at its lower part. Air, however, may find admission into a vein in which no venous pulse exists, provided the orifice of the vein be kept open. Thus, in Expt. 9 of the commission, the axillary vein was opened where no venous pulse could be detected, and no introduction of air took place till the limb had been forcibly separated from the trunk, and the orifice of the vein kept on the stretch. In Experiment 10, however, the brachial vein having been opened at the lower edge of the axilla, and the admission of air favoured as much as possible, no air entered. Hence then, though air invariably enters a wound made into a vein in which the venous pulse exists, it *may* gain admission where the venous pulse is absent provided the orifice be kept open. The experiments do not determine the greatest distance from the part at which air may be introduced into a vein; but Experiments 9 and 10 of the commission, render it highly probable that, though it may find its way into the *axillary* vein when the position of the parts is favorable to its introduction, no means that can be resorted to will cause it to enter the commencement of the *brachial.* We may, therefore, fix upon some point between these two veins as the limit to the possible introduction of air; and as air did not enter the jugular when an opening was made into its upper part, or even into a point as low as the junction of the middle and lower third, we are justified in asserting that the limit of the possible admission of air into the veins is but little removed from the site of the venous pulse.

The following propositions embody the principal results obtained by the experiments of the commission under this head:

*a.* When an opening is made into a vein at any point at which the venous pulse exists, air immediately enters.

*b.* If an opening be made into a vein a short distance beyond the point at which the venous pulse exists, air may enter, provided the orifice be kept open; but if the opening be made at a considerable distance from the site of the venous pulse, no air enters, though the orifice be kept open and the admission of air be as much as possible encouraged.

The situation in which the introduction of air takes place points distinctly to the true cause of its admission. Barry and Poiseville have shown that during inspiration the blood of the large veins flows more freely towards the heart, and that during expiration it regurgitates. This effect does not extend, as Barry thought it did, to the small veins, but diminishes rapidly as we pass from the heart. Thus, in the experiments made by the commission, the venous pulse was observed in the lower third only of the external jugular vein; it was present also in the subclavian, but absent in the axillary and brachial veins. Now, wherever the venous pulse exists, wherever, in other words, the blood tends to flow more freely towards the heart with each inspiration, and to regurgitate with each expiration, then the contents of the vein, whether they consist of air or of blood, must be drawn into the heart at each inspiration, and again more or less completely expelled by each expiration. If an opening be made into a part of a vein somewhat beyond the situation of the venous pulse, and the aperture be kept open, air still gains admission; but if the aperture be not put on the stretch the pressure of the atmosphere approximates the sides of the vein, and the air does not enter. The introduction of a tube, however, by preventing the collapse of the vein, ensures the free admission of air. A rigid state of the coats of the vein would have the same effect.

2. *The Signs indicative of the Admission of Air into the Veins.* These consist of certain sounds heard in the vein itself and in the region of the heart. (*a.*) *In the vein* the sound resembles the *lapping* of a dog or cat, or, especially where the vein is very large, as in the horse, a loud *gurgling*. This sound is synchronous with the inspiration, but occasionally it is much more frequent, and then accompanies the diastole of the right cavities of the heart,—or of the right ventricle, according to Bouillaud, who states that the phenomena of flux and reflux have been seen to be synchronous not merely with the respiratory movements, but also with the diastole and systole of the right cavities of the heart. This, it may be remarked, harmonizes with the views of Mr. King on the safety-valve functions of the heart. (*b.*) *In the region of the heart*, as has been already remarked by Nysten, a *bruit de soufflet*, with or without a gurgling sound, is heard. This bruit continues for a long time after the air has ceased to gain admission into the vein, and is synchronous with the movements of the heart and not with those of respiration.

3. *The Symptoms produced.* At the end of from one to ten minutes from the first introduction of air, the respiration and circulation become embarrassed and frequent, the animal loses its strength, and is anxious and agitated. If the further introduction of air is prevented, the symptoms rapidly improve and the animal regains its strength and health. If, on the other hand, the air is still allowed to enter the vein, all the symptoms are aggravated, the respiration becomes more and more laborious,

the debility is extreme, the urine and fæces are discharged involuntarily, cries, convulsions, and tetanic spasms occur, and death takes place in the majority of instances within half an hour of the introduction of air. If death takes place at a later period the animal seems to die of exhaustion.

4. *The Period at which Death takes place.* This depends on so many different circumstances that it will be difficult to come to a just conclusion respecting it. The age, size, and strength of the animals submitted to experiment will exercise a material influence. The report of the Commission directs attention to two points, the effect of position, and that of previous strength or debility. In the vertical position death takes place more speedily than in the horizontal. Thus in four experiments on dogs placed in the vertical position, death took place in three, six, sixteen, and twenty-five minutes, respectively; in four experiments, in which the horizontal posture was maintained death took place in four and a half, fifteen, twenty-five, and thirty minutes, respectively. The average of the first four experiments is twelve minutes and a half, that of the last four is eighteen minutes and a half. The extremes in the first case are three and twenty-five, in the last four and a half and thirty-six. Hence, then, death takes place more speedily when the animals are placed in the vertical position than when they are placed in the horizontal position. This statement is corroborated by the fact that, whenever the animals were held upright all the symptoms became more distressing, but were instantly relieved by the resumption of the horizontal position. The relief thus experienced seemed to depend on the escape of blood and froth from the orifice of the veins.

The effect of debility in hastening the death of the animals submitted to experiment will be seen from the following comparison: Two dogs, not enfeebled before the experiment, (Expts. 3 and 5 of the Commission,) died, the one in a quarter of an hour, the other in half an hour: two others, purposely enfeebled before the experiment, (Expts. 6 and 7 of the Commission,) died in four and a half minutes and twenty-five minutes, respectively. Four horses, not debilitated before the experiment, (Exps. 30, 31, 32, 33, of the Commission,) died in fourteen, twenty-eight, thirty-five, and 104 minutes, respectively; whilst four other horses, debilitated before the experiment, (Expts. 34, 35, 36, 37, of the Commission,) died in nine, twelve, thirteen, and sixteen minutes, respectively. The mean of the first four experiments is forty-five minutes; the extremes, fourteen and 104; the mean of the last four experiments is twelve and a half, and the extremes nine and sixteen. Hence, death takes place more speedily in animals which have been purposely debilitated. The shortest period in which death took place in consequence of the spontaneous introduction of air was three minutes, in a dog placed in the vertical position, and not debilitated before the experiment; whilst four and a half minutes was the shortest time in a dog placed in the horizontal position, and debilitated before the experiment.

5. *Post-mortem Appearances.* (*a.*) The examination made immediately after death. The distension of the right cavities of the heart and of the pulmonary artery by frothy blood is a constant appearance, and sometimes increases the bulk of the organ to three times its natural extent. Sometimes (Expts. 6, 7 of the Commission,) unmixed air is found in those cavities. The left cavities of the heart, with one exception (Expt.

18), contained no air, and offered a striking contrast to the distension of the right side. In Expt. 18, only a few bubbles of air were discovered. The cranium having been opened in some dogs which had been placed in a vertical position, air-bubbles were found in the jugular of the opposite side, in the facial veins, and in the cerebral veins and sinuses. (Expts. 14, 22, 23, &c.)

(*b.*) Examination made some time after death.' In one only out of four dogs, which were inspected from four to ten days after the introduction of air, (Expts. 12 and 13,) the heart and large vessels were found free from air. In this instance the heart was found collapsed and flabby. Air was detected either in the left cavities of the heart (Expt. 27,) or in the arteries and veins of the extremities, in each of the other three dogs. Hence, then, the air which is at first confined to the right side of the heart and pulmonary artery passes, at the end of a certain time, through the pulmonary veins to the left side of the heart, and thence to the arteries and veins of the whole body. In horses, in addition to the distension of the right side of the heart, the left cavities of that organ, the aorta, and the vessels of the brain, were found to contain air; and that in cases where the body was inspected directly after death. This difference is attributed, in the report of the Commission, and with apparent justice, to the larger size of the capillary vessels of the lungs, which admit of the more free passage of the air from one side of the heart to the other.

6. *The Proximate Cause of Death.* This, in the opinion of M. Bouillaud, is threefold: 1. The enormous distension of the right cavities of the heart by the air expanded by the heat of the blood, and the consequent impediment to the free contraction of that organ. 2. The presence of air in the pulmonary artery and its branches; which air, by rendering the blood spumous, prevents its free transmission through the lungs. 3. When the air enters the veins of the brain, the compression which it exercises on that organ. The first and third of these have been already mentioned by Nysten; but to the first alone is any importance attached by Cormack. "It may be stated," he says, "as clearly established, that when a quantity of air, by entering the circulation, proves suddenly fatal, the immediate cause of the arrestment of the vital functions is the inability of the right side of the heart to contract and expel its elastic contents; and therefore all the phenomena which follow are consequences of this first cause." (*Thesis*, p. 21.) The experiments of Dr. J. Reid on the paralysis which the right side of the heart undergoes when over-distended, (to which we have formerly referred when speaking of asphyxia,*) appear to us to confirm this view to a certain extent, by giving a reason for its inability to contract when air has been forcibly injected into a vessel leading to its cavity. With regard to the second cause adduced by Bouillaud, we are disposed to think that it has been too much overlooked. The degree in which its operation will be conceded, however, must depend upon the views entertained of the physiology of the circulation. If we believe (as we think we are not only warranted, but compelled to do,) that the changes which the blood undergoes in the

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* See British and Foreign Med. Review, Vol. V. p. 104; and Edinburgh Med. and Surg. Journal, vol. xlv. p. 387.

capillaries, both of the lungs and of the general system, are necessary for the creation of a part of the motive powers of the circulation, it is obvious that, if these are stopped in any way, a check must also be put to the movement of the fluid. In asphyxia, we cannot doubt that the obstruction to the circulation commences in the capillaries of the lungs, in consequence of the deficient supply of oxygen preventing the due aeration of the blood; and that the paralysis of the right ventricle from over-distension, and that of the left from want of its required stimulus, are but results of this primary cause. Now, where death takes place rapidly from the forcible insufflation of air into the veins, it is easy to perceive how the distension of the right ventricle, and the consequent stoppage of the circulation, are the immediate results of the process. But where air has spontaneously entered, and is found to have passed into the pulmonary artery, it does not seem unreasonable to suppose that it may prevent the passage of the blood with which it is mixed through the lungs; the due relation between the blood in the vessels and the air in the cells being just as much disturbed by the admixture of air in the former as by its deficiency or impurity in the latter: and, if the blood be thus obstructed in the lungs, it is obvious that, as the right ventricle already contains an extraneous bulk of air, it must speedily acquire a fatal distension, from the return of the venous blood from the system. That the admission of air into the vessels of the lungs may act also more mechanically appears probable from an experiment of Dr. Cormack's. He threw a quantity of air into one of the mesenteric veins of a rabbit, and in eight minutes afterwards killed the animal. *The liver was in a state of almost complete anæmia*, and, upon slicing it, minute bubbles of air appeared at every point on the incised surfaces. There was no air apparent in the heart, nor in any part of the body except the liver. (p. 26.) Is it not highly probable that the admission of air into the vessels of the lungs would produce a similar state of anæmia in those organs, and, by preventing the return of a due supply of blood to the left side of the heart, produce syncope and death? The non-existence of air in the cerebral vessels, in more than one experiment, disproves the opinion of Bichât that death results in all cases from the effect of the air on the brain. The opinion, also, that where the animal survives the experiment during some time, and ultimately sinks, death is due to emphysema of the lungs, is refuted by the experiments of the Commission, no less than by those of Nysten and Cormack. Whether or not each of the causes assigned in the report of the Commission bears a part in producing death must remain a matter of doubt. The extreme distension of the right side of the heart seems alone sufficient to account for the fatal result in the majority of cases.

7. *Remedial Measures.* Upon this question, so important in a practical point of view, the experiments of the Commission throw but little new light. The compression of the chest and abdomen (Expt. 18) does not prevent the admission of air into the vein. How, indeed, should such compression prevent the introduction of air, unless at the same time it stop the respiration? For we have seen that wherever an opening is made into a vein in which the venous pulse exists,—in which, in other words, the contents of the vein are drawn into the heart by the expansion of the chest,—air must gain admission. Compression of the chest

and abdomen, therefore, cannot prevent the entrance of air into the veins. Can compression of these parts remedy the ill effects which follow the introduction of air? The experiments of the Commission are neither numerous nor accurate enough to determine this question. The only experiment (Expt. 27) in which compression was tried is valueless. The air had only continued to find admission into the vein during a quarter of a minute: now, from the introduction of so small a quantity of air, death would in all probability never have happened, even had compression of the chest and abdomen not been resorted to. It is true that the compression caused a quantity of frothy blood to issue from the vein; it might therefore have proved useful. If compression were repeatedly exercised, and the aperture of the vein closed after each compression, there is little doubt that good effects would follow, and that life might in some cases be preserved; but the experiments of the Commission throw no light on the subject. Two experiments (28 and 29) were made with a view of ascertaining the effect of withdrawing the air by means of a syringe; but these, too, are inconclusive; and in one of them the syringe was out of order. In the first experiment, a mixture of pure blood and froth was extracted; but in the second pure blood only was withdrawn. In both instances the animal recovered. All that we are justified in saying of the two remedies,—compression of the chest and abstraction of air by means of a syringe,—is that they are likely to prove beneficial, but that the experiments of the Commission lend us no assistance in forming an opinion. The symptoms produced by the introduction of air were evidently relieved, in more than one instance, by allowing the free exit of blood from the opening of the vein, and aggravated by closing the orifice. In two of Nysten's experiments, also, life seems to have been saved by the free flow of blood, in the one case, from an accidental wound; in the other, from a free opening made intentionally into one of the large veins. Nysten states that the beneficial effect of bloodletting was established by repeated experiments. The attention of the Commission does not seem to have been directed to this subject.

We have now brought our review of the labours of the Commission to a close; and, though we feel under obligations to them for some useful additions to our knowledge, we cannot but regret that a valuable opportunity has been, in a great measure, lost of throwing light upon a subject of great practical importance. If our limits would permit, we might be tempted to hazard some remarks on the *art of experimenting;* an art which the Commission have not thought it necessary to study or acquire. It is quite possible to introduce into experiments on animals the same accuracy, the same strictness, which we employ in our experiments on dead matter. The admirable experimental researches of Edwards have furnished a model which others would do well to imitate: those of the Commission will supply us with examples of many of those faults against which the inexperienced observer requires to be cautioned.

III. We now pass on to the application of experiments made on animals to the human subject. We shall not stop to discuss the difficult question, whether or not the experiments made upon animals are strictly applicable to the human subject; but, assuming that they are so, we shall compare them with fatal accidents which are stated to have followed the admission of air into veins in the neighbourhood of the chest, wounded during operations. Between thirty and forty such accidents have been described with greater or less minuteness, but there are but few of which the accounts are not open to serious objections. We shall arrange them in four groups: 1, The facts which are narrated upon hearsay; 2, Cases in which death has not followed the supposed admission of air; 3, Fatal cases in which no examination was made after death; 4, Fatal cases examined after death.

1. *Cases stated upon Hearsay.* These are four in number, and are stated to have occurred in the practice of M. Graefe, Sir A. Cooper, Lodge, and Duportail: they do not merit the slightest attention.

2. *Cases not followed by Death.* These are fifteen in number, and occurred in the practice of Mott, Clémot, Barlow, Warren, Roux, Mirault, Rigaud, Delaporte, Dubourg, Malgaigne, Bégin, Toulmouche, Amussat, and Velpeau. We shall notice each of them briefly. Mott (*Journal of Surg. and Med. Science, &c.* Nov. 1828, p. 127,) extirpated a tumour from the region of the parotid gland: the facial vein was opened; a peculiar sound was heard; the patient uttered a cry of distress, and nearly fainted. There is nothing characteristic in these symptoms, and the vein opened was too remote from the thorax to admit of air. In M. Clémot's case, a tumour was dissected from the region of the axilla: a sound of *aspiration* was heard; the patient fainted, but soon recovered. Here also there is nothing characteristic of the introduction of air. The same may be said of a second case, in which the same sound was heard after cutting a small vein beneath the clavicle; but no fatal consequences ensued. In a case by Barlow, a transient fainting was attributed to the division of a varicose vein on the *cheek*; a part at which the introduction of air seems impossible. Two cases are narrated by Dr. Warren, in his work on Tumours: in one of these (that of W. Buril, p. 298,) a cancerous tumour was extracted from the neighbourhood of the parotid gland: on exposing the carotid artery, with a view of applying a ligature to it, a vein was opened, and a sound was heard resembling the passage of air-bubbles through water. The patient complained of being ill; symptoms of apoplexy supervened, which were relieved by opening the temporal artery. No further bad consequences followed. Here the only symptom which could indicate the admission of air was the sound referred to. M. Roux's case is not worth mention. Rigaud, (*Quelques Faits de Pratique Chirurg.* Paris, 1836,) in performing an operation on the subclavian artery, opened a vein, which he took for the external jugular: a peculiar sound, a sort of aspiration of air, was heard three several times; but no dangerous consequences followed. M. Delaporte, (*Bulletin de l'Académie*, tome i. p. 132,) during the extirpation of a tumour from the neck, was alarmed by a whistling sound and sudden syncope. No dangerous consequences followed. The external jugular was opened by M. Malgaigne (*Gaz. Méd.* 1836, p. 167,) and the same whistling sound was heard; followed, however, by no bad symptoms. M. Amussat's case (*Bull. de l'Acad.* t. i. p. 894,) resembles the two former, except that the sound supposed to characterize the admission of air was *jerking*, and as it were *zigzag*. Velpeau relates the following case, (*Bull. de l'Acad.* t. i. p. 896:) having wounded the internal jugular vein during an operation, a distinct

whistling was heard, followed by a sort of bubbling sound at the bottom of the wound. The patient cried out that she was dying, and fainted. The vein having been secured by the finger, the operation was completed without further accident. M. Bégin's case (*Presse Méd.* p. 463,) resembles the above: he opened the internal jugular, and immediately heard a sort of *glou-glou*, followed by no bad consequences. M. Mirault, in dissecting a tumour from the right side of the neck, wounded the internal jugular: three distinct whistling sounds were heard, and tetanic spasms followed. The patient soon recovered, but died suddenly at the end of three hours. In a case stated by M. Toulmouche, (*Bull. de l'Acad.* t. ii. p. 146,) a similar sound was followed by fainting. The operation was performed on the breast, and the supposed introduction of air took place during a forcible separation of the arm from the trunk.

In one or two of the above observations, the vein which was opened was situated beyond the point at which, in the experiments made on animals, air was found to enter the vein. The effects produced are, therefore, in all probability, referrible to other causes than the introduction of air into the veins. In the other cases, the veins which were wounded were within the distance from the thorax at which air may find admission into a vein. In these instances no fatal effects ensued. The syncope which occurred may or may not have been caused by the introduction of air. The peculiar sound which is stated to have been heard is almost the only ground for attributing the symptoms to that cause. The admission of air is not *proved* by these cases; but, granting that air did enter the wounded veins, we have the satisfaction of knowing that it is frequently followed by no dangerous consequences.

3. *Fatal Cases not examined after Death.* These are five in number, and occurred to Warren, Barlow, Goulard, Klein, and Mangeis. Warren's case is given at full length in the British and Foreign Medical Review, for April, 1838, p. 420: it leaves much to desire, and does not carry very strong conviction to our minds. In separating a tumour from its connexion with the axillary vein, "a vein was divided, and a small quantity of venous blood discharged. Scarcely was this done when *the patient struggled, her complexion changed to a livid colour, and at the same instant a bubbling or gurgling noise, which had not been noticed before, was heard, though indistinctly;* but the place from which it issued was not visible, the surrounding skin and fat lying over it. On this, the axilla was immediately compressed. The patient became insensible, *breathing as in apoplexy.* The tumour was at once separated. The posture of the patient was changed, and stimulants were diligently administered; but the pulse became less distinct at every instant, the lividity of the cheeks increased, the body grew cold, and the respiration more and more feeble until it entirely ceased. The lungs were inflated as a last effort, but without avail." We confess that we are sceptical as to this case. An *indistinct* bubbling or gurgling noise is the only symptom which appears to us to be characteristic; and in an *indistinct* sound of this sort, occurring in the course of an operation, we place but little confidence. The struggles might or might not have been caused by the introduction of air; the other symptoms produced might as easily be attributed to apoplexy from determination of blood to the head. It is to be regretted that a post-mortem examination was not made. The case

narrated by Klein (*Gräfe and Walther's Journal,*) is even less convincing than that of Warren: he extirpated the thyroid gland in the case of a deaf and dumb child, and death took place in less than a minute. This case occurred in 1814, but was not narrated till long after that period, when it was adduced in confirmation of Dupuytren's case. The rapidity with which death occurred seems to be the only ground for attributing the fatal result to the introduction of air: but this very circumstance renders the assigned cause doubtful; for, in experiments on animals, three minutes was the shortest space of time in which the introduction of air proved fatal. Goulard's case is not more satisfactory than that just mentioned. In the course of an operation he wounded a vein, which he supposed to be the axillary vein, but from which little blood flowed. The patient was seized with convulsions, and died. As the introduction of air is not the sole cause of convulsions, we can scarcely admit this case as an instance of death from that cause. (See *Gazette Méd., &c.* 1833.) Barlow's case is more satisfactory. He was removing a tumour from the neck of a woman, and, whilst dissecting the skin, he heard a whistling and gurgling sound. The patient died suddenly, *without sighs or convulsions.* Sighing and convulsions, though usually present in the experiments on animals, were not invariable symptoms: the absence of them, therefore, does not entirely exclude this case from the number of those in which death was caused by the introduction of air into the veins. Mangeis bled a woman in the arm: eight ounces of blood had scarcely flowed from the vein, when the patient uttered a cry and expired. This case, too, is mentioned as an instance of the fatal effects of the introduction of air. We need not remark that there is no foundation whatever for the opinion.

4. *Fatal Cases examined after Death.* These are seven in number, and rest on the authority of Piedagnel (Beauchêne), Dupuytren, Castara, Delpech, Roux, Ulrich, and Putegnet.

Beauchêne (*Piedagnel, Thèse,* No. 250, Paris, 1827,) was removing a large tumour attached to the right shoulder of a man, twenty-three years of age. But little blood had been lost, when, on separating the last adherent portion of the tumour, M. Piedagnel heard a sound resembling the entrance of air through a small aperture into the chest of a living animal. It was the opinion of all present that the pleura had been wounded. The patient exclaimed, "Mon sang tombe dans mon cœur! je suis mort!" He became pale, his head fell backwards, the eyes were fixed, and he could not distinguish objects. Respiration was easy but loud, and seemed to be performed chiefly by the left lung: the movements on the right side of the chest being very feeble. The pulse was extremely small, frequent, hard, and irregular. The whole body was covered with a cold sweat, and he had some convulsions. All restorative means failed, and the patient died a quarter of an hour after the fatal cut had been given. On inspecting the body eighteen hours after death, the right pleura was found not to have been opened, but the external jugular vein had been wounded: in fact, a portion of this vessel (half its caliber, and an inch in length,) had been cut out. The wound in the jugular terminated just as it joined the subclavian. *None of the cavities of the heart contained any blood. The walls of the right cavities were flabby, very thin, pale, and of a much greater caliber than those of the* *opposite side.* The brain presented a gray appearance, and all its blood-vessels, of a size sufficient to be visible, were distended with air. The aorta, crural arteries, and inferior vena cava, contained air mixed with blood. (See Cormack, pp. 14, 15.) The empty and flabby state of the cavities of the heart is peculiar, and did not exist in any of the animals submitted to experiment. The existence of air in the vessels seems to put the question as to the cause of death beyond all doubt. In Dupuytren's case, death may be ascribed, with equal confidence, to the same cause. In removing a tumour from a girl's neck, "a sound similar to that heard in the last case led the operator to remark that, had he been cutting in the neighbourhood of the air-passages, he would have supposed that he had made an opening into some of them. No sooner had he said this, and at the same time given the last stroke of the knife which concluded the separation of the tumour, when the patient exclaimed 'I am dying!' was seized with a general trembling, and quickly expired." "The body was examined twenty-four hours after death, and *the right auricle was found distended with air.*" "The other cavities of the heart (which were healthy), the arteries and veins throughout the body, and the membranes of the brain, contained fluid blood mixed with air." (*Cormack*, pp. 15, 16.) M. Castara's case is thus stated by Velpeau: "In 1826, a surgeon of Luneville, M. Castara, (*Saucerotte, Thèses de Strasbourg,* Mars, 1828,) removing a tumour which occupied the fossa infra-spinalis of the right shoulder, heard all at once a sort of *glou-glou* at the bottom of the wound. The patient, aged twenty-one years, fainted and expired suddenly, without convulsive movements. Twenty-four hours after death, the right cavities of the heart were found filled with liquid blood, mixed with a large quantity of air-bubbles. There were some bubbles of air also in the left cavities of that organ. The entire venous system of the upper extremity, including the forearm, was also filled with air-bubbles. The only vein opened was a branch of the sub-scapular, and the opening was less than a line in diameter." Upon this case M. Velpeau makes the following remarks: "There are in this fact some very remarkable particulars. It is the only one at present on record which can be compared, as far as the state of the blood in the heart is concerned, to the experiments made on animals. But the patient died suddenly; a circumstance which has never been observed in direct experiments. So small an opening, and that made into a vein beyond the axillary, does not allow the entrance of air to a degree to endanger life either in dogs or horses. We cannot understand, moreover, the presence of air in the veins of the right forearm, when this gas was absent from the inferior vena cava and from the left upper extremity. Though this fact of M. Castara is one of the most convincing, it leaves still some doubt on the mind." Delpech, (*Mémorial des Hôpitaux du Midi, deuxième année,* pp. 231—654,) in extirpating the arm at the shoulder-joint, heard two *snuffing* sounds, followed by fainting and instant death. The hemorrhage had been inconsiderable. The body was placed under water, and a considerable quantity of air-bubbles was found in the right cavities of the heart. In M. Roux's case, (see *Gaz. Méd.* 1833, p. 498, and *Transact. Méd.* t. x. p. 75,) a *whistling* sound was heard, whilst he was removing a tumour from the region of the parotid gland. The patient immediately uttered a plaintive cry, and was strongly agitated. A fainting took place, which was not of long continuance; the patient went on well for some days, but died at the end of a week. Air-bubbles were found in all the vessels. Though the presence of air in the veins proves its admission during the operation, there is no proof that the fainting was due to that cause; and there is no reason to attribute the death of the patient to it. Ulrich relates a case (*Journ. des Conn. Med. Chir.* tom. xi. p. 91; or *Gaz. Méd. de Berlin*,) in which the internal jugular was opened during an operation. A *whistling* sound was heard, and death took place in a minute. The right auricle was distended by air, but contained no blood. Black and liquid blood, without air, in the right ventricle. The suddenness of the death in this case is worthy of remark: as we have before stated, death never took place in animals in less than three minutes. In another case, stated by M. Roux, *Journ. des Conn. Méd.-Chir.* t. iv. p. 108; or *Revue Méd.* 1836, t. ii. p. 417,) gas was found in the heart and vessels after death, from a severe operation performed on a man already much debilitated. The upper extremity was extracted at the shoulder-joint, in consequence of a severe burn followed by sloughing of the whole limb. The patient, previously so greatly debilitated, fainted, and could not be restored. Some persons who were present *thought* that they heard a sort of whistling sound. Putegnet mentions the following fact in his thesis, (*Thèse*, No. 156, p. 41, Paris, 1834:) A man who had had an apoplectic fit was immediately bled from the jugular: death took place suddenly, and air was found in the right auricle.

The brief and imperfect sketch which we have given of the above facts, while it will form a sort of index, may prove useful by directing attention to many parts of this interesting subject which still demand careful examination. Comparatively few of the instances adduced can be regarded as satisfactory cases of the introduction of air into the veins. The report of the Commission selects the cases of Beauchêne, Dupuytren, Delpech, Roux, and Amussat, as the most conclusive. Velpeau regards the admission of air as *probable* in the cases of Bégin, Malgaigne, Mirault, Warren, Barlow, Delaporte, in one of the two reported by M. Roux, and by M. Clemot, and in the case mentioned by himself. He says that the introduction of air is *extremely probable* in the cases of Delpech and Ulrich; *and almost certain* in those of Dupuytren, Castara, and Goulard. He adds, however, that no one of these facts can be strictly compared to the results of direct experiment, and that all of them fail in producing complete conviction. With this opinion of Velpeau our own entirely coincides. If the experiments of the Commission could be assumed as the standard of comparison, to which all the cases of introduction of air into the veins of the human subject might be referred, almost all the cases reported would be effectually excluded by a want of strict resemblance in one or more of their symptoms to those observed in the case of animals. Thus, all the cases of sudden death from the supposed introduction of air would be shown to be fabulous, by the fact that three minutes was the shortest time in which death took place in the experiments on animals. The absence of convulsions or of any other symptom observed in the animals submitted to experiment would throw a doubt upon many other cases. The absence of distension of the right cavities of the heart by frothy blood would effectually exclude others; the position of the wounded vein would add to the difficulty; till at last no single conclusive case would remain. But there is one appearance observed after death, which can leave no doubt whatever in the mind of the most sceptical,—viz. the presence of air in the several parts of the circulating system. It is true that air may be *generated*[*] in the vessels of the living body; but it is an extremely rare occurrence, and would be a singular coincidence indeed if it occurred in every person submitted to an operation. These cases, then, in which air was found in the circulating system after death, may be admitted as conclusive of the introduction of air through a wounded vein; but they cannot be regarded as satisfactory evidence that death was attributable to the presence of air: for we have seen that the admission of air into the veins is not necessarily attended by fatal consequences, and that a very considerable quantity of air may be introduced without causing even marked uneasiness. In the course of surgical operations, too, so many other causes exist capable of destroying life,—as loss of blood, exhaustion from excess of pain, fear or other strong emotion, determination of blood to the head, (as in one, if not in both the cases reported by Warren,)—that we ought to hesitate before we attribute the fatal consequences to the admission of air, even should air be discovered in the several parts of the circulating system. The tendency of the human mind to refer results to causes which are least understood, and most calculated to indulge the love of the marvellous, is well known: against this the medical man has especial need of being placed on his guard; and, in the case of the subject which we are now discussing, it is more particularly necessary. A better instance of the tendency to which we allude can scarcely be imagined than the one with which Dr. Handyside[†] has furnished us. A Mr. Doherty, who had been for some days labouring under delirium tremens, cuts his throat, and loses (according to Dr. H.'s own statement) a pound and a half of blood from a large number of divided vessels. Now, one would think that the sudden loss of this quantity of blood would be sufficient to cause and to explain death; but air was found in several arteries and veins:—*ergo*, death was attributable to the admission of air into the organs of circulation. But this appears to us, as well as to Dr. Cormack, a very hasty conclusion; and, with the view of showing our readers that it was so, we shall enter a little more fully into the details of the case, which are interesting in several points of view.

In the first place, we happen to know that the individual concerned (who was in the first year of his medical studies) had attended on the previous day a lecture on the anatomy of the neck; and the character of the incisions, which were not made in the usual situation, evince the influence of the knowledge thus attained on his mind. We believe that the lecturer had alluded to the fact that persons attempting suicide usually cut across the *front* of the neck, and that the wound thus inflicted is not necessarily or speedily fatal; whilst any one who was acquainted with the anatomy of the parts would make his incision on the *side* of the neck, over the carotid artery and jugular vein. This very course was adopted in a most extraordinary manner by the subject of the present relation.[*]

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[*] See Cormack's Thesis, chapter iv.

[†] See Edinburgh Med. and Surg. Journal, vol. xlix. p. 209, &c.

Two wounds were inflicted by Mr. Doherty, one on each side of his neck, running nearly parallel with the ramus of the lower jaw. On the left side, we learn from Dr. Handyside that "the wound extended obliquely downwards from the lobe of the ear towards the junction of the body with the left cornu of the os hyoides, measuring two inches and a half in length; and it appeared from its direction to have been made with the instrument held in the left hand of the individual. [It was found upon enquiry that Mr. D. was ambidexter.] The platysma myoides to the breadth of two inches and a half, the anterior two thirds of the sterno-mastoid, with the trunk of the spinal accessory nerve, and the lower part of the parotid gland, had been freely divided. Of the parts beneath, the conjoined origin of the occipital and posterior auris arteries, with the posterior facial vein, were found cut across; and through the latter of these vessels, several globules of atmospheric air were found to have entered the internal jugular vein."

The wound on the right side was of very remarkable character. It was four inches in length, the middle part corresponding with the angle of the jaw, but half an inch beneath it. One superficial and two deep wounds had evidently been inflicted, if not by another person, with the right hand of the deceased himself, as was evident from the appearance of the edges of the incision. This was a remarkable circumstance, a suicidal wound on the neck made with the *right* hand being almost invariably on the *left* side; but in the present instance no doubt of its character could be felt. It was found to have laid freely bare the transverse process of the atlas, and to have opened up the space which intervenes between it and the transverse process of the axis, sufficiently to expose to view the vertebral artery. Amongst the parts which were divided were the internal jugular vein with the pneumogastric and anterior branches of the descendens noni nerves; but the carotid artery was itself unwounded. This curious escape appear to have been due to the nature of the instrument with which the wound was inflicted, and the mode in which it had been employed. The razor had a thick blunt extremity, and appeared to have been *dug* into the wound in such a manner that whilst the terminal part of its edge divided the sheath of the vessels, and the vein and nerves, this portion had pushed the artery before it. These facts are all interesting when the case is viewed in reference to forensic medicine, as they tend to establish the *intent* with which the wound was inflicted. The following are the details relating to the injection of the vessels with air.

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[*] Instances of this kind are by no means of unfrequent occurrence. We related in our last Number, p. 171, a case of this kind from Sir Charles Bell; and a parallel one is related by Dr. Graves. "A very remarkable case occurred while I was at Hamburg. My friend, Dr. Oppenheim, was called out one day to visit a man who had cut his throat. The carotids escaped, but the injury inflicted was so great that the patient died after protracted sufferings. His body was given to Dr. O. for dissection. The man who took care of the place happened to be in the room at the time, and Dr. O. said to him in a joking way 'John, have you any fancy to cut your throat? If you have, don't do it in such a bungling way as this man; a little more to the left here, and you will cut the carotid artery.' The individual to whom this observation was made was a very sober steady man, with a family and comfortable subsistence; he never manifested the slightest tendency to suicide, and had no motive to commit it. Yet, strange to say, the sight of the corpse of a suicide, and an observation uttered in jest, made a powerful impression on him; the idea of self-destruction seized upon his mind, and, about an hour after Dr. Oppenheim left the dissecting room, he was horrified at hearing that John had cut his throat. Fortunately he had not profited by the anatomical instructions given to him; he did not cut the carotid, and consequently recovered.—*Lectures on the Institutes of Medicine in Lond. Med. and Surg. Journ.*; vol. vii. p. 69.

> "On examining with great care the contents of the blood-vessels, I ascertained that the internal jugular veins, though empty of blood, were slightly distended with air. The arteries of the neck generally were empty, with the exception of the left carotid, which contained a considerable quantity of air. The middle and inferior thyroid veins, the venæ anonymæ, the descending vena cava and the vena azygos were found partially distended with air, besides containing a little fluid blood. . . . The ascending vena cava contained a little uncoagulated blood and air. On cutting into the liver under the surface of water, air was observed to escape from the hepatic veins; and on treating the spleen and kidneys in a similar way, like results were obtained. The pancreas, and the coronary veins of the stomach presented also the same results. The abdominal aorta on being carefully insulated for three inches in extent by ligatures applied above and below, and also to its branches, and then placed under the surface of water, gave vent on being opened to several globules of air. The femoral, popliteal, and brachial arteries, upon being similarly treated, were observed to contain air."

Now, were this all our information respecting the appearances observed on the examination of this curious case, we should be disposed to regard Dr. Handyside as correct in attributing death to the entrance of air into the veins; and, in fact, this view was taken by Dr. Cormack in his prize thesis, being founded on an imperfect account of it. But Dr. Handyside farther tells us that "on opening the pericardium, the cavities of the heart were found collapsed and nearly empty. The right auricle was perfectly empty of blood with the exception of a minute coagulum at the foramen ovale. This cavity contained some atmospheric air. The pulmonary artery and its minute ramifications contained a very little frothy blood, quite insufficient to separate their walls which were collapsed." Now we have already seen that it was the constant result of the experiments of Nysten, as well as of the later researches of Amussat and others, that whether air was injected into the veins or allowed to enter them spontaneously, in all cases where death took place speedily, the right cavities of the heart and the pulmonary artery were found distended with frothy blood. On this subject Dr. Cormack remarks, as the result of his own experience, that

> "If the right auricle be very rapidly distended to such an extent as instantaneously to arrest its contractions, the blood and air are found unmixed; but this is a result which can only be obtained when a wide tube is used, and a great force employed in the introduction of the air. The fact, however, connected with the frothing of the blood, which it is of essential importance for us to bear in mind at present is this: *When an animal is speedily killed by the introduction of air, and bubbles of this fluid are found in the vessels, the right side of the heart is always distended with frothy blood.* Of the correctness of this statement I am perfectly satisfied from the results of numerous experiments, and it is an opinion amply corroborated by those previously performed by Nysten, Magendie, and many others." . . . "It is maintained, then, that the simple circumstance of *the heart being found collapsed, and destitute of frothy blood, of itself affords sufficient proof that Mr. Doherty's death has been erroneously attributed to the air found in the blood-vessels after death.*" (*Remarks*, pp. 7-8.)

Various explanations may be given of the existence of air in the vessels in this case, as well as of the immediate cause of death. Air may have been generated during life, as in the instances mentioned by Dr. Cormack in chap. iv. of his thesis. It appears, too, highly probable that in the case of Mr. Doherty, a considerable proportion of the air entered the vessels after death to supply the vacuum created by the loss of blood, and to fill up the void in the arterial system occasioned by the passage of its fluid contents into the veins, and the subsequent dilatation of their caliber. As there were several arteries as well as veins divided in the case of Mr. Doherty, there can be little doubt that through these apertures a good deal of air must have gained admission after death. As to the immediate cause of death, we think that it has been satisfactorily shown by Dr. Cormack that the quantity of blood lost was probably greater than the pound and a half at which it was estimated by Dr. Handyside; but that the rapid loss even of this quantity was not unlikely to have produced fatal syncope, especially in a person labouring under delirium tremens, as Mr. D. was known to have been, although the circumstance is not hinted at by Dr. H.

It is scarcely necessary for us to go over the arguments adduced by Dr. Cormack in refutation of the doctrines stated by Dr. Handyside on the proximate causes of death from entrance of air into the veins; since we have already given our own opinions on this subject, which nearly correspond with those of Dr. C. We must say, however, that we cannot regard this portion of Dr. H.'s paper as creditable either to his sagacity as a teacher of physiology, or to his candour as a writer. Inferences are drawn which are not in the least warranted by the experiments upon which they profess to be erected; and there is frequently, to say the least, a complete misconception of the meaning of authors from whom he has quoted, and a neglect of prominent facts which militate against his own views. His researches on the subject do not appear to have been very extensive, since his list of authors is evidently taken from Dr. Cormack's thesis; and from mere carelessness he has been led into an error by no means creditable to his discernment. He has quoted Bretonneaux as one of the experimenters on this subject, and has placed him next to Nysten on his list; when the fact really was, as stated by Dr. C. (Thesis, p. 25,) that the *experiments* of this gentleman were on the effects of the injection of pus into the veins, and that in one instance, air was supposed to have entered by *accident*.

In conclusion, we would refer our readers to Dr. Cormack's thesis for many facts and observations which our limits prevent us from alluding to; but must still recommend the question of the introduction of air to the attention of the profession as one which requires to be elucidated by future experiment. The perusal of the report of the Commission will suggest many enquiries which still remain to be made, and furnish many a caution by which the future experimenters may profit. We would urge upon all who may be disposed to prosecute experimental enquiries into this or any other similar subject, the necessity of much previous reflection, of a matured plan, and of minute and scrupulous care in performing every part of their experiments. Let every point which can be submitted to calculation or expressed in numbers be minutely attended to,—such as the quantity of the air introduced, the duration of the experiment, and of its effects, the size and strength of the animal,—let all the conditions of all the experiments, excepting always the point to be ascertained, remain the same, and the same certainty may be obtained with regard to the phenomena of life which already characterizes much of our knowledge of the laws which preside over dead matter. We are convinced that the uncertainty of our knowledge, on many points of importance, depends more on the defective methods we employ than on the essential difficulties which beset our investigations.

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## Art. XIII.

*The Life of* Edward Jenner, m.d. ll.d. f.r.s., &c. &c. &c. *With Illustrations of his Doctrines, and Selections from his Correspondence.* By John Baron, m.d. f.r.s., &c. &c. Two Vols.—*London*, 1827-1838. 8vo. pp. 624-471.

The first of these volumes has been some years before the public, and the other anxiously looked for, and quite recently published: they constitute one of the most interesting pieces of medical biography ever composed; and their perusal, whilst it cannot fail to please medical readers of every age, will at the same time be singularly serviceable to every youthful student of our profession, and particularly to all who cherish a hope of enriching medical science and serving the public at large by new discoveries in the science to which they are devoted. The history of Jenner exhibits the life-long efforts of a man of philosophical character and constant habits of observation, catching a glimpse, early in his professional life, of a truth of the highest pathological importance, connected with a discovery of amazing consequence to mankind: we see him keeping this important object ever in view, clearing away the obstacles between him and it, regarding it on every side, and submitting it, with equal industry, candour, and simplicity of mind, to the inspection of every eye; and labouring incessantly to establish it, when fully convinced of its real and.incalculable value,—despite of cold disregard and vehement opposition, and misrepresentation, and ridicule, and calumny: but sometimes also with the cheering sympathy of noble minds and the approving judgment of some of the first men of his time,—for the general good of mankind.

Such a spectacle cannot be contemplated too often, or reflected upon too much. Beholding the industry and patience of Jenner, and the partial success which alone crowned his efforts during his lifetime, we learn the useful lesson, that those who aspire to be benefactors of their race must sustain their efforts with a higher hope than that of popular favour; or be exposed to severe discouragement and the chance of desisting from their undertakings whilst they are yet incomplete.

The example set in this respect by Jenner has conferred immortality upon him. Around no tomb is there a purer wreath of fame than that which adorns his memory. Scantily encouraged when living, and almost denied a monument when dead, his life, his labours, his studies, constitute his true glory; for they are those of a mind of a high and pure order, destined to leave its annals recorded in good done to human beings for ever.

Easy and grateful would be the task of following his eloquent and affectionate biographer through the whole career of this great man; to trace not only his public services, but his private character; to paint him in his retirement, as well as in the full observation which he attracted. But the actual state of the public mind, and the anxiety at this time felt by the profession, on the subject which occupied so much of Jenner's thoughts, and which is so inseparably connected with his reputation, demands especial, if not exclusive attention; and to this point, though not without some sacrifice of our own inclinations, we shall at present particularly address ourselves. Amidst many recent demonstrations of public and professional opinion on this subject, very strong manifestations were exhibited at the late meeting of the Provincial Medical and Surgical Association, at Bath, of a general anxiety prevailing in the minds of medical men respecting the reappearance or increased prevalence of small-pox, and the precise degree of protection afforded by vaccination. The proportion of the cases in which it fails to protect, and the causes of this failure, were justly thought of sufficient importance to be made matters of immediate and extensive enquiry by a committee; and it may not be inappropriate to their labours to take a brief and unprejudiced view of some of the steps in the rise and progress of vaccination, from its discovery down to our own day. This we shall be enabled to do, without any apprehension of being wearisome, by following Dr. Baron's lucid order, and by condensing the valuable details with which his work abounds. There will still be left some topics of interest for further consideration, to be hereafter spoken of.

The first intimation received by Jenner of the possible existence of a disease communicated by contact from the cow, and capable of protecting the affected individual from the occurrence of small-pox, was given to him when he was an apprentice with a surgeon at Sodbury: he was doubtless previously acquainted with the popular notion of this, prevalent in that district of Gloucestershire, but it was at this time that it was first impressed on his attention; and from this time he never lost sight of it. Not yet twenty years of age, he contemplated the possibility of delivering mankind from the scourge of one of the most severe and desolating diseases to which human beings were subject: but he was nearly fifty before he published his first work on the Cow-pox. In the interval he had been the pupil of John Hunter, and, on commencing practice in the country, had kept his mind in continual exercise, not only on subjects strictly professional, but in various departments of natural history. He had endeavoured, generally in vain, to stimulate his medical neighbours to take a part in an investigation of which they could not see the importance. They had all seen examples of the alleged protection, but they had also witnessed puzzling exceptions, the explanation of which they wanted patience or genius to unravel. This task he performed himself; and in 1780, after five years of close attention to the subject, and when he was arrived at the age of thirty-one, he was enabled to explain what had perplexed and confused previous enquiry. He at that time stated, to his friend Edward Gardner, his opinion as to the origin of the cow-pox from the heel of the horse; specified the different sorts of disease which attacked the milkers when they handled infected cows, and the particular variety which afforded protection against small-pox, and anxiously and with much emotion communicated his hope that the result of this might eventually be the total extinction of small-pox.

When his mind was much occupied with these views, Jenner made some experiments respecting the nature of swine-pox. He inoculated his eldest son, in 1789, with swine-pox matter; and the result was a disease similar in its progress "to that which arises from the insertion of true small-pox matter, when the disease is very slight. He sickened on the eighth day: a few pustules appeared; they were late and slow in their progress, and small." The subject of this experiment was repeatedly and carefully inoculated for small-pox at subsequent periods, without the slightest effect. Two years afterward, "variolous matter was again inserted by two small incisions through the cutis." Of this the result was as follows. The inoculation was performed April 7th, 1791.

"9th. Evidently inflamed. 10th. An efflorescence, of the size of a shilling, spread round the inferior wound. 11th. The incision assumed a kind of erysipelatous elevation; the efflorescence much increased. 12th. The appearances much advanced. 13th. A vesicle, containing a brownish fluid, and transparent, about the size of a large split-pea, on the superior incision, the inferior about twice as big; the surrounding parts affected with erysipelas. The erysipelas extended to the shoulder, and then pretty quickly went off. The child showed no signs of indisposition the whole time.

"March, 1792. E. Jenner was again inoculated: the matter was taken from a child that caught the disease in the natural way, and had it pretty full. It was inserted fresh from the pustule. The same evening an inflammation appeared round the incision, which, at the end of twenty hours, increased to the diameter of a sixpence; and some fluid had already been collected on the lips of the scratch, which the child had rubbed off." (Vol. i. p. 130.)

This experiment, therefore, Dr. Baron observes, strongly corroborates the opinion, founded on other facts, that the swine-pox, the small-pox, and the cow-pox "had one common origin, and were, in fact, varieties of the same affection."

Several difficulties presented themselves to Jenner in the preliminary part of his enquiry. He soon found that what was commonly *called* cow-pox was not a certain preventive of small-pox. This circumstance was readily explained by ascertaining that cows were subject to various eruptions on their teats, of which the true cow-pox was but one. More perplexity was occasioned by the indubitable fact that even the true cow-pox did not always protect: but of this fact, which seemed at first sight formidable enough to put an end to the whole enquiry, Jenner first divined, and then by observation proved, that the cause was to be sought in the precise period of the disease in the cow at which the virus was communicated to the milker.

At length, on the 14th of May, 1796, the first attempt was made by Jenner to communicate the cow-pox infection by virus taken from the hand of a person casually affected by it, and inserted by artificial inoculation in the arms of another human being.

"Matter was taken from the hand of Sarah Nelmes, who had been infected by her master's cows, and inserted by two superficial incisions into the arms of James Phipps, a healthy boy of about eight years old. He went through the disease apparently in a regular and satisfactory manner; but the most agitating part of the trial still remained to be performed. It was needful to ascertain whether he was secure from the contagion of small-pox. This point, so full of anxiety to Dr. Jenner, was fairly put to issue on the 1st of the following July. Variolous matter, immediately taken from a pustule, was carefully inserted by several incisions, but no disease followed." (Vol. i. p. 137.)

A letter of Jenner's is given, addressed to his friend Gardner, in which he relates this experiment; and he states that, having never before seen the disease except in its casual form, he was astonished at the close resemblance of the pustules, in some of their stages, to the variolous pustules.* Certainly no event so interesting to mankind as this had taken place since the discovery of the practice of inoculation; and of the vaccine inoculation the benefits were to be immeasurably greater.

From this period until 1798, further experiment was prevented by the disappearance of cow-pox from the dairies. In these circumstances, Dr. Jenner made many efforts to generate the disease from the heel of the horse; having adopted the opinion that the *grease* was the origin of small-pox and cow-pox: but there were difficulties in the way of these trials not easily overcome. Although this opinion was not correct, it has since been clearly established that the grease is the form in which the cow-pox affects the horse; and that matter taken from it will produce a pustule resembling the vaccine pustule, and possessing the same protecting power, without passing through the constitution of the cow. Of the latter fact Dr. Jenner was not aware when he published his Inquiry. It appears that there are at least four animals (the horse, the cow, the sheep, and the goat,) affected with a disease communicable to man, and similarly protective from what seems a more malignant form. The dog, the goat, the ass, and the sheep are capable of receiving the vaccine disease by inoculation.

Dr. Jenner's "Inquiry into the Causes and Effects of the Variolæ Vaccinæ," by which his great discovery was first communicated to the public at large, appeared in the form of a thin quarto, of scarcely more than seventy pages, in June, 1798; and was dedicated to his friend the late celebrated Dr. Parry, of Bath. The following views occupy the first few paragraphs:

> "The deviation of man from the state in which he was originally placed by nature seems to have proved to him a prolific source of diseases. From the love of splendour, from the indulgences of luxury, and from his fondness for amusement, he has familiarised himself with a great number of animals, which may not originally have been intended for his associates.
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> "The wolf, disarmed of ferocity, is now pillowed in the lady's lap. The cat, the little tiger of our island, whose natural home is the forest, is equally domesticated and caressed. The cow, the hog, the sheep, and the horse are all, for a variety of purposes, brought under his care and dominion.
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> "There is a disease to which the horse, from his state of domestication, is frequently subject: the farriers term it *the grease*. It is an inflammation and swelling in the heel, from which issues matter possessing properties of a very peculiar kind, that seems capable of generating a disease in the human body (after it has undergone the modification that I shall presently speak of,) which bears so strong a resemblance to the small-pox that I think it highly probable that it may be the source of that disease." (Vol. i. p. 146.)

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* The same remark was made by Soemmering, who had been much accustomed to the minute study of the eruption in small-pox: he was struck with the identity of cow-pox to small-pox in its mildest form; and particularly where there had been but *one* small-pock, a "pearl-like pustule." The very same remark was also made by Ch. L. Hoffman, whom Soemmering calls "a lynx-eyed man," although then eighty years of age. (See vol. ii. p. 216.)

Twenty-three cases were given, illustrative of the progress of the infection; the first sixteen being examples of the casual disease, and the rest the result of inoculation. Among the former is the case of an individual who was infected from the heel of a horse, and afterwards completely resisted the small-pox contagion: among the latter is mentioned that of his second son, Robert Jenner, who "did not receive the infection;" a circumstance rendered important by the circumstance that, at a later period, the said Robert, having been accidentally exposed to the contagion of small-pox, and being of course unprotected, as he had not gone through the cow-pox, was inoculated at once (Jenner having no vaccine virus at the time) for small-pox, as the only security in that emergency against the severity of the natural disease: of this fact the enemies of Jenner, of course, made the most. Dr. Jenner states it as his belief, in the Inquiry, that the facts it brought forward proved "that the cow-pox protects the human constitution from the infection of small-pox;" whilst the opinion that the source of the infection of cow-pox "is a peculiar morbid matter arising from the heel of the horse" was also supported by sufficient evidence, although not proved by actual experiments made under his own eye. At that time he thought that the matter of grease only acquired its protective power after passing through the system of the cow; but subsequent trials proved that this was not a necessary condition.

It is a strange fact in the history of mankind, that the same people who run so hastily in quest of quack medicines, and so readily submit to dangerous methods of counter-irritation, undeterred by the clearest cases of murder; the same people who believe in the astonishing virtue of the billionth part of a grain of camphor, or the curative influence of magnetic manipulations, should, in the case of Dr. Jenner's great discovery, have exhibited such excessive cautiousness that, in the whole course of three months which he passed in London, soon after the publication of his Inquiry, he could not obtain one individual as the subject of the vaccine experiment. He actually quitted London, after three months of patient or impatient waiting, without having vaccinated a single person. Mr. Cline, being supplied with some vaccine virus, did indeed insert it into the hip of a patient who had disease of the hip-joint; but this was done by way of counter-irritation. The patient, however, had the variolæ vaccinæ, and was subsequently inoculated for the small-pox in three places, "which were slightly inflamed on the third day, and then subsided." Mr. Cline became fully aware, it would seem, of the importance of Dr. Jenner's discovery, from the circumstances of this case, although he failed to propagate the vaccine infection from it. As Jenner was at that time unprovided with a supply of vaccine matter, an interval was offered in which the illiberal industriously derided the alleged discovery. Jenner felt this kind of misrepresentation very keenly, and, in reply to Mr. Cline's exhortations that he should remove to town, prophetically foretold the confusion that would, in all probability, arise before the true cow-pox matter was accurately distinguished from the spurious; especially as he foresaw that, even after the disease had been truly gone through, the common process of inoculation would in some cases produce inflammation, vesication, and even pus, in the wounded part, with some constitutional symptoms; circumstances, of which the enemies of science would avail themselves; although, as he observed, the same thing had happened again and again to those who had had the small-pox. (Vol. i. p. 150.)

We may here mention, as interesting facts connected with the early history of vaccination, that, four months after Dr. Jenner left London, he vaccinated the children of his friend Mr. Hicks, of Eastington, the first gentleman who submitted his children to the practice. The first person of rank who allowed her child to be vaccinated was Lady Frances Morton (afterward Lady Ducie). Several ladies, as well as gentlemen not of the medical profession, subsequently lent their personal aid to promote vaccination. Mr. Hicks became an experienced practical vaccinator, and was the first person to practise, in a doubtful case, a second vaccination a few days after the first: he observed the second vaccination making "immense strides to overtake the first;" and this remark became the foundation of the test proposed by Mr. Bryce, of Edinburgh. This was, perhaps, the only improvement not suggested by Jenner himself.

Dr. Jenner, says Dr. Baron, "always considered small-pox and cow-pox as modifications of the same distemper; and that, in employing vaccine lymph, we only make use of means to impregnate the constitution with the disease in its mildest, instead of propagating it in its virulent and contagious form, as is done when small-pox is inoculated." (Vol. i. p. 162.) To the illustration of these views a considerable portion of Dr. Baron's first volume is devoted. With great learning and research he endeavours to show,

> "First, that an eruptive disease, *common* both to man and to the inferior animals, has been known in different ages and in different countries; and that the descriptions given of this eruptive disease by various writers accord so completely with those acknowledged to be characteristic of small-pox, as to render it highly probable that this disease actually existed at a much earlier period than that usually assigned to its origin.
>
> "Secondly, that as there are numberless writers who have described the small-pox in man, so there are others of established name and reputation who have treated of a similar eruptive and pestilential disease as existing, in various countries and in different times, among the inferior animals, but especially among cattle; that to this disease they have unhesitatingly applied the name of Variola, and actually recommended such treatment as experience had proved to be useful when that disease attacks man." (Vol. i. p. 163.)

The numerous passages quoted from writers of various ages in support of these views are not of equal weight; but several of them seem almost decisive, and all deserve attention. As bearing on the appearance of a pestilential and eruptive disease among cattle, closely resembling variola, the account given by Dr. Layard, in the Philosophical Transactions for the year 1780, leaves scarcely a doubt upon the subject. The disease described by Layard was an eruptive fever of the variolous kind, prevalent among horned cattle, and rendered more favorable by inoculation: the epidemic prevailed on the coast of Friseland, North and South Holland, Zealand, and Flanders; but not in the similar coast and climate of the east of England. At other periods it was observed in England and in Picardy, where an account was given of it by Vicq d'Azyr; who agreed with Layard in considering it closely allied to the variolous eruption. Some of the animals had the neck covered with the eruption. Dr. Baron observes, that Layard's experiments by inoculation give an interest and a confirmation to the truth that some of the inferior animals are liable to small-pox in some of its worst forms; and he remarks, that if the disease observed by Dr. Layard had been mild instead of malignant, and if he had attempted to communicate it to man by inoculation, he might have anticipated Jenner's great discovery. It was then of less consequence than it now is to illustrate the connexion between the diseases of man and of the inferior animals, and no attempts were made to ascertain whether the variolæ could be communicated from man to the brute, or from the brute to man. The latter point is established by the discovery of the variolæ vaccinæ; and some experimenters appear to have established the former. Dr. Baron refers to M. Viborg's statements in the Medical and Physical Journal for September, 1802, that he communicated human small-pox to dogs, apes, and swine; and that it had been proved, at the Royal Veterinary College of Berlin, that the cow likewise receives the small-pox by inoculation. (Vol. i. p. 216.)

As Dr. Baron considers the evidence disclosed by the phenomena of Variolæ Vaccinæ strongly corroborative of the historical proofs adduced in support of the above-quoted propositions, and that this evidence is rendered more complete by the history of Variola, as it has been recognized in man, he devotes a chapter of his first volume to the history of Variola and of Variolous Inoculation. It is his opinion, we have seen, that the small-pox was known to and described by authors long before the date to which its origin is usually referred; and even 1500 years before the Christian era. The first recorded case of small-pox, under that peculiar name, is probably, he observes, that of Elfrida, the daughter of Alfred, A.D. 907; and the next in the monkish annals is that of her grandson Baldwin, who died of it in 961; although in the interval it cannot be doubted that many cases occurred, which escaped accurate description. The name of Variola, with its derivatives, seems to have been adopted "by the Latin family of Europe;" and that of pocca, or pock, or little pouch, by the Teutonic or Saxon. In the eleventh century, Avicenna first gave a clear account of small-pox and measles: he distinguished the confluent and distinct small-pox, and represented the measles as different from small-pox, "in a degree at least." Like Rhazes, he observed that "*small-pox may affect the same individual twice.*" Other writers continued to confound the two diseases. In the fifteenth century, Fernelius still included small-pox and measles among pestilences, treating of neither by name. Dr. Mead, says Dr. Baron, promulgated without due enquiry the opinion that small-pox could not affect the same individual twice; contrary to what was evidently the general belief among the early writers: and consequently, from Mead's time until within a few years, the second occurrence of small-pox was always looked upon as an attack of chicken-pox, swine-pox, or some other eruptive disease. The assertion of the friends of vaccination that second attacks did occur was disregarded; but Dr. Baron observes, that more than 130 different writers have recited examples of its occurrence. Louis XV. died of a second attack of small-pox, when sixty-four years of age. The fact of such liability is now universally admitted.

Among the innumerable and almost forgotten attempts to deprive Jenner of the honour of originality, it may be instructive to mention that one was an assertion that Boerhaave had contemplated something analogous to his discovery: but Boerhaave's idea, as clearly set forth in his aphorisms, quoted by Dr. Baron, (*Aphor.* 1388 to 1392,) was merely that of finding some specific preventive of small-pox in the shape of a medicine; and he suggested a combination of mercury and antimony. To such frivolous and vexatious attacks, however, are those exposed whose views are really original, and whose motives are the purest.

In 1717, Lady Mary Wortley Montague caused her son Edward, then a child, to be *inoculated* for the small-pox, at Constantinople, by Mr. Maitland, surgeon to her husband, then British ambassador there. In 1722, her daughter was inoculated, by the same surgeon, in England, "this child being the first known subject of the new practice in civilized Europe." To this accomplished and enterprising woman, therefore, we are indebted for a practice which at least deprived the small-pox of much of its virulence, although it caused its prevalence to be greater than before. For the honour of medicine it should be mentioned that the second child inoculated in England was a child of Dr. Keith. The royal family soon set a courageous example of the same kind; but the practice made way very slowly; particularly as it was soon discovered that cases of small-pox after inoculation were often severe. In eight years after its introduction, 845 persons were inoculated, of whom seventeen, or nearly one in fifty, died. Denunciations of the practice were thundered from the pulpit; and similar ones have been subsequently levelled against vaccination. But Dr. Baron justly observes that many of the clergy countenanced the new practice. We gladly acknowledge the aid given by the same learned body to the diffusion of vaccination. In 1740, the practice of inoculation had nearly fallen into disuse. Subsequent accounts from America and the West Indies, revived its credit, and "in 1746 the Small-pox Hospital in London was founded for the purpose of inoculating the poor, and of keeping the patients distinct from the general population of the city;" the latter regulation being soon unfortunately departed from. In 1752, notwithstanding confident assertions to the contrary, it appears that the deaths from small-pox had actually increased. In 1763, the practice of inoculation was prohibited in Paris, by royal authority, in consequence of the great and fatal increase given by it to the diffusion of small-pox. In Spain the practice was scarcely ever admitted; and there, it is said, and Dr. Baron believes, the small-pox has occasioned less mortality than in any other country. The conclusion which we must arrive at, with Dr. Baron, is that without proper restrictions and careful seclusion of the infected, inoculation does but augment the evils of small-pox by increasing the number who are affected, and severely affected. In 1786, the Empress Catherine invited Baron Dimsdale to Petersburgh, and submitted herself and her son Paul to inoculation: the practice became fashionable; and the small-pox became so prevalent, that Sir Alexander Crichton states, that previous to the introduction of vaccination, every seventh child born in Russia died of small-pox.

The period was now approaching when it was to be discovered that a disease derived from one of the domestic animals would exert a powerful influence on the human frame. From the preceding enquiry, it appears certain that a fatal pestilential eruptive disease, common to man and the lower animals, has been known from the earliest period of authentic history, and that a somewhat similar if not perfectly identical disease yet exists in various regions of the earth, which disease is often fatal, but is subject to various modifications in respect to virulence, and is susceptible of further by artificial communication. It is thus understood how sheep, horses, and other animals may be subject to the disease, as well as cows or oxen; and that matter derived from the horse in the grease, the form of the disease in that animal, has produced the true vaccine disorder. The existence of the variolæ vaccinæ in the dairies of England not seeming to have been of long duration, Dr. Baron thinks there is good ground for believing that the disease, as originally noticed by Dr. Jenner, was but the endemic remains of the more extensive or epizöotic disease described by Layard.

> "This opinion is strengthened by the following well authenticated facts. Soon after the publication of Dr. Jenner's discovery it was found that cow-pox existed in several counties of England—in Devon, Dorset, and Somerset, in Hampshire, Buckinghamshire, Middlesex, Wiltshire, Staffordshire, Norfolk, &c.; in all, eighteen counties. Accounts were also received of the existence of the variolæ vaccinæ among the cattle of Lombardy, Holstein and other regions where the pestilential eruptive disease, denominated *pestis bovina*, had previously raged, as recorded by Ramazzini and other medical writers. At a later period it was also found in Persia, both in cows and sheep, as appears in a letter from Mr. Bruce, English resident in Bushire, (11th vol. of the Edinburgh Medical and Surgical Journal, p. 270.) It was discovered also by Don F. X. Balmis among the cows of the valley of Atlixico near the city of Puebla de los Angeles; in the neighbourbood of Valladolid de Mechöacan, where the Adjutant Antonio Gutierrez found it; in the district of Calabozo in the province of Caraccas, by Don Carlos de Pozo, physician of the residence, and by Humboldt in the Peruvian Andes.
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> "In the Report of the Central Committee of Vaccination in Paris for the year 1821-22, it is stated that the disease has likewise been found in the vicinity of Clairveaux, by M. James Dubry; and to this list Ireland ought to be added on the authority of Dr. Barry, of Cork. From these facts it may very fairly be inferred that the variolæ will hereafter be found among cows in other parts of the world." (Vol. i. p. 239.)

A disease so widely observed cannot, Dr. Baron concludes, depend on mere local circumstances: and he looks upon its prevalence in every clime as another circumstance supporting the view of its affinity with small-pox. We do not admit the legitimacy of this last inference. Dr. Jenner, however, always maintained this view, and considered that in employing vaccine lymph he was only affecting the constitution with a mild instead of a virulent and contagious form of the disease; a view which has often been overlooked by disputants, with the effect of creating much useless discussion.

In a subsequent part of his first volume, Dr. Baron is enabled to allude to the recent prevalence of the variolæ vaccinæ in a malignant and fatal form among cattle. The report is given by an intelligent veterinary surgeon (Mr. Tiley) well acquainted with the disease. He was sent for, in 1825, to see a cow which was very ill, and found the cow's disorder to be aggravated cow-pox. On the teats were several blackish scabs, and the whole skin was one continued disease, "not of vesicles or scabs, but a discharge similar to that produced by a blister." There was violent fever, and the cow died. Between forty and fifty cows became similarly affected, but none died except the first. It was supposed that the infection was conveyed to the first cow by a servant of a neighbouring farmer, who was asked to milk it, and who daily dressed a greasy-heeled horse. All the servants who milked the affected cows had sore fingers, with one exception. In the second volume Dr. Baron adds an account of the epizöotic, from the report of Mr. M'Pherson, which prevailed in the northern parts of Bengal at a period (1832) subsequent to the publication of the first part of his work. It prevailed among cows, very extensively and severely; and Mr. M'Pherson procured matter from the cows so affected, and found it productive of the true vaccine disease, the protecting power of which was afterwards fully tested. The cows were affected with pustules all over the body.

It is pretty generally known, that soon after the publication of Dr. Jenner's first work, some of the first trials of vaccination were made at the Small-pox Hospital by Dr. Woodville; and the vaccine and variolous virus becoming commingled, an eruptive disease was produced, which, although milder than ordinary small-pox, was very different from "the benign solitary pustule" which characterizes the variolæ vaccinæ. The evil was unfortunately propagated all over Europe; but a fact of great value was, according to Dr. Baron, at least one of the results of this blunder; for it was proved "that even this matter, after repeated re-inoculations, lost much of its virulence; ceased altogether to produce eruptions; and at length became almost assimilated to the true vaccine character." (Vol. i. p. 245.) Two facts are here put in apposition by Dr. Baron: one, that the cow-pox sometimes affects the milkers with a severe disease; the other, that small-pox, after repeated inoculations, approximates in its nature to the mildness of cow-pox; the number of pustules gradually diminishing until only *one* is seen, and that at the place of insertion, efficiently protecting the constitution from subsequent small-pox, although running its course without constitutional disturbance. Had the result followed the inoculation with pure small-pox virus, the support thence afforded to Dr. Baron's and Dr. Jenner's views of the identity of the two diseases would have been vastly greater: but, in the case referred to, it was not true small-pox that was inoculated. In fact, the very circumstance of the vaccine character prevailing ultimately in this case, may be held as an argument against the identity of the diseases: for, if this identity existed, there seems reason for expecting that the results of this mixed infection should, in all future inoculations, exhibit some of the peculiar characters of each disease; there appearing no sufficient cause for the destruction of either: whereas, the contagions being considered essentially different, it might be supposed that they could not continue mingled without the character of one becoming predominant.

Those interested in this enquiry (and what medical man is not?) should by all means carefully peruse Dr. Baron's Eighth Chapter, which treats of the Difference between Variolæ and Variolæ Vaccinæ, and of Varioloid Diseases. It contains numerous facts, to which it would, as Dr. Baron says, have been easy to add many more, tending to show that if vaccination were steadily and carefully practised both the small-pox and the varioloid eruptions would disappear. The statements made of the mortality of small-pox may, indeed, now be usefully recalled to mind. Of all who were born, one in fourteen died of small-pox; and this even after the practice of inoculation was introduced. Of those affected by natural small-pox, one in five or six died; but of those who had been inoculated only one in fifty. "These conclusions were drawn by Dr. Jurin from an examination of the London bills of mortality for a period of forty-two years."

The other evil consequences of the disorder were many and great; but are too well known to require enumeration. The same may be said of the contrasted benefits of introducing a mild and non-contagious disease by vaccination; but if any of our readers should entertain any doubts on this subject we would earnestly recommend him to refer to the evidence from all parts of the world contained in Dr. Baron's pages. The facts observed by Dr. Thomson in Edinburgh, in 1818-19, and by Mr. Crosse at Norwich, in 1819, are mentioned in this part of Dr. Baron's work. Their value is well known to the profession. Dr. Jenner had long before ascertained some of the points confirmed by these epidemics: he had shown that previous infection could not infallibly prevent susceptibility to the variolous poison: he had proved that by inoculating a person with small-pox matter who had been previously vaccinated, a local vesication might be produced from which virus could be obtained capable of producing a mild but efficacious form of small-pox; and he had maintained that the varieties in the vaccine pustule comprehended "every gradation in the state of the pustule, from that slight deviation from perfection which is quite immaterial, up to that point which affords no security at all." He subsequently pointed out that fluid taken from a spurious vaccine pustule can propagate its like; and that fluid taken from a genuine pustule when too far advanced may produce varieties which may be perpetuated, a circumstance which he did not in his first vaccinations suspect, and which other inoculators continued to neglect for some time. These truths have been too often forgotten.

We conceive it to be unnecessary in the present day, either in vindication of Dr. Jenner, or for any other purpose, to dwell on the errors and misconduct of Dr. Woodville and Dr. G. Pearson in relation to vaccination. The desire to usurp the merit belonging to another was in these cases singularly blended with a misapprehension of the doctrines and practice of him on whom they would have inflicted injustice and injury. It is melancholy to reflect on such conduct, too often seen, indeed, to arise from an ill-regulated wish for fame, unaccompanied with a sincere love of truth. Zealous vaccinators in this and other countries fell sometimes into the same mistakes as these physicians, and inoculated with small-pox virus or mingled virus, with unfortunate effects; but they did not exhibit the unhappy passions to which the above-mentioned vaccinators were too evidently a prey. Nor can we omit to mention that the late Lord Egremont, on the occurrence of some disastrous consequences in the neighbourhood of Petworth, the result of *contaminated* matter supplied by Dr. G. Pearson, applied his honest and excellent understanding with success to investigate the real cause of these supposed effects of cow-pox matter: he frankly communicated to Jenner the supposed effects (variolous-like eruptions) which had appeared, received Jenner's clear explanations, and with them a supply of real cow-pox virus, with which numerous vaccinations were made, with the happiest products. It affords us sincere pleasure to be able to say, that although Drs. Woodville, G. Pearson, and Moseley, distinguished themselves by very different conduct, there were not wanting many medical men who approached the subject candidly and calmly, and soon and fearlessly avowed their perfect confidence in the protection afforded by the genuine cow-pox. Among these Mr. Ring deserves honorable mention. Staggered by the effects of Dr. G. Pearson's distributions, he industriously enquired into their causes, and he was mainly instrumental in procuring a public testimonial in favour of vaccination, to which were appended, among others, the eminent names of Baillie, Cline, Cooper, and Abernethy.

If our design in this article were to notice every step in the history of vaccination, it would afford some interest to the reader, and certainly some instruction to detail the circumstances attending its introduction into America. The zeal and courage of Dr. Waterhouse, and the enlightened candour with which he met those reputed cases of severe vaccine disease which there, as elsewhere, were the product of carelessness or mistake, might be contemplated with unmixed pleasure. The patronage afforded to it by President Jefferson, and in France by Lucien Bonaparte and the scientific members of the Institute, and in Spain by the unusual energy of the government of that country, must have brought gratifying reflections to the mind of Jenner, and have consoled him under the very different treatment which he so often experienced in our own country. Mr. Jefferson devoted his attention (in 1802) to establishing the "*point of time*, counting from vaccination" (these are his own words), "when the matter is genuine in all cases;" and he decided, and Dr. Waterhouse confirmed his decision, that it was *eight times twenty-four hours;* which went forth, Dr. Waterhouse said, "like an edict." It was indeed an edict founded on intelligent observation. But Jenner was not without the active and useful support of many liberal-minded professional men in England: we have spoken of Mr. Ring and others, and the name of Mr. Dunning, of Plymouth-Dock, deserves to be especially recorded. By the judicious arrangements of Dr. Marshall and Dr. John Walker, vaccination was extensively introduced into the army and navy in Egypt, and also in Gibraltar, Minorca, Malta, and Sicily. In 1800, Sir Gilbert Blane, sanctioned by the late Earl Spencer, then First Lord of the Admiralty, effected the general introduction of the practice into the navy; a plan in which Dr. Trotter warmly co-operated. Dr. De Carro, of Vienna, whose name is so honorably known in connexion with vaccination, was the instrument of diffusing the blessings of the practice over the East; and it was greatly promoted at Constantinople and throughout the Archipelago by the exertions of Lord Elgin, (in 1802.) In Ceylon, in consequence of measures adopted by Governor North and Dr. Christie, vaccination became so general as very strongly to support the belief that if it were practised with equal industry everywhere, variolous contagion might be completely controlled. It was to Ceylon and to Sweden that Dr. Jenner used to refer when wishing to show what his discovery might accomplish.

In 1801, Jenner published his account of the Origin of the Vaccine Inoculation: a copy was sent to the Institute of France, and in the note which acknowledges it, signed by Coulomb, *Cuvier*, and Delambre, we think we detect the enlightened style of the illustrious naturalist: "to gather new truths from whatever quarter they may come, to spread them when they are useful, this is its first duty." These were surely the great views which Cuvier entertained of the duty of the institute.

When Jenner's discovery had been communicated to nearly every part of the globe, and honours had flowed in upon him from the princes and philosophers of several countries, the parliament of England took into grateful consideration the sacrifices made by him in the prosecution of his enquiries, and the labours of making them known wherever knowledge of any kind could penetrate. A previous contribution of a piece of plate was made to him in his own county of Gloucester, although Lord Berkeley, who zealously promoted it, found much supineness among the country gentlemen. It was necessary to approach parliament by a petition, which was referred to a committee, to whom evidence of the value of vaccination was given by some of the most eminent physicians and surgeons of the kingdom, and by distinguished unprofessional persons; including his late majesty, then Duke of Clarence. Dr. G. Pearson again appeared on the stage, as a witness, endeavouring to show that vaccine inoculation had been practised by others before Dr. Jenner; and that Dr. Jenner, although he promulgated the practice, knew very little about the matter, and that the experiments and observations of Dr. Woodville and himself were necessary to correct his mistakes. This, and a little more evidence to the same purport, had little effect upon the committee; except perhaps to create some misapprehension concerning the full extent of Jenner's merits. Dr. Baron sets the question in a fair light, when speaking of their report.

> "The committee, while referring to the pretensions advanced in opposition to Dr. Jenner's claims, took, on the whole, a just view of these pretensions; but they were not equally correct in estimating the real nature of his merits. They truly observed that in various parts of England an opinion was current, among the common people employed in dairies, that the cow-pox was a preventive of the small-pox. 'It appears,' they add, 'not improbable that in some very rare instances this knowledge was carried one step further, and that the cow-pox was communicated either by handling the teat or by inoculation from the animal, for the purpose, and with the intention of securing against the danger of small-pox; but the practice of which Dr. Jenner asserts himself to be the original inventor is the inoculation from one human being to another, &c.' With all deference to the committee, it must be remarked that this forms but a very small part of his claims to public gratitude and remuneration. His chief merits are, as has been already proved, of a very different description. They consist in the patient, laborious, and original investigations which enabled him to extract correct and scientific information from the most unpromising materials; to divest popular tradition of all its obscurity and uncertainty; and to elicit from vague and contradictory rumours the most accurate and valuable truths. It was not till all this was achieved that he ventured to perform his first inoculation: and, had it not been so, this most admirable and interesting experiment had been as valueless and inconsequential as the *alleged* vaccinations of Mrs. Rendall and Farmer Jesty." (Vol. i. p. 503.)

The parliament concluded by voting Dr. Jenner 10,000*l.*; a sum which in the estimation of all the respectable medical authorities of the time ought to have been doubled. But the government had at that time little to spare for discoveries calculated to preserve human life. The grant, inadequate as it assuredly was to Jenner's services, was very tardily paid. Two years after it was voted, he had not received a farthing of it. When at length paid, the fees of office, and other charges, took nearly one-tenth from the amount. But the English parliament did not forget Jenner. In 1806, after another and most complete enquiry, conducted by the College of Physicians, the further sum of 20,000*l.* was voted to him; accompanied with such expressions of esteem from the leading men of the time as were not less valuable than the grant itself. Dr. Pearson alone thought the first sum of 10,000*l.* too great; and renewed his opposition to Jenner in an Examination of the Report of the Committee. To this, as to Pearson's previous onsets, Dr. Jenner declined to reply, trusting to the goodness of his cause, to truth, and to the justice which is now done to his memory. We sincerely wish that all those who lay claim to the honour of discoveries would imitate, among his other virtues, this part of Dr. Jenner's conduct. Perhaps it is only those who are calmly confident of the truth of what they maintain who can do this: there is a restless search for novelty that may be felt by many who want the support of the belief that truth is great and will prevail.

From numerous medical societies Dr. Jenner received votes of thanks and congratulation conveyed in the warmest and most respectful language. His views were discussed in the Physical Society held at Guy's Hospital, on four successive nights. Jenner himself attended, and was greeted "with universal and rapturous applause." These and other honours from the profession were deeply gratifying to him. Whatever faults may be laid to the charge of the medical profession, the general body has never been found insensible to the merit of a really great man. The tendency of our professional studies is to generate habitual caution, but this is a habit very distinct from any unwillingness to acknowledge genuine merit and undeniable services rendered to science. We have already had occasion to observe that many of the clergy exerted themselves zealously to diffuse a knowledge of Jenner's discovery. The first English clergyman who addressed his parishioners on the subject from the pulpit was the late Rev. Dr. Booker, of Dudley; a gentleman of some literary pretensions, and much vivacity, and whose lot was the somewhat singular one of living to record in his old age the virtues of *three* beloved wives on one tombstone.

One very extraordinary claim to the priority of discovery in the instance of vaccination we cannot pass over in silence. M. Husson, who, in 1803, emphatically expressed his opinion in favour of Dr. Jenner, wrote to him in highly complimentary phrase, did him justice in a published work, and even avowed his intention of coming to England merely to be able to say that he had seen him,—this same M. Husson, in 1821, in an article in the *Dictionnaire des Sciences Médicales*, recounts among the literary and scientific thefts of England, that of the discovery of vaccination, of which he asserts the first idea was suggested by a Frenchman; and he declares that a Frenchman (M. Ribaut) conceived the idea of communicating the cow-pox to man in 1781, and mentioned it before an English physician, who communicated it to Dr. Jenner. Independently of other disproofs, it seems scarcely necessary to say more than that Jenner's enquiry commenced five years before the date assigned to this communication. M. Husson had read the Report of the Committee of the House of Commons, and quoted it freely: he must therein have seen that Dr. Jenner communicated the result of his past enquiries to Mr. E. Gardner, in 1780. It does certainly appear, however, that M. Ribaut, the protestant minister at Montpelier, had imagined from his observation of a disease resembling small-pox in sheep, and *perhaps* from what he had heard of it as it appeared in cows, that it would possibly be advantageous to inoculate man from the cow; but there is not a shadow of proof that his opinion was ever, in any way, made known to Jenner; and not the smallest reason for supposing it possible that a man of Jenner's candid noble nature would have concealed the source of such a suggestion, even if, instead of being subsequent, it had been prior to that of his own mind. But to such vexations may an honest man be exposed. Jenner had to bear many such, and among them one or two attempts, which we feel assured were fraudulent, to trace his discovery to passages in ancient Sanscrit writings. Another trial to him must have been the natural death of the Royal Jennerian Institution; which, although originating (in 1803) in the purest motives, among the most honorable men of the profession, and patronized by persons of the highest rank, and for a time most active and most useful, perished (in 1808) by some irregularities in the proceedings of some of its officers; more especially of Dr. Walker, whose instructions respecting vaccination deviated so much from those of Jenner as to compel him to bring charges against Dr. Walker, who was, however, retained in office by a majority of three, notwithstanding full proof of the charges. This was effected by one of those disgraceful acts which now and then occur in the management of public institutions; several votes (twenty) being created by money for the occasion. From that time the society, if it can be said to have had any existence, has been in no way, we believe, conducted on the principles of the great name it very improperly retained. By such events was doubtless somewhat counteracted the pleasure which Jenner must now have derived from the consciousness of his widely and rapidly extending fame; for in six years from its first promulgation the discovery of vaccine inoculation was known in every region of the world: "proceeding eastward and westward," says Dr. Baron, "it traversed the circumference of the globe."

In 1804 arose the belief, now by many entertained, and which seems to us far from groundless, that the cow-pox afforded only a temporary security. This belief, however, is considered by Dr. Baron to rest on no solid foundation. The cases brought forward by Goldson to support this objection were, he says, from the first, perceived by Jenner to be cases in which the cow-pox had never properly taken place. Mr. Dunning is honorably mentioned as setting this question in a clear point of view. Like Jenner, he had become acquainted with the benefits conferred by vaccination in consequence of his study of small-pox; and it ought ever to be borne in mind, says Dr. Baron, that Jenner's knowledge respecting small-pox was the medium of his discovery of the laws which govern the variolæ vaccinæ. It had been shown that, in the inoculation for small-pox, the mitigation of the symptoms depended much on the number of pustules produced; and that even *one* pustule could afford protection. "To moderate the eruption and to subdue the fever was the object of all the enlightened physicians who treated small-pox. Inoculation most materially aided these intentions; vaccination carried them into full effect." (Vol. ii. p. 18.)

It must, doubtless, be trying to the patience of those friends of Dr. Jenner, who have carefully kept his principles in view, to see them lost sight of and neglected, and out of this neglect questions and doubts again and again arising, in defiance of former refutations. Thirty-four years ago, some of the points which have been discussed within the last few years were, Dr. Baron observes, fully explained.

> "It was then clearly ascertained, that there were deviations from the usual course of small-pox, which were quite as common and infinitely more disastrous than those

which took place in vaccination. These deviations regarded two apparently different states of the constitution. In the one, the susceptibility of small-pox was not taken away by previous infection; while, on the other hand, some constitutions seem to be unsusceptible of small-pox infection altogether. It was found, that similar occurrences took place in the practice of vaccination; but as the security which the latter afforded was more likely to be interfered with by slight causes than the former, it became absolutely necessary that great care should be shown in watching the progress and character of the pustule. Dr. Jenner had from the beginning felt the propriety of this watchfulness; and had distinctly announced, that it was possible to propagate an affection by inoculation conveying different degrees of security, according as that affection approached to, or receded from, the full and perfect standard. He also clearly stated, that the course of the vaccine pustule might be so modified as to deprive it of its efficacy; that inoculation from such a source might communicate an inefficient protection, and that all who were thus vaccinated were more or less liable to subsequent small-pox." (Vol. ii. p. 20.)

We fear Dr. Jenner's directions for avoiding these occurrences have often been disregarded; and the character of the pustule, and the time and quality of the lymph, and the integrity of at least one pustule, and the general state of the skin, too much overlooked. Many obstacles have everywhere presented themselves to the systematic observation of the progress of the vesicle; and hence, we feel strongly assured, the cases of partial or inefficient protection have been very numerous, and the cases of complete protection proportionably diminished. The opinion expressed by Mr. Dunning, in 1806, may be probably correct, that the constitution loses its susceptibility to small-pox contagion in proportion to the perfection of the vaccine vesicle; leaving the constitution incapable of producing small-pox in its ordinary form in cases where the vesicle has been perfect, or modifying its subsequent appearance according to its approach to that perfection.

Two facts are mentioned in this part of Dr. Baron's work, which have so important a bearing on the question of second attacks of small-pox that we cannot resist inserting them in this place. One is the fact mentioned in a letter of Dr. Jenner to Mr. Dunning, that the surgeon of the South Gloucestershire militia told him he was so susceptible of the contagion of small-pox that he never attended a patient in that disease without catching it. The other is mentioned by Dr. Baron himself, in a note, (vol. ii. p. 26,) and relates to the child of a cousin of Dr. B.'s, who was vaccinated in India, apparently with success, and subsequently in England, again receiving the infection; and who was afterwards inoculated for small-pox, and took the disease, and at a still later period was exposed to its influence, and had it again. "Could a stronger illustration," remarks Dr. Baron, "be adduced of the doctrine laid down in these volumes touching the identity of the diseases in question, with relation to their protecting power?" We could ourselves give several cases of equal authenticity and weight; but we see in them no proof of the identity of the two diseases, although a striking similarity in their protective powers.

In this part of the work, also, Dr. Baron inserts a few passages from one of Dr. Jenner's journals, containing remarks which he considers subsequent investigations admirably to have confirmed the sagacity of. The following are the passages:

"The origin of small-pox is the same as that of cow-pox; and, as the *latter* was probably coeval with the brute creation, the former was only a variety springing from it.

"There are certainly more forms than one, (without considering the common variation between the confluent and distinct,) in which the small-pox appears in what is called the natural way.

"It will be enquired (if the foregoing reasoning be *à priori* correct,) in what way can the action of cow-pox, (or the equine pock,) in preventing subsequent small-pox, be reconcilable with the established laws of the animal economy? My reply is, for the reasons which I have stated on the basis of facts, that they were not *bonâ fide dissimilar* in their nature; but, on the contrary, *identical*. On this ground I gave my first book the title of 'An Inquiry into the causes and effects of the *Variolæ Vaccinæ*,' —a circumstance which has since been regarded by many as the happy foresight of a connexion which was destined, by future evidence, to become more warranted." (Vol. ii. p. 30.)

Passing over many interesting incidents in the history of vaccination, and in the personal history of Dr. Jenner, although related in so touching and beautiful a manner that to pass the latter over is not easy, we find, in the eighth chapter of the second volume, a candid summary of the facts relating to vaccination. In this chapter Dr. Baron considers the important questions, 1, of the cow-pox retaining the distinctive marks which characterized it when first discovered; and, 2, of its having lost in any degree its original prophylactic powers. Dr. Baron fully believes that the cow-pox is now what it was at the beginning. There are instances in which it has passed from one human subject to another for more than thirty years, consequently through fifteen or sixteen hundred individuals, and yet in which no degeneration has taken place. (Vol. ii. p. 244.) He nevertheless thinks it proper to employ recent lymph from the cow, when it can be procured. Dr. Baron acknowledges that, in some cases, "small-pox has occurred after the most perfect vaccination;" but he thinks such cases do not exceed in number the cases of small-pox after small-pox. Our own experience and enquiries, we must say, lead to an opposite conclusion; and we think the general opinion of the profession coincides with ours.

It is unnecessary for us to dwell on the share vaccination has had in the improvement of the health of the community, and in the diminution of the mortality among infants. Upon these points, notwithstanding the erroneous conclusions of Dr. Watt, of Glasgow, medical men are pretty well satisfied. Its benefits in protecting infirm constitutions from a disease so often followed by the worst consequences, cannot be accurately calculated; but they must be very considerable. As regards the complete extirpation of small-pox by the practice of vaccination, Dr. Baron does not feel discouraged by the variolous epidemics which have of late years appeared in Lombardy, Denmark, France, and England. In all his professional life, he has only seen one *fatal* case of small-pox after vaccination: but surely he must be well aware that many such cases are on record: indeed, there are few practical *surgeons* who have not seen some such.

For the enormous proportion of successful revaccinations in the Danish army, (2175 in 3173,) he would account by the first vaccinations being imperfect; and he would apply the same remark to the revaccinations in Prussia. This is, to say the least, a summary way of getting over the difficulty. In our next Number, we hope to place this question in its true light, in an analysis of the recent elaborate work of Dr. Heim.

This brief account of some of the events connected with the important subject of vaccination might, we have conceived, be with some advantage placed before the reader. No one, we think, can go minutely through the details which we have but imperfectly condensed, without deriving from them a conviction of the stability of the greater number, if not of every one of the points which Dr. Baron is anxious to represent as firmly established. That Jenner was the first to think of protecting the human constitution from the small-pox by the artificial introduction of the cow-pox, is not a matter which any reasonable man is now inclined to dispute. That the disease in the cow, as observed by Jenner, was but the remains of a more severe and general affection of the skin and mucous membranes of the animal; and that such an affection, in one form or other, had been prevalent at different times, both in the cow and in other animals, from a remote period; and that one form of this affection was what is commonly called the *grease* in the horse, is also, we are of opinion, supported by strong evidence. That this affection, artificially or accidentally introduced into the human system, can produce an affection of different degrees of severity, but usually of singular mildness, and confined to a solitary pearl-like pustule, is sufficiently established; and that this affection, in proportion to its completeness or modifications, protects the constitution from subsequent small-pox, in a very great proportion of cases, and for a period of years at least, if not for life, we entirely believe. That its failure to do this has, in a vast proportion of cases, arisen from inattention to the character of the cow-pox affection in those from whom lymph has been taken, and to the period or age of the lymph; and to the condition of the skin and constitution of those into whose system it was introduced by inoculation, we entertain a very strong conviction. We feel no surprise at what we consider but so many proofs of the carelessness of early vaccinators; in which, among other negligences, it is notorious that very often not one vesicle was allowed to remain undisturbed. That in the great majority of cases in which pure lymph is employed, according to its degree, there is, as a result, a protecting quality imparted, but only more or less modified, we can scarcely doubt: and, when all these circumstances have obtained due attention, we are convinced that the cases in which the constitution has not been protected against small-pox will be found to have exceeded in number the cases of well-attested second attacks of small-pox, in a much smaller proportion than is now generally believed. It remains, we think, still to be proved that, where all Dr. Jenner's directions have been attended to, the vaccine lymph has undergone the smallest deterioration. Against making any or all of these points the subject of new and most accurate experiment and observation, no objection can, of course, be entertained: on the contrary, we think that, in the present state of the public mind, such experiments are necessary; but we cannot too strongly recommend to all vaccinators, and more especially to the vaccinating officers of institutions in large towns, to consider how responsible and important a duty they are engaged in; how mischievous their labours may be, if negligently performed; how full of benefits to the whole community, if conscientiously attended to. Neither Dr. Baron, nor Dr. Jenner himself, if he were living, would object, we feel assured, to anything by which the truth might be made more clear, the public mind confirmed, and the trust in vaccination rendered so secure that the practice might become universally successful.

If such should be the result, the yet-disputed question of the actual identity of the cow-pox and small-pox becomes of minor importance. There is an evident and strong analogy between the two affections; and, generally, those who have had either are unsusceptible of the other, and those who are by natural constitution unsusceptible of one are equally unsusceptible of the other: but, as the artificial communication of the cow-pox from the lower animals to man, or from one human subject to another, has never produced a general eruption, or true small-pox, (although repeated small-pox inoculations have reduced the eruption to *one* pustule, and although Mr. M'Pherson's inoculations of the human subject from cows covered with a pustular disease produced true vaccine disease, and furnished protective virus;) and as the inoculation of the cow with small-pox matter does not appear hitherto to have been satisfactorily proved to produce cow-pox; the evidence of the identity of cow-pox and small-pox must be considered as yet incomplete, without any prejudice to vaccination. The good to be expected from it, making all possible deductions, is, the *almost complete* extirpation of small-pox; a disease which, if now let loose upon the increased population of this country, would sweep into the grave more than 50,000 persons in every year, and leave many times that number in a state of debility which would predispose them to fall victims to other diseases, probably after becoming the parents of debilitated children; for no single disease ever contributed so largely and so directly to the deterioration and destruction of human beings as small-pox. Even admitting that the first cause of small-pox may be some unknown atmospherical quality, occasionally present, its presence would be rendered nearly innocuous from the want of a number of unprotected constitutions. Cases of small-pox would still occur,—some in those previously vaccinated, some in those who had previously had small-pox; but the disease would not spread; and, unless mankind, relieved from fear, should also become utterly devoid of caution, no devastating small-pox epidemic would be heard of more.

It would be difficult for us to express the gratification with which we have read the account of Dr. Jenner's Life and Character. His biographer sustains throughout a high and becoming tone, worthy of his subject and of himself. There is no base flattery of the living, nor any unmeasured condemnation of the dead; but everything is spoken of in a manner at once calm and courageous; and the opinions given are evidently guided by the strictest love of justice. We admire Dr. Baron's affectionate but not injudicious zeal in defence of Jenner's reputation, and accept with gratitude the details into which he has not thought it unworthy of biography to enter, of the personal appearance, and even the dress, of the great discoverer of vaccination. Among the many examples of spirited and admirable narrative, in various parts of the work, we might quote the account given of Dr. Jenner's interview with the Duchess of Oldenburg and the Emperor Alexander: and in the description of Jenner's love of natural scenery, that of Barrow Hill, one of his most favorite spots, and that of the Chantry, his residence at Berkeley, are depicted with a force and beauty which every reader of sensibility will appreciate. We can readily conceive that it must have been impossible to know Jenner so intimately as it was Dr. Baron's privilege to do, and not to be charmed with his character. In going over the details compressed into the preceding pages, we have again and again been struck with admiration of his many excellent qualities: of his candour in communicating his reflections, in every stage, to all mankind; of his inexhaustible patience in explaining his views to all sorts of writers, in a correspondence, as he used to say, with the world; of his personal activity as a vaccinator; of his equanimity under all sorts of claims made to his prejudice; of his forgiveness of injuries in the case of Dr. Woodville and others; and of the acute sensibility and dignity of mind which were in him so exquisitely blended with true humility, and a philosophical calmness that not the most insolent abuse could long disturb. In these days it is scarcely credible that the opponents of a discovery, of which the probable effect was to extirpate a scourging malady, appealed to the violent prejudices of the vulgar, by means of prints representing the human visage transformed into the likeness of that of a cow. With all our knowledge of the unprincipled character of the public press, we read with surprise that the "Cow-pox Chronicle" was a publication devoted to assailing the vaccinators with unlimited periodical ribaldry; and we can scarcely believe that the public were so brutally stupid less than half a century ago, that, after a public discussion in what unworthily bore the name of the British Forum, it should be with solemn ignorance decreed that gas-lights and cow-pox were mere instances of popular credulity. Still less credible does it appear that, when his grant was under discussion in parliament, one of the arguments used against it was, that Dr. Jenner might have made a fortune by keeping the nature of his discovery secret. Heavier injustice was often done to Jenner. In the establishment of the National Vaccine Board, in 1809, he was nominally constituted director; but he was compelled to resign by finding he had no influence. Although his name was a safe passport to the traveller over Europe, and Napoleon released prisoners on his intercession, he was unable, at any time, to get any appointment for any of his relatives from our own government, or any preferment in the church for his nephew. The College of Physicians, ever consistent with its principles, overruled the generous desire of Baillie, and other ornaments of their body, and withheld the fellowship from him. By acts like these the college has incurred the mortification of seeing its honours refused by those to whose names its honours could add nothing. Neither these slights nor the wit of his enemies disturbed, although they might sometimes wound, the pure spirit of Jenner. He felt a holy reliance in the truth of his discovery, which he well knew could not long be concealed by any acts or efforts of man. To his livelier friend Mr. Ring, who had indignantly satirized the detractors from his fair reputation, he says, "Your satires against the anti-vaccinists are keen; but the keenest of all are those which you engrave *with the point of your lancet.*" Of his more serious sources of consolation Dr. Baron quotes many proofs, which could scarcely with propriety be transferred to our pages. One of them we find in the delightful *letters* appended to the second volume: writing to the Rev. Mr. Clinch, of Newfoundland, when packing up for Cheltenham, Jenner says, "Never aim, my friend, at being a public character, if you love domestic peace. But I will not repine: nay, I do not repine, but cheerfully submit, as I look upon myself as the instrument in the hands of that Power which never errs, of doing incalculable good to my fellow-creatures." These views are further alluded to in the following account, by Dr. Baron, of Jenner's general temper of mind.

> "Though the general cast of his character exhibited a happy union of great solemnity and seriousness with extraordinary playfulness, amounting at times even to the height of mirth and jocularity, yet no one ever found these latter qualities misplaced, or obtruding themselves unseasonably. Almost all the great incidents of his life tended rather to suppress them, and to keep them in the shade. In the early part of this work it has been shown, when meditating on the grand results of his vaccine experiments, how devout were his feelings. Towards the close of his life many incidents argue the increasing power of that principle. He frequently expressed his regret that mankind were so little alive to the value of vaccination. Among the last words that he addressed to me, not many days before his fatal seizure, he used this remarkable expression: 'I am not surprised that men are not thankful to me; but I wonder that they are not grateful to God for the good which he has made me the instrument of conveying to my fellow-creatures.'" (Vol. ii. p. 295.)

After several serious interruptions of his health, Dr. Jenner had an alarming attack of faintness, giddiness, and insensibility, in August 1820; he being then seventy-four years of age. This attack left no paralysis, and, after a little needful repose of his faculties, he became gradually, and in the course of a few months, as much engaged as ever in his usual occupations. His astonishing industry still continued, and his habitual cheerfulness seldom deserted him. At length, after a busy day, he retired to rest as usual, on the 24th of January, 1823. The next morning he arose at his accustomed hour, and came down stairs to his library. Not making his appearance at the breakfast-table, his servant was sent to him, and found him lying on the floor, apoplectic. There was general insensibility; and life was extinguished early on the following morning: an easy, placid, and appropriate death for him whose studious hours had ever been devoted to effecting good ends. Such was the tranquil end of Black, who died in his chair in his laboratory, like one who had fallen, after life's labours, into a sweet sleep; and such the end of Petrarch, who was found leaning on a book in his library, resting peacefully amidst the memorials of elegant letters in which the happiest part of his life had been spent. Jenner, like Cuvier and other naturalists, had contemplated the approaches to this termination of human life. Seven years before his death, in one of his many admirable letters to Mr. Moore, he mentions some warning symptoms, and adds, "I am tolerably easy about it, as at my time of life I must expect to see and feel the preparation for the extinction of vitality."

Not many days before Dr. Jenner's fatal attack, he wrote this parting statement respecting vaccination on the back of a letter, constituting perhaps the last words recorded by him on the subject. We agree with Dr. Baron, that "nothing could be more solemn, whether we consider the time or the expression of this his final judgment." And with these words we conclude.

> "My opinion of vaccination is precisely as it was when I first promulgated the discovery. It is not in the least strengthened by any event that has happened, for it could gain no strength; it is not in the least weakened, for, if the failures you speak of had not happened, the truth of my assertions respecting those coincidences which occasioned them would not have been made out." (p. 311.)

We recommend Dr. Baron's volumes, in the strongest terms, to the attention of all our readers,—young and old, medical and surgical.

## ART. XIV.

*Counter-Irritation, its Principles and Practice; illustrated by One Hundred Cases of the most painful and important Diseases effectually cured by External Applications.* By A. B. Granville, M.D., F.R.S., &c. &c. &c.—*London*, 1838. 8vo. pp. 353.

The task of noticing a work of this kind is the most disagreeable that can possibly devolve upon a reviewer. To pass it over entirely would be dishonest; to speak of it with approbation is impossible; and to speak of it as it deserves would be to employ severer language than we wish to use. If the first impressions conveyed by the general aspect of the work, and an inspection of its divisions and headings, were to be taken as truly answerable to its design and end, we should pronounce it an ingenious but unblushing advertisement of a secret remedy; and as such, out of the pale of respectable medical literature. If we hesitate to do so, it must be ascribed to the reluctance we feel to utter such a sentence on any production of the pen of a writer whose ability, education, experience, and station in society, all force upon us the admission that a purpose inconsistent with the principles commonly held to be honorable by the medical profession, must be the last he could entertain. At the same time we deplore the want of judgment betrayed by addressing such a work to the public, since we feel assured that the general opinion of the profession will be much less favorable to the intentions of the writer than that which we have professed ourselves anxious to entertain.

But it is especially necessary to remember, that this work is not addressed to the profession, and that it is solely meant for the public. If the single application of which it is designed to illustrate the extraordinary efficacy, is kept a secret from the first page to the last, we are disabled from saying that the author has been actuated by any other than a prudent desire to withhold from the unprofessional the ready mischief of a formula. No doubt it might have been better to address a few words to the profession alone, and to mention the combination of ingredients asserted to be so remarkably useful; but we have no right to expect, in any work, incompatible excellencies. A book designed to enlarge the empire of counter-irritation, by placing in the hands of all practitioners a means found more serviceable than any previously known formula of external application, would be received with gratitude as an important contribution to therapeutics. The truth it contained, although very valuable, might also be conveyed in a few words. But such is not Dr. Granville's intention. We have no right, therefore, we repeat, to say to him, your book might have been compressed into a few pages, and the exact composition of your almost infallible liniment should have been communicated. On the contrary, we are compelled to say, you have become deeply impressed with a sense of the value of counter-irritation; and by years of reflection and study have been, under Providence, instrumental in discovering a combination which had escaped previous investigation; had not been accorded to the physicians of the East, nor to the more scientific labours of Western Europe; had eluded the perspicacity of Le Fay, St. John Long, and Dr. Turnbull. You have found this application in many instances of instantaneous service, and in none without eventual benefit. You are profoundly convinced that its application requires great physiological discernment; and to be applied safely, must be applied by your own hand. You are seriously impressed with the objections entertained by the public to external modes of treatment, and see, with regret, an enormous and useless expenditure of drugs. All this you solemnly say; and you write, therefore, to the public, to show them, by examples, how superior is this new mode of external treatment, how agreeable, how efficacious; and yet you refrain from enabling them, by the use of the specific liniment you have discovered, to do themselves mischief by having indiscriminate recourse to it. The only business of a reviewer, therefore, is to see how this undertaking has been performed.

The conclusions to which Dr. Granville has arrived are thus conveniently stated in his concluding pages.

"1st. That there exists a species of external treatment, by which a great many very important disorders of the human frame hitherto considered as incurable, or difficult of cure, may be speedily and successfully cured without having recourse to internal remedies.

"2d. That although, from time immemorial, several agents of known power have been and are still employed in the external treatment of diseases; nevertheless, the various ammoniated spirituous preparations described and recommended in the present volume, have never before been offered to the public, although they possess much greater energy for carrying on and expediting that treatment.

"3d. That even where the diseases are of a nature to require the use of internal remedies, the same ammoniated spirituous preparations, externally used, will be found to be a most powerful auxiliary in hastening and securing the good effect of those remedies.

"4th. That in several instances, by the rapid and almost instantaneous manner in which they act, the ammoniated spirituous preparations have been the means of saving life from imminent danger.

"5th. That the principle on which all such external agents are supposed to act in the cure or alleviation of human maladies, has been termed COUNTER-IRRITATION; but that, in adopting such term, many of the phenomena which accompany the use of ammoniated external applications are left unexplained.

"6th and lastly. That by promoting a more general adoption of a counter-irritating or external treatment of disease, and thereby saving the constitution of patients from the pernicious effect of a polypharmacous treatment, a great service would be rendered to the public, and an important era established in the annals of practical medicine." (p. 352.)

Here we see, very perspicuously set forth, the value of counter-irritation in general; the superior value of Dr. Granville's secret preparation; its rapid action; its almost universal utility. The title-page (to turn from the last page to the first) declares that these views are illustrated by "one hundred cases effectually cured." The preface confirms the hopes of the reader, and contains what, in the peculiar circumstances of the case, is a piece of indispensable information, the exact address of the author, the number of his house, the name of his street, and that of the nearest fashionable square.

If we might presume to say a word of the first four sections out of the nine into which this work is divided, we should say that they were rather above the comprehension of the general reader: but persons more experienced than ourselves in the state of the public mind in London, consider this as a very trifling objection in medical writings. We should have considered Section V. as a better commencement, in which the field is pretty well cleared of all former counter-irritations. Not that Dr. Granville speaks illiberally of Le Fay's pomatum, "supposed to consist principally of hellebore," or of "the practice of the late Mr. St. John Long," who is "believed to have employed the milky juices of Euphorbiaceous plants, diluted with gummy mucilages;" or of the "remedies eulogized by Dr. Turnbull." But he sweeps all their applications off the stage, as being, although sometimes productive of good effects, dangerous, "because their poisonous principles may be absorbed into the system." In the cases of death occasioned by St. John Long's secret embrocation, of which the facts came to our particular knowledge, the fatal event was not attributable to this cause. We hope Dr. Granville's secret embrocation is less dangerous: no doubt it would be more scientifically applied. St. John Long derived his knowledge from his studies as a coach-painter; Dr. Granville's education has been of a different kind. They only resemble in keeping their remedies secret. Poor St. John Long probably took his secret out of the world with him; and will, we trust, find it useful in his own defence when confronted with his victims. Dr. Granville, we are assured, will impart his to the world, or at least to some well selected successor before he dies. The communication could only properly be made to one whose circumstances would place him above suspicion.

There is something ingenious in the headings of the sections of the work before us, which we cannot overlook. Section sixth, for instance, treats "of Ammoniated Counter-Irritants, *and particularly of Antidynous Lotions, a species of powerful External Applications*, capable of producing all the Phenomena of ordinary artificial Counter-Irritation, *and Something more, which the usual Counter-Irritants have hitherto failed* to produce." In this section Dr. Granville disposes of the merits of the actual cautery, boiling water, mineral acids, the usual preparations of ammonia, and at least one hundred and three preparations of cantharides, by this single observation: "that as it is impossible to fix or determine the precise dose of the powerful agent contained in them, which will enter or affect the animal system when used externally, so we cannot predicate the quantity of effect it will produce beyond what may be desired—particularly on such parts of the constitution as ought not to be affected at all, and which cannot be affected by the said agent without material injury. The utmost caution, therefore, is necessary in the use of all such counter-irritants; whereas *the ammoniated preparations I recommend* are neither obnoxious to such objections, nor do they require any such caution." Dr. G. is not answerable for the *italics*; but we conceive they do not detract from the force, or even from the meaning of the passage quoted. The truth to which they give, we venture to say, more prominence, is evidently that which Dr. Granville is very anxious to convey, as in the same page he intimates that his secret preparation produces a "peculiar and wished-for effect," without any of the drawbacks attendant upon all previously known authorities.

To this useful counter-irritant, Dr. Granville gave the name of ANTIDYNOUS, simply from its instantaneous effect in relieving pain, which he says, very unnecessarily, "is a positive fact." The removal of pain is not always, he confesses, permanent; but then, on the other hand, the

*Antidynous* application is capable of producing other striking effects. In "TIC, dependent on *organic* mischief done to the nerves or brain," the pain is, "at every attack" suspended; but "not so in Tic-douloureux, which is the consequence of sympathetic action between certain organs," for then "the relief produced by these preparations" (*the antidynous*) "is permanent." (p. 52.) This relief is almost "magically rapid." A very excellent blister is raised at the same time. Dr. Granville thinks it acts on "the nervous papillary terminations which it deadens;" this effect being continued or projected to the other extremity" (of the nerve) "in which lies the morbid pain to be relieved." (p. 54.) The case of the Countess of ——— is mentioned to illustrate this theory: she was relieved of spasms in the back in thirty-five seconds: "all pain ceased, not only in the parts nearest to the application, but also in the most remote parts of the spinal column." Of course the application was of "suitable strength;" yet it neither produced heat, "much less rubefaction," in the time mentioned. The effect might possibly be produced, Dr. G. admits, by "absorption of the volatile particles of the ingredient used," or their transmission along the sheath of the implicated nerve, after the manner of the electric fluid. But the fact is certain, "nor can there," he says, "be the smallest fear of contradiction to the assertor of it," that the antidynous lotion is a most important addition to the class of counter-irritants. Indeed, until medical men are acquainted with its composition, contradiction is plainly out of the question. But the public are not encouraged to any rashness; for Dr. G. thus impressively concludes section vi.

> "I may add, likewise, that in not a single instance of their application *under my immediate notice* have I witnessed the smallest injury done to the part; although, on a very few occasions, either through design or neglect, and not unfrequently from an inopportune and injudicious use of them *by persons unacquainted with their real power*, extensive vesication, and an abraded ulcerated surface have been produced, where no such results were required or desirable." (p. 56.)

Again we plead guilty of the *italics;* but, simply observing that no one but the inventor *can* well be acquainted with the real power of a secret medicine, we think the public can scarcely be so stupid as not to understand these warnings.

The seventh section of Dr. Granville's work is partly devoted to showing the use of natural counter-irritation, in the shape of habitual ulcers, eruptions, &c., on the danger of curing which we are disposed to think he lays an undue stress; and partly, to repeating that the antidynous lotion is the best, the safest, and the most generally successful of all external applications; and that under its use *long attendances* are completely put an end to. It cures immediately, and safely, and not unpleasantly. The fashionable physicians of Rome, skilled in all the attractive arts and imbued with the science of Greece, promised, we read, no less to the aristocratic circles of the imperial city two thousand years ago: but to this day the word of promise has been kept only to the ear. The ammoniated lotion may fulfil it to the sense. For not polypharmacy, not veratria-rubbings, not infinitesimal doses, nor any nor all other means or appliances possess all the advantages of the antidynous lotions. If Dr. Turnbull, or Dr. Quin, or indeed any other fashionable practitioners, continue to be the oracles of the aristocracy, no blame can henceforth be attributed to Dr. Granville. His candour as to the effects of his lotion is equal to his reserve as to its nature. Its composition is indeed unknown; but he does not conceal the fact that it is able to render all ordinary medical attendance unnecessary. Apothecaries may abscond, and doctors depart, and druggists drown. The painful cures which they effect in weeks, of which the records are too often read in voluminous diaries of expense, are wrought by the antidynous lotion in a given number of minutes, often without any physic given, and generally with very few fees received. It will be in vain that the profession resist this scheme: the public must adopt it with one voice. If any one doubts this, or suspects us of over-rating Dr. Granville's pretensions, let him carefully read the conclusion of the eighth section of his work.

We fear that many medical readers, however, not being acquainted with Dr. Granville's predetermination to omit the formula for an application the subject of so much promise and so much praise, may be led on with a delusive hope from chapter to chapter, only to be disappointed in the end. Dr. Granville dies, and makes no sign. Every now and then the secret seems about to be revealed, yet it is never divulged. The whole book is a long riddle, an elaborate enigma. We are acquainted with a country doctor who cures all sorts of dropsy by a secret medicine. Nothing can be more candid than our friend: we see him many times in a year, and he communicates his success as unreservedly as Dr. Granville; but he tantalizes us no less by his very communicativeness. Again and again he tells us that his medicine is perfectly safe, and very pleasant, and always successful; and ever does he wind up by saying that it consists merely of—"a few simples." Dr. Granville's book has reminded us of our country friend an hundred times. The antidynous lotion is ever held up almost in view; it is said to rubefaciate, to vesicate, and to relieve pain; it is called indifferently antidynous or ammoniated: everything is done except to declare its composition. Just when the reader's curiosity begins to flag, section nine is given, the last in the book, not to reveal the great secret, but to provoke curiosity the more. In this section the lotion is described by its properties, but still left, after the enigmatical model, undefined; as if merely to exercise the reader's wits.

As to the choice of *place* of application of the lotion, which some may consider an easy matter, Dr. Granville tells us it "must depend on our own intimate knowledge of the relation that exists between certain inward organs and certain portions of the skin." (p. 78.) As to the choice of *time*, the ingenious reader may perhaps surmise the truth, that the sooner the "antidynous or ammoniated lotions" are applied the better. Nervous headachs, which these lotions always cure, are cited in illustration of this: nevertheless, in this, and in all other cases, "a minute examination of the case itself" is required. We are glad that Dr. G. has inserted this caution; for the druggists are sure, before long, to advertise "Dr. Granville's Lotion." The *duration* of the application is seldom to be longer than from one to six or eight minutes. Less than one minute is often sufficient; but ten or twelve minutes are necessary for vesication and cauterization. Sometimes Dr. G. has deemed it necessary to bind the external application to the part, and leave it on till dry; "a result which takes place very speedily, owing to the almost ethereal volatility of the lotions." (p. 83.) In this page, in addition to this characteristic, we learn that the lotion is "colourless and transparent." The *mode of application* is by impregnating with it a piece of linen folded six or seven times, or a piece of thick coarse flannel. One or the other of these being placed on the spot chosen, is to be pressed very steadily and firmly with a thick towel doubled several times, or with an instrument, also invented by Dr. Granville for this particular purpose.

One-fifth of the volume being devoted to the account of the undescribed lotions, four-fifths are filled up with a narrative of cases; comprising every variety of suffering, from epilepsy and trismus to a severe blow on the nose; besides a "fall from a lofty haystack." These cases Dr. G. pronounces to be "interesting and attractive;" and they are presented as mere selections from nine years' practice.

It is painful to us to observe a physician of Dr. Granville's standing labouring under an evident fear that his simple assertions will not be believed; and it is to this unreasonable suspicion that we are indebted, it appears, (p. 97,) for several particulars in the record of the cases which, to say the least of them, add nothing to their authenticity. We, at least, have no desire to doubt the truth of every word in each of the cases; and we, therefore, greatly lament the manner in which they are detailed. The titles of the patients and their letters might, Dr. Granville cannot but know, be far overmatched by the learned Dr. Morison; and the *facts* they relate are often exactly such as the writers are least fitted to be the historians of. A gentleman of Dr. Granville's experience must be well aware that, in the case of the veriest quack medicine, nobody doubts the relief given: all the question is about the disease. We regret, therefore, to see that Dr. Granville's patients write to "meet the wish" of the doctor: we are suspicious when they beg to remind him that "he will remember" so and so: in short, when they write like people *desired* to write a letter that can be published. The newspapers, those great organs of the dissemination of truth, abound with letters quite as well done. Such patients always have somebody to thank for the inestimable blessing of Doctor Somebody's attendance; and, under Providence, they are always obtaining benefit, after trying all the rest of the faculty in vain.

Case iv. is that of "the Right Hon. the Countess of ——." "Her ladyship" had a pain in her chin. The lotion was applied with great accuracy to "that part of the chin which corresponds with the opening through which the mandibulo-labialis branch of the fifth nerve comes out." Common readers, for whose sake Dr. G. expressly says that his book is written, "free from technicalities and scholastic definitions," will be doubtless impressed with considerable awe on finding their simple chins divisible into so many parts. The right honorable patient was cured for a whole week: then the pain recurred, or was inclined to recur, and prevented by the application of *one* leech. Then we have one case of "a young lady of Southampton, remarkable for her general appearance of perfect health and personal attractions, lately married;" Lady Caroline ——, a young and unmarried daughter of the Earl of ——; and the young son of Lord F— L— G—, with an F— L— G— letter, full of proper acknowledgments. We have "noblemen," and "gentlemen filling considerable situations under government;" another countess "closely allied to two of the first families in this country;" and the "Duchess of ——" in her chamber; we have "a noble earl, not more conspicuous for his station in society, which he has, through a long career, enhanced by great public and private worth, than proverbially known for being at one time the victim of the severest form of nervous headach;" we have ladies of "wealthy baronets;" and soul-thrilling notes from persons of high rank, of which the purport is, "Pray do come this evening, without fail, to Stanhope street, if you cannot dine with us; for the Countess —— is suffering dreadfully from faceach," (p. 225;) and moving incidents on the occasion of Dr. G. going out of town to dine "with a nobleman," and finding "Viscountess —— in the drawing-room, dressed for dinner," with a very severe toothach; when "fortunately there was in the house a bottle of ammoniated counter-irritating lotion;" and Lord T——, when he came home to dinner, as he "occupied a ministerial station of high importance," heard the whole story of toothach and wonderful cure at once. We were wondering, when reading the beginning of this case, where the lotion was to come from; and it advantageously illustrates the propriety of keeping a bottle ready. But where is the lotion made; where to be bought? Surely it cannot be a fact—a *positive* fact, as Dr. G. says of one of his own,—that it can only be procured on application at 16, Grafton street, Berkeley square! It is impossible to believe this. Is not Dr. Granville a member of the British Medical Association, a body so jealous of physicians, and so irate against all irregular proceedings? With what face, with what decency, could bodies of men and learned associations petition for the annihilation of unfortunate quacks, if such proceedings were thought venial in regular physicians, Fellows of the Royal Society, and whose practice is amongst the nobility and gentry of the land? This would be, as with Sunday legislation, where the humble alone are stricken, and the elevated offender is secure; as poor rogues hang whilst richer ones only go abroad.

Of Dr. Granville's talents and knowledge there can be but one opinion. It is therefore as painful as it is astonishing to see him quit the sober and legitimate path which he has hitherto trod, and hazard the dreadful chance of being confounded with men every way his inferiors. That he does not do this from mere obtuseness of feeling, nor from ignorance, is too clearly revealed by the pains he has taken (by direct communications to the editors) to conciliate the favour of the medical periodical press to his *omissions*,—to call them by the gentle name which he himself employs to designate them. In these communications he professes not to consider it incumbent upon him to reveal to the profession what he regards as a valuable secret obtained by his own industry and ingenuity; and that having, in his opinion, told them, clearly and without mystification, that his lotion consists of a solution of ammonia, stronger than any in the London Pharmacopœia, he disclaims all idea of injustice when he leaves them where he himself began. He does not think it necessary to mention the exact strength of the lotion employed in any case; and he professes to have explained that the vehicle of solution is any spirituous distillation of aromatic herbs and camphor; the strength to be apportioned to the particular case. Having discovered, by experiments *on himself*, what strength of lotion to employ, he leaves others to do the same. In truth, Dr. Granville is far from being clear and explicit even to this extent; but speaks of his lotions as formed by regulating the proportions of "ammonia, spirits of wine, vegeto-aromatic spirits, camphor, and other stimulating and evaporable substances, differing from the few preparations already in use, and combined with water, or with oils, or butyraceous vehicles, or saponified into cerates." (p. 51.) We can scarcely imagine anything less lucid than this. Giving Dr. Granville all possible advantage of his plea, what is the deduction to which we are driven? that his book is not written to give the profession any information at all; but merely to tell the public that he possesses a means of giving relief, which he refrains, for his private emolument, from adding to the resources of the medical art. We do not believe that Dr. Granville approves of this kind of ethics, and we lament to see him affect to adopt it.

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## ART. XV.

*Practical and Experimental Chemistry; adapted to Arts and Manufactures.* By E. MITSCHERLICH, Professor of Chemistry, &c. *Translated from the first Portion of his Compendium, by* STEPHEN LOVE HAMMICK, M.D.—*London*, 1838. pp. 311.

THE Manual of Chemistry of the justly celebrated Berlin professor has been long known to the scientific world, and we need hardly say that it has added considerably to the reputation which he previously enjoyed. In a former Number we had occasion to notice the translation of this work into French. An English version is now before us; and we wish it were in our power to speak as favorably of it as of the excellent original; but, with every disposition to look with a lenient eye upon what is evidently a first attempt on the part of the translator, our duty compels us to say that Dr. Hammick has scarcely shown himself competent for the task. We have here about one half of the first volume; and, as the translator will doubtless proceed with the work, we trust he will receive in good part the few remarks which we feel compelled to offer.

To all acquainted with the original work, it must be obvious that, for a good translation of it, there is required an excellent knowledge of the German language and of the science of chemistry. In his preface, the translator makes an apology for harshness of style, which he ascribes to his having adhered to a literal translation. Now, however proper it may be to adhere closely to the original text, a translator is never justified in sacrificing the true sense of a passage to such an object. This Dr. Hammick has frequently done, so that in many places his style is not only harsh, but perfectly unintelligible. We shall not specify passages, because we are satisfied that all who read his volume will have no difficulty in finding them. We are surprised likewise to observe many grammatical errors: but, serious as these faults are, there are still greater on the subject of chemistry itself. He states in the preface that Reaumur's thermometer is "generally used in foreign schools." Our experience would lead us to say that the centigrade is more generally employed; and certainly, in all the standard modern works on chemistry, both in French and German, this is the thermometer used by the authors. Professor Mitscherlich, among others, employs the centigrade; but the translator has calculated nearly all the temperatures by Reaumur, in raising them to Fahrenheit's scale. Thus, phosphorus is described as melting at 160°, and the boiling point of ether is stated to be 112°; mistakes which we should have thought the slightest reflection upon the known chemical properties of these bodies would have prevented. The error extends, so far as we have seen, throughout the whole volume; and its occurrence is the more remarkable, since (at p. 42) Mitscherlich speaks of the boiling point of water as being at 100°, which Dr. Hammick translates 212°; but, in the same sentence, (p. 40, translation,) while Mitscherlich describes sulphur as boiling at 420°, (i.e. 578° F.) the translator makes the temperature 970°; an error of 400°. Probably Dr. Hammick has by this time discovered his mistake. We have no doubt that in the second volume he will guard against this; and, if he will receive from us a word of friendly advice, he will not adhere so literally to the original as to lose the author's meaning: he will be a little more careful in his grammar, and will submit the translation to the critical judgment of some friend, practically acquainted with chemistry, before he presents it to the public.

## Art. XVI.

*An Essay on the Antiquity of Hindoo Medicine, including an Introductory Lecture to the Course of Materia Medica and Therapeutics delivered at King's College.* By J. F. Royle, m.d. f.r. & l.s. &c., Professor of Materia Medica King's College.—*London*, 1837. 8vo. p. 196.

This work of Dr. Royle's exhibits considerable research in ground hitherto almost untrodden, and opens entirely new views on the origin and progress of medicine during the early ages. In this respect it is a very creditable addition to the few and unsatisfactory notices we possess on the early history of medicine. It is also particularly acceptable for the accuracy of its details on the geographical distribution of some of our most valuable drugs. The industry with which the investigation of this subject has been pursued may be appreciated from the following statement.

> "It would be tedious to relate the various measures I adopted for acquiring a knowledge of the substances at present in use in the several parts of that extensive empire (India); as well as for pointing out the sources whence a still greater variety might, if desirable, be obtained. It is enough to say, that I made collections of every thing that was procurable in their bazaars, tracing them as much as possible to the plants, animals, and countries, whence they were derived. I had the native works on Materia Medica collated by competent Hakeems and Moonshees, and the several articles arranged under the three heads of the animal, vegetable, and mineral kingdoms." (p. 25.)

From the facts which Dr. Royle has collected together, it is very evident that both the Arabians and Persians obtained from Northern India many of the articles of their materia medica; and that they also availed themselves of the written experience of the Hindoos. This we are not altogether surprised at, but we were not prepared to find that many of the plumes of originality are plucked from their more illustrious predecessors, the Greeks.

No certain period can be assigned to the Hindoo authors on materia medica; "for these, like the writers on every other subject among this extraordinary people, are without any other than their fabulous chronology, by which we may even approximatively ascertain the age in which they lived." It is, however, made very evident that medical science and the knowledge both of mineral and vegetable medicines, was in a state of considerable advancement among this people even anterior to the time of the so-called father of physic himself. That this ancient high state of Hindoo medical learning, attended as it was by the extensive disseminating of the indigenous articles of its materia medica, has not found record in the former annals of medicine, can only be explained by the total ignorance, which has prevailed in Europe, even to our own day, of the refined language of India.

> "For when the name of even the most celebrated Hindoo writer presented itself before a modern author writing expressly on the subject, it is passed over without comment or examination—'Scharak Indus, a Rhazeo citatus plane ignotus.' (Sprengel. Hist. Rei. Herb. 1. p. 234.) That a Hindoo system of medicine does exist, we know from their numerous books on all branches of that science; that some of these were written prior to the Arabs, we have shown by their being quoted in the works of the latter. How much earlier than the eighth century the principal of them were composed, we may only hope to ascertain by the progress now making in settling Indian Chronology. But in absence of this it is practicable, as I have stated, to get a conviction of the cultivation of medicine among the Hindoos at still earlier periods, from occasional notices by writers of the West; and we cannot but allow them an early knowledge of the properties of many of the valuable drugs which their country afforded them, when we see the necessarily subsequent employment of the same by the Greeks and Romans." (p. 75.)

Dr. Royle has very ingeniously brought together much collateral evidence to show the great antiquity of Hindoo medicine. He shows that the work of Dioscorides, who lived A.D. 63, is well calculated to prove how much the ancients were indebted to India and the East for their medicines. Dioscorides has however fallen into the error of stating many drugs to be the natural productions of Syria and Media, which are indigenous only to India; but this is easily explained by our knowing that the products of the East reached the West both by the Red Sea, Arabia, and Egypt; as also by the Euphrates, through the desert-surrounded Palmyra to Syria: he has therefore considered the place of export as the country actually producing the drug. Theophrastus (who lived B. C. 288,) mentions many of the spices and aromatics of India, as does also Hippocrates. But a yet more remote antiquity may be assigned to medical knowledge amongst these people, if we consider the period when translations were made from Hindoo works into other languages, though this indicates rather the age when neighbouring nations were able to appreciate the value of such works than the time when they were written. "Thus they were translated into Tamul by the Maha Rishi Aghastier, who appears to have introduced the religion as well as the science of the Hindoos into the Peninsula before the Christian era." (p. 66.)

The extent of their knowledge may be appreciated from the nature of their therapeutics:

> "But in the works of Charak and Susruta, to which, as has been proved, the ear-

liest of the Arabs had access, we find numerous metallic substances directed to be given internally; as oxide of iron, with ginger and cinnamon as a tonic; the rust in cachexy, and the sulphate in dropsy. Arsenic they prescribed not only in leprosy, and probably other cutaneous affections, but the oxide (arsenious acid) has long been with them a favorite and most efficacious remedy, in conjunction with pepper and aromatics, for the cure of intermittent fevers. Mercury appears to have been externally employed in the time of Pliny, as in his work occurs the remarkable passage (lib. xxxiii. cap. 8): 'Omnia quæ de minio in medicinæ usu traduntur, temeraria arbitror: præterquam fortassis illito capite ventreve sanguinem sistendum, dum ne quid penetret in viscera, ac vulnus attingat: aliter utendum non equidem censeam.' But the Hindoos have from very early times been in the habit of prescribing the sulphuret in the form of fumigation; and the preparations which we consider equivalent to calomel and to corrosive sublimate, in the form of pills combined with sugar, pepper, and aromatics; in a great variety of affections and to the extent of exciting salivation." (p. 45.)

It appears that the ancient Hindoos everywhere except in large towns practised all branches of the profession, as is the custom amongst the moderns. Those who practised surgery appear to have been acquainted with some of the more important operations, as lithotomy and extraction of the fœtus *ex utero*, and in their works are descriptions of no less than 127 surgical instruments.

The following observations upon theoretical and practical physicians, promulgated in one of their earliest works may be read with advantage even in the present day.

"'Having completed the indispensable course of study, practice is then to be as indispensably acquired; for he, who is versed in both, deserves to be honoured as the chief of physicians. As it is said, he who is acquainted with the science of medicine only from studying the books which treat of it, and is not well grounded in the practice also, is bewildered when called upon to attend the sick, like a coward in the day of battle. He who engages in practice, presumptuously disregarding written science, is held in no estimation by the virtuous, and merits death from the King. Both these descriptions of persons are unskilful, incompetent in their profession, possessed of only one branch of the necessary qualifications, like birds with but one wing. The medicaments, which contain within them the properties of ambrosia, are as sharp weapons, the deadly thunderbolt, or fatal poisons when administered by the ignorant; let no such man be trusted. He, who, imperfectly master of his profession, treats maladies which require either medicines or the knife, murders his patients—the vile practitioner! through his own cupidity, and the fault of the ruling authority. But he, who is conversant with both theory and practice, is competent to attain the object of his professional career, borne onwards like a war-chariot on two wheels through the ranks of the enemy.'" (p. 49.)

Amid much of the good sense with which the administration of medicines is recommended, a compliance with various ceremonies is directed which may in the present day excite a smile; after these have however been complied with, it is quaintly recommended for the patient's good that before he takes the medicine "the god of physic is to be worshipped, in the person of his deputy, the physician, who must be paid well for his services."

We recommend Dr. Royle's learned yet unpretending volume to the notice of all interested in the study of the history of medicine and the materia medica.

# PART SECOND.

# Bibliographical Notices.

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### ART. I.—*On Diseases of the Bladder.* By WM. COULSON, Surgeon. *London*, 1838. 18mo. pp. 153.

IT would be well if authors writing on scientific subjects would fulfil one of two conditions, each of which would be admitted by all as a legitimate reason for producing a book: one of these is, to communicate what is new and important; the other, to render more perspicuous or comprehensive a subject, the materials of which are either much scattered or not commonly accessible.

We cannot but entertain considerable doubts whether either of these objects has been attained in the work before us. Not only, as it appears to us, does it contain nothing positively new, but it even involves some things which before were reduced to comparative simplicity. The author's discussions are, moreover, too diffuse; and his distinctions between one disease and another are occasionally so vague and shadowy as to make it difficult to identify them. At best, the volume is but a collection of such observations and cases as have been already recorded, although they may have resulted from the author's own experience; and we have that opinion of Mr. Coulson that we wish he himself had felt that something more would be expected from him than the construction of a pocket compendium of certain of the more prominent and distressing of the diseases to which the urinary bladder is subject.

The work is divided into ten chapters, the subjects of which are—Irritability of the bladder; paralysis; acute inflammation of the mucous tunic; subacute inflammation of the same tunic; acute inflammation of the muscular coat; chronic inflammation of the same; inflammation of the peritoneal coat and of the surrounding cellular tissue; fungus hæmatodes and cancer; foreign bodies in the bladder; operation for stone; wounds and injuries of the bladder.

In the first chapter, after stating the symptoms of irritability of the bladder, and giving cases illustrative of the disease, the author observes that "the distinction between this affection and subacute inflammation of the mucous tunic is, however, easy;" giving the following as his reason for thinking so: "If," he says, "the affection has been of very long standing, the general health (if the disease be irritability) suffers but little; whereas, in chronic inflammation of the bladder, the constitutional powers sooner or later give way." We cannot quarrel with the author for entertaining this opinion; though we should have thought that experience would have taught him, as it has taught us, that it was a quicksand in practice. According to our experience, chronic inflammation of the bladder, accompanied by mucous or muco-purulent discharge although it produces a train of symptoms which are distressing to the patient, does not commonly excite very active sympathetic action in the economy; and the disease remains for many years local, and quite compatible with the continuance of life. We often meet with old men, who, for ten, fifteen, or twenty years, have suffered from chronic inflammation of the bladder, while the general health remains satisfactory: we therefore maintain, that the distinction proposed by the author cannot be safely relied on: indeed, it appears to us that the product of secretion must be regarded at present as the truest test between irritability and inflammation. We would not, however, be understood to deny that cases of chronic inflammation may commence with irritability; that is, there may be, for a certain time, no apparent change of structure: we wish merely to convey an impression that we possess no power of marking out a precise demarcation between the one affection and the other.

Again, we are satisfied that the attempt to make out a distinct series of symptoms, characteristic of inflammation, affecting separately either of the two internal tunics of the bladder, is, as a general rule in practice, utterly vain. How often has the author seen a case of pure and simple inflammation of either the mucous or muscular tunic? We would not deny the occasional existence of such a condition; but we believe it to be so very unfrequent that, if we possessed the power of discrimination, it could be only very rarely exercised, and would therefore have little practical value. We know of two cases in which inflammation affected the peritoneal coat, without attacking either of the others: we know of none in which it has been limited to the muscular tunic; and, although there are cases on record where the mucous tunic was alone affected, yet some of them are unsatisfactory.

Are we to infer, from the author's not formally noticing it, that it is his opinion that chronic inflammation of the mucous membrane of the bladder does not occur? He describes a chronic inflammation of the muscular tunic: if the mucous coat is exempt, upon what circumstance is this caprice of nature founded? We know that mucous tissue, wherever situated, is subject to chronic inflammation, and we know no law specially applicable to the bladder by which it should be excluded. On the contrary, judging from our own experience, we should say that it often occurs in that organ. We believe that our author has confounded it, on the one hand, with irritability, on the other, with subacute inflammation; and we think that several of his own cases may be adduced in support of this view of the subject: and, indeed, his own remarks imply as much; for, in another part of his work, after stating the symptoms of irritability, and giving cases illustrative of the disease, he says, "If the affection has been of very long standing, the general health (if the disease be irritability) suffers but little; whereas, in *chronic* inflammation of the bladder, the constitutional powers sooner or later give way."

Upon the remaining chapters of this work we have no remarks to offer: we have stated their general character in the beginning of this notice. In conclusion, however, we would observe, in justice to the author, that, although this little volume will, in no degree, add to his reputation, it will certainly be useful to the junior members of the profession, who may either not have access to larger treatises, or time or inclination to read them. To this class of readers one portion of Mr. Coulson's labours will be especially useful,—that which indicates the more important of the formulæ commonly employed by the best surgeons of the metropolis in the treatment of these diseases.

## Art. II.—*Manual of British Botany.* By D. C. Macreight, m.d., Fellow of the Royal College of Physicians, &c.—*London*, 1837. 12mo. p. 296.

This little work, arranged upon the natural system of De Candolle, attracted our attention as promising to supply a want increasingly felt amongst botanical students. The synopsis of Dr. Lindley, although in general strictly scientific, and devoid of obscurities of any moment to the initiated reader, requires, nevertheless, such a precision in the application of the concise analysis of orders as to frustrate, not seldom, the sedulous investigation of the student. But in the manual before us, the compiler does not appear to have lost sight of the fact, that few of his readers in this country will be accomplished adepts in the natural system: indeed his generous accommodation of himself, as the advocate of the school of De Candolle, to the dawning intelligencies of those who are just emerging out of the nursery of Linnæus, is strikingly manifested by the desire which he expresses, (*Preface*, p. 2,) that those who cannot read and distinctly pronounce his brief definitions of species, should at once go back and spell out their lessons in the dame-school of Dr. Hooker. We doubt not that in Dr. Lindley's view, such amiable liberality to the Linnæans must savour of absolute heresy; and in truth, if Dr. L. be just in his opinion (see preface to Synopsis,) that the language of this school is far from accurate: "that terms are applied in their works so vaguely and erroneously, and that they so abound in mistakes, most of which are at variance with all correct notions of the structure of plants, that they are totally unfit to be placed in the hands of students;" Dr. Macreight will have disserved those whose advantage he expressly contemplates, in referring them to the British Flora of Dr. Hooker for more diffuse information upon species.

We have examined the analytical tables of the manual in two or three of the more intricate and important orders, and we should conceive that diligent attention may lead the student in general safely to the species required, and to a correct view of both the order and genus to which it belongs. Dr. Lindley's Synopsis possesses, however, one advantage in a concise *specific character* attached to the name of each plant, which must still recommend his work to those who have not leisure or inclination for a prolix analytical examination of species. We heartily desire that Dr. Macreight's little work may find acceptance with those who admire elaborate ingenuity and faithfulness to principles. The author will have reason to note several *corrigenda* in a second edition.

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## Art. III.—*The Narrative of a Recovery from Tic Douloureux.* By the Rev. C. E. Hutchinson.—*London*, 1838. 8vo. pp. 43.

This is an interesting and even affecting narrative, drawn up from the purest motives of benevolence and conveyed in a style of elegance and simplicity, and without a particle of affectation or pretension. The author, a clergyman of the established church, is himself the subject of the case, which seems to have been a very severe one but with nothing very uncommon in its character. The disease lasted four years, with considerable intervals of ease; the site of the pain being chiefly the arm and hand. After trying almost every remedy that has been recommended in neuralgia, with at most very temporary benefit, the patient finally proceeded to Carlsbad and drank the waters for about six weeks, with partial relief. His attacks, however, continued, although in a mitigated form, until the following year (1832) when he took a course of the factitious Carlsbad at Brighton; since which time he has had no return of his disease. We have no doubt that both the true and the factitious waters were beneficial in this case; but it is by no means a strong instance of their efficacy. The author overlooks the possible influence of other agencies, especially change of climate and lapse of time; and he mentions incidentally a circumstance that occurred in the spring immediately succeeding the relief obtained in the autumn at Brighton, which circumstance might in the opinion of some explain the permanency of that relief; and had it occurred at an earlier period of the treatment, might, possibly, have stamped in the author's mind the remedy immediately preceding it, with all the preeminence which he now gives to Carlsbad. "In the spring following (he says) I lost a considerable quantity of blood from the hæmorrhoidal vessels. From this time I can date a very sensible improvement in my general health." (p. 29.) Although this pamphlet is written for non-professional readers, it will interest the medical practitioner also; and we recommend it to both classes, with confidence. It is very unlike most of the productions of lay instructors in the medical art.

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## Art. IV.—*Practical Observations on Hysteria, especially relating to its Organic Character.* By John Prichard, Member of the Royal College of Surgeons, London; and Surgeon to the Leamington Hospital.—*Leamington,* 1838. 8vo. pp. 36.

The object of this pamphlet appears to be that of showing that the nature and origin of hysteria are not so undefinable, and its treatment is not so unsatisfactory, as they are generally apprehended to be. Its author sets out by a suggestion, that it is "much oftener an organic affection than we are disposed to consider it;" and he at the same time expresses his conviction "that, in each and every case, it is to be regarded as emanating from uterine disturbance." He maintains that the disturbance may be very slight; but confidently assumes that, however slight it is, it is the cause of the hysteric phenomena. The irritation, he conceives, is transmitted to the brain, and from thence to other parts of the nervous system, "and especially to the par vagum and its important connexions." All cases of this kind he terms *cerebral* hysteria, in contradistinction to painful local affections, which he classes under the head of *neuralgic* hysteria: but he considers even these last to be generally occasioned by uterine disease or disturbance; the sympathy being in them *directly* communicated. The present publication chiefly relates to this latter form; and its principal object is to point out the proneness of such affections to pass on to the inflammatory state; particularly illustrating it by cases of long-continued pain, referrible to the situation of the cardiac orifice of the stomach, and terminating in perforating ulcer.

We confess we have some difficulty in referring all these cases to uterine irritation, and to classing them under the head of hysteria. But there are several excellent practical observations scattered through this little work. Various interesting points are touched upon in a manner which proves Mr. Prichard to be an observing and reflecting practitioner; and, if he had allowed his ideas a little more space, and been a little more careful in the arrangement of his subjects, a clearer impression would have been left on the reader's mind than the disquisition in its present form is calculated to produce.

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## ART. V.—*Lehrbuch der vergleichenden Physiologie der Haus-Saügethiere.* Von Dr. E. F. GURLT, Professor an der Königl. Thierarzneischule in Berlin. *Mit drei Kupfertafeln.*

## *Elements of the comparative Physiology of the Domestic Mammalia.* By Dr. E. F. GURLT, Professor at the Royal School of Veterinary Medicine in Berlin. *With three Plates.—Berlin,* 1837. 8vo. pp. 376.

THE Berlin school of veterinary medicine is probably one of the most complete in Europe. The buildings are situated in a large garden, and comprehend several lecture-rooms, the dissecting-rooms, workshops, stables, and an excellent museum of comparative anatomy and physiology. We believe there are at least eight teachers connected with this institution. Dr. Gurlt is professor of anatomy, and, with two assistants, superintends the dissecting-rooms; Dr. Erdman is professor of chemistry; Dr. Albers, of physiology and pathology; Hertwig lectures on the practice of medicine, and has a regular *clinique* on the diseases of animals. There are, besides, lectures on botany and natural history; and a journal, embracing all the departments of veterinary medicine, is edited by Gurlt and Hertwig. The facilities afforded by this institution, we are persuaded, have contributed materially to cherish the spirit of experimental research which distinguishes the Berlin school of medicine.

Dr. Gurlt's work affords ample evidence that he has availed himself of his opportunities: it contains a luminous compendium of physiology, with all its recent improvements; and many of the subjects are enriched by interesting original observations. The work is divided into two parts, General Physiology and Special Physiology: in the first of these the author treats successively of the *natural history* of the domestic mammalia, the *microscopic anatomy* of the tissues, *chemistry* and *physics* in their application to physiology, and concludes with a general survey of the phenomena and conditions of *life*. The domestic mammalia he refers to four orders: the horse and ass to the Solidungula; the ox, sheep, and goat to the Ruminantia; the dog and cat to the Carnivora; and the sow to the Pachydermata. Dr. Gurlt thinks it more advantageous to retain the Solidungula as a distinct order, than to include them in the Pachydermata, as is done by Cuvier.

The Anatomy of the Tissues contains all the late discoveries on this subject. The descriptions are short, distinct, and well illustrated by engravings, which we find, on the whole, to be very correct representations of the microscopic appearances. The structure of cartilage, bone, and teeth is very fully explained in conformity to the latest observations of Purkinje and Müller.

The medullary nervous tissue is described in accordance with the discoveries of Ehrenberg, as noticed in Art. VIII. of our present Number; but Dr. Gurlt agrees with those who believe that the beaded appearance presented by the smaller fibres is owing to a change which they gradually undergo after death. He uniformly found the fibres cylindrical in the valve of Vieusseus in the horse, if examined in its natural state, immediately after death; but, when kept for some time, they presented the varicose dilatations.

Special Physiology is arranged under four subdivisions: 1, the vital organs and functions; 2, the generative organs and functions; 3, motions; 4, the sentient organs and functions, including the mental manifestations.

The sudorific glands, first discovered in the human skin by Purkinje and Breschet, have been very carefully examined by Gurlt in the domestic mammalia. He gives a short account of their structure, in the Anatomy of the Tissues; but, in treating of the organs of secretion, he gives a fuller view of their peculiarities in different animals.

> "The matter of perspiration is secreted from sudorific glands, very recently discovered. In the *horse* they are found in all parts of the skin, and consist of convoluted bags. Those situated in the skin of the organs of generation are the largest, and are of a brownish colour, owing to a brownish granular matter contained in them: in all other situations they are more free from colour, and more transparent. On the bare skin under the tail they are rather larger than in any of the parts covered by hair. The ducts are uniformly straight, or but slightly serpentine. In the *ox*, the sudorific glands, though not less numerous, are of much smaller size than in the horse, and consist of simple sacs of an oval shape, with ducts which are bent, here and there, in their course. In the *sheep*, the glands are about the same size as those of the hairy parts of the skin of the horse, and each consists of a convoluted bag, gradually passing into a duct with many serpentine bends. In the *sow*, also, the glands are of the same size and structure as in the hairy parts of the skin of the horse. The duct issues from the gland abruptly, and makes few bends. In the *dog* and *cat*, it is difficult to discover the sudorific glands: they are of minute size. Each consists of a simple oblong sac, and the duct proceeds in single bendings to the surface of the skin. On the skin of the nose, and on the soles of the feet, the glands are larger than in other parts, and consist of convoluted sacs." (p. 193.)

The meibomian glands are represented as situated *exteriorly* to the cartilages of the eyelids, between the cartilages and the orbicularis palpebrarum. We were surprised to meet with this inaccuracy in an author who, in preparing his interesting paper on the hair follicles and sebaceous follicles, must have examined the roots of the cilia and the adjacent structures. The meibomian glands are situated in grooves on the *inner* surface of the cartilage, or, according to Zeiss, in channels in the substance of the cartilage. We have not been able to distinguish the thin film of cartilage described by Zeiss, but there is no doubt that the glands are nearer to the inner than to the outer surface.

The second subdivision of the Special Physiology is on Generation and Development. It is very full, contains several original observations, and will be interesting to every student of physiology. Some parts might be made more intelligible by a few additional engravings. The exochorion and its villi are represented as originating in the vitellary membrane of the ovum. This view of the origin of the chorion, first proposed by Baer, has been very generally adopted by subsequent enquirers; but it follows, from the researches of Mr. Wharton Jones on the ova of rabbits, that the vitellary membrane and its contents, at a very early period, acquire a gelatinous envelope, which is the true rudiment of the exochorion; while the vitellary membrane gradually disappears. (See *Philosophical Transactions* for 1837, p. 339.)

The nervous matter of the brain and spinal marrow is described as a *deposit* or *precipitate* ("ablagerung") from the *fluid* contained in the dorsal canal. A similar origin is ascribed to the retina, (pp. 253, 255, 257.) The only ground alleged in support of this hypothesis is, that the fluid, at first transparent, afterwards becomes turbid; but we cannot assent to a view so inconsistent with the general analogies of development on such slender evidence.

There is no notice of the membranous vestibule and semicircular tubes which float in the liquor Cotunnii of the labyrinth.

The vitreous body of the eye is erroneously described as slightly *increasing the divergency* of the incident rays of light, because its *refractive power is less* than that of the lens. This is a mistake which has frequently been made by physiological writers. Rays passing from a denser into a rarer medium always become more *convergent* when the surface of the denser medium is convex, and the surface of the *rarer medium concave*, as is the case with the limiting surface of the lens and vitreous body.

The microscopic structure of some of the organs is described in the anatomy of the tissues; of others, with the function of the organ, or under the head of Nutrition or the head of Development. In some cases the structure of the same organ is described under two or three different heads: this tends to perplex the reader, and constitutes the chief defect in the plan of the work, which in all other respects is very clearly arranged.

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## Art. VI.—*Die Nachtheile unzeitiger und uebermässiger Anwendung des Aderlasses und anderer Blutentziehung.* Von Dr. L. Wetzlar.—*Aachen*, 1837.

*On the Injurious Consequences of unnecessary and immoderate Bloodletting.* By Dr. Wetzlar.—*Aix la Chapelle*, 1837. 12mo. pp. 194.

Dr. Wetzlar seems to us to labour under an exaggerated apprehension of the ill effects of bloodletting, and that too even in cases and quantities in which its fitness is in this country considered as fully demonstrated. He has satisfied himself that there is still room for a new work on the above subject, notwithstanding the recent appearance of similar ones by his countrymen Schneider and Simon—works too which by no means minced the matter,—the one coming out under the redoubtable title of "*Hæmatomania*," and the other under the no less appalling one of "The *Vampirism* of the Nineteenth Century." Dr. W. commences his work with a comprehensive historical review of the variations of popularity to which this first of all remedies has been subjected. The new Italian school, and the active treatment of England, from Sydenham's time to our own days, are his peculiar aversion. The much talked of but little understood case of the lamented Malibran is adduced by him as triumphant evidence of the justice of his attack on English practice; a case, by the way, as to which he seems to be in possession of no accurate or authentic information, but to have rested satisfied merely with the newspaper accounts of the day; a source as to the value of which our English readers do not require to be enlightened. The school of Broussais, as it may be supposed, does not escape unscathed. Some of his anecdotes in regard to the excessive employment of leeches are sufficiently piquant; as, for example, that of a friend of the author, who being seized with the Parisian dysentery, so common to new-comers, was ordered by the

Coryphæus of the sect to put eighty leeches to the abdomen; an application at which his German prudence having revolted, pastilles of ipecacuanha along with simple mucilaginous drinks were substituted with the happy effect of speedily removing the disorder! The editor of the Drapeau blanc, less judicious or less fortunate, applied no less than five hundred leeches to his gouty finger in successive days, without having any diminution of his sufferings to congratulate himself on, even after the last of them had dropped off.

Dr. W. has some good remarks on epidemic and endemic constitutions in relation to the effects of bloodletting. In respect to the latter, he contrasts the town of Aix la Chapelle, where he is resident, with Bonn, which is at no very great distance from it, bloodletting being much worse borne at the former than at the latter, the situation of which is more open and airy.

He dwells at considerable length on the fallacious indications for venesection drawn from certain pains of the head, chest, and abdomen, and, we think, very judiciously. The following picture is not exaggerated.

> "Stitch in the side may depend on mere congestion, and that, moreover, often of a merely passive kind; thus chlorotic patients are frequently affected with a violent pain of this kind combined not unusually with cough and difficulty of breathing. But the pulse is here generally a sufficient means of diagnosis, being seldom or ever hard, whilst at the same time the effort of speaking does not exasperate the pain, which is usually seated beneath the false ribs. In spite of the chlorotic condition, many physicians bleed in such cases; and a momentary improvement may be the result, but only so far as the mere pain in the side is concerned; the general strength is depressed still farther, the stitch speedily returns, and under the repeated employment of the supposed remedy, the patient soon sinks consumptive or dropsical."

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## Art. VII.—*A Practical Compendium of the Materia Medica; with numerous Formulæ adapted for the Treatment of the Diseases of Infancy and Childhood.* By Alexander Ure, M.D., Member of the Royal College of Surgeons.—*London*, 1838. 12mo. pp. 221.

In compiling this little work, the author has followed the plan of Professor Fränkel, in his "Practical Therapeutics for the Diseases of Childhood;" and has produced a book which will be useful both to the student and practitioner. The various articles of the materia medica are here treated of principally in their application to the complaints of children; and numerous formulæ for their administration are given, derived from various sources. In a Preliminary Dissertation, general therapeutical agents are fully discussed; as bloodletting (local and general), bathing, emetics, enemata; and the modifications necessary in the employment of them, in consequence of the tender age of the patient, are well explained. Dr. Ure has enriched his work with many formulæ derived from German authors. The more important articles of the materia medica, and those more generally in use in the diseases of childhood, are very fully treated of; and all the cautions necessary in the administration of calomel, opium, and the poisonous remedies are mentioned, so as to render the work at once a safe and useful manual in the class of diseases to which it is especially devoted. We strongly recommend it to those commencing general practice, to whom the diseases of infants are often a stumbling-block.

## PART THIRD.

# Selections from the British and Foreign Journals.

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## I. THE FOREIGN JOURNALS.

### ANATOMY AND PHYSIOLOGY.

*Experiments* ON THE INTRODUCTION OF AIR INTO THE VEINS, *made by* M. AMUSSAT, *before a Commission of the Royal Academy of Medicine of Paris, consisting of* MM. Velpeau, Gerdy, Blandin, Barthélemy, Adelon, Moreau, and Bouillaud. *Reported by* M. BOUILLAUD.

[THE following are the experiments referred to in Article XII. of the present Number, page 446. Although we give an abstract of each experiment narrated, we have adopted a different arrangement from that employed by the reporter of the Commission.]

§ I. AIR ARTIFICIALLY INTRODUCED OR INJECTED.

A. *Air injected by means of a Syringe.* (Experiments 20, 23, 21, 22 of the Commission.) The animals not purposely debilitated before the experiment.

*Experiment* 1. Young bull-dog, of the average height. Laryngotomy performed previous to the experiment. Opening made into the right jugular. Venous pulse ascertained to exist. Having introduced into the wound of the vein a tube sufficiently large to prevent the exit either of air or blood, and provided two ligatures, by which the vein might be secured both above and below the wound, air was injected cautiously and gradually, by means of a moderate-sized syringe. When two-thirds only of the air had been injected, the animal was strongly agitated; the respiration became anxious and embarrassed, and the circulation accelerated; and, when all the air had been injected, a sound was heard in the heart, resembling a mixture of blowing and gurgling, ("un mélange de souffle et de glou-glou.") This sound disappeared in a minute; the respiration returned to its natural state; and, on the ligatures being applied, the animal walked quietly to its kennel. It still lives.

*Exp.* 2. Dog of ordinary size, slightly enfeebled by an experiment made ten days before. Left jugular vein opened in the middle of the neck, and a *small syringeful* of air injected into the cerebral end of the vein. Posture vertical. The dog struggled and howled: at the end of a minute he fell, and died.—*Inspection:* Bubbles of air in the mammary veins. Heart enormously distended; frothy blood in the right auricle, and in larger quantity in the right ventricle. Not a bubble of air in the left cavities of the heart, which contain a bright red blood. Venous blood in the inferior vena cava. Bubbles of air in the left jugular and facial veins, as also in the latter vein of the opposite side; (the right jugular had been obliterated in a former experiment.) Bubbles of air in the subclavian veins of both sides; also in the longitudinal sinus, and the veins which empty themselves into it, and in the superior petrosal sinuses.

*Exp.* 3. Dog of ordinary size. Injection, in half a minute, of somewhat more than fifteen cubic inches of air into the right jugular vein. The dog remains calm during the injection, and, on compressing the chest, a few bubbles of air pass out of the wound. Seventeen minutes after the first opening, the vein being again opened transversely, the air introduced itself with *lapping* sound. The wound is kept open and cleared of clots, and the air continues to enter. At the end of ten minutes from the second opening, the animal is set at liberty, and reserved for a second experiment ten days afterwards.

*Exp.* 4. Large mastiff. Opening made into the jugular vein at the middle of the neck: but little blood is lost, and no air enters. After two minutes, a small syringeful of air is introduced, without causing any agitation. A second syringe and a half full of air being introduced, the animal struggles violently, but shows no other symptom of the introduction of air. Another syringeful of air is now injected into the carotid artery. Convulsive movements and tetanic spasms, lasting some seconds, are followed by complete loss of power. Twenty-five minutes from the first opening, the animal is placed in the vertical position, and the jugular vein opened at its lower part. *Lapping* sound follows; the respiration becomes difficult; the animal falls, and discharges its fæces. In six minutes more, the respiration is convulsive and difficult; and after a few minutes the whole body is convulsed. One hour and forty-seven minutes from the commencement of the experiment, the animal was killed by opening the aorta.—*Inspection:* Serum in the pericardium; parietes of the heart flabby. Pure blood, without air, in the right cavities of the heart. Large air-bubbles in the superior vena cava. Some air-bubbles also in the veins of the brain and in the mammary veins.

[These last two experiments are mixed, and therefore inconclusive. In both the air was first injected, and then allowed to enter spontaneously: in the last the air was injected not only into the jugular vein, but also into the carotid artery.]

B. *Air injected by insufflation through a Tube.* (Experiments 24, 25, 26, 38, 39, 40 of the Commission.) The animals (dogs, horses, and one mule,) not purposely debilitated.

*Exp.* 1. Dog of moderate size. Air blown through a tube into the thoracic end of the right jugular vein. At conclusion of experiment, cries, struggles, panting, discharge of urine. Death in two minutes.—*Inspection:* Heart extremely distended; its right cavities filled with frothy blood; its left cavities quite free from it.

*Exp.* 2. Young dog of small size. Air blown gently into the thoracic end of the right jugular vein. Directly after the injection of air, cries, slight struggles, discharge of urine. Death at the end of one minute.—*Inspection:* Heart greatly distended; frothy blood in the right cavities; left cavities containing common blood.

*Exp.* 3. Small dog. Air blown gently into the thoracic end of the jugular vein. Immediately after, cries, violent struggles, and discharge of urine. Injection repeated at the end of three minutes, and followed by cries, falling of the head, and convulsive respiration. Death in half a minute from the second injection, and in three and a half minutes from the first.—*Inspection:* Strong contractions of the right auricle, which is distended, as well as the right ventricle. From the latter escape air and frothy blood. Frothy blood in the left cavities of the heart and in the aorta.

From these three experiments it appears that the insufflation of air from the chest of a man causes almost instantaneous death. In one instance, death ensued in one minute; in another, in two minutes; and in the third experiment, in three and a half minutes from the first introduction of air. The first experiment of the series (A, § I.) is the only one which admits of comparison with the three just detailed. In that experiment little inconvenience followed the injection from a syringe of a moderate quantity of air. In all the experiments in which air was introduced from the mouth, death was the prompt result. It is *probable*, then, that air which has been breathed proves more speedily and certainly fatal than air which has not been breathed. A mixed experiment, (No. 21 of the Commission,) in which air was first injected by a syringe, and then allowed to find its way into the vein, and this without fatal consequences, strengthens this probability.

As regards the post-mortem appearances, it will be remarked that air and frothy blood were found only in the right cavities of the heart in the first two experiments of the present series; and in these death took place after an interval of one and two minutes respectively. In the third experiment, death took place at the end of three minutes and a half, and frothy blood was found in both cavities of the left side of the heart and in the commencement of the aorta.

*Exp.* 4. Old horse. Left jugular vein opened at the middle of the neck. On introducing the tube, some air having entered, insufflation was practised immediately. After the second insufflation, the animal falls, struggles violently, and makes fruitless attempts to rise; frequent respiration, groans, convulsions. Death in six and a half minutes from the commencement of the experiment.—*Inspection.* Heart enormously distended. Much froth in the right ventricle, on the surface of a large clot. Frothy blood in the coronary artery and aorta. Blood, with very little air, in the left cavities. The entire venous system, not excepting the portal veins, containing a large quantity of air.

*Exp.* 5. Mare. Air twice breathed through a tube into the jugular. At the end of the second insufflation, the animal falls as if struck by a club, neighs, and expires between five and six minutes from the beginning of the experiment.—*Inspection.* Distension of the right cavities of the heart. Mixture of pure and frothy blood in these cavities; bloody froth in the pulmonary artery; a fine red froth in the left ventricle. Bubbles of air in the coronary veins; mixture of blood and froth in the aorta. Numerous bubbles of air in the veins of the convexity of the brain, and infiltration of air in the sub-arachnoid cellular tissue.

*Exp.* 6. Mule. Air breathed forcibly into the thoracic end of the divided jugular. Two insufflations; after the second of which the animal falls, rises again, totters, and again falls; struggles, and grows rigid. Four minutes from the commencement of the experiment, groans and convulsive respiration. Death in five minutes. —*Inspection.* Extreme distension of right cavities of the heart; frothy blood in all its cavities. Bubbles of air in the veins of the convex surface of the cerebrum, in those of the corpus callosum, and under the arachnoid.

The experiments just reported agree with similar experiments made on dogs. In dogs, death took place in from one to three and a half minutes; in horses, from five to six and a half minutes. The existence of air in almost all parts of the venous system, and more especially in the veins of the cerebrum in the horse, forms the chief difference between the two sets of experiments referred to.

### § II. AIR SPONTANEOUSLY INTRODUCED.

#### A. POSITION HORIZONTAL.

(*a.*) *Animals not debilitated before the experiment.* Experiments 1, 2, 3, 4, 5, (dogs,) 30, 31, 32, 33, (horses,) of the Commission.

*Exp.* 1. Dog, the size of a fox. Oblique incision made into the upper part of the right jugular vein. Venous pulse in the lower third of the vein, but not at the point of incision. No introduction of air. A puncture being now made in that part of the vein where the venous pulse existed, about one inch from the subclavicular vein,—introduction of air, with a sound resembling the *lapping* of a dog or cat. This sound exists chiefly during inspiration, but at certain moments corresponds with the *movements* of the heart. After ten minutes, the animal still living, the chest was opened, when the right auricle and ventricle were very greatly distended, but still contracting. After losing about three ounces of blood the animal expired, about twenty-seven minutes after the first incision. Death took place gradually, without loss of arterial blood, five or six minutes after opening the chest and pericardium.—*Inspection.* Frothy blood, of the colour of the lees of wine, in the right auricle. Black blood, without air, in the left auricle; left ventricle empty. Frothy blood in the pulmonary artery and its branches. Lungs healthy, but containing little blood. Escape of air-bubbles from the subclavicular and jugular veins.

*Exp.* 2. The sounds of the heart having been ascertained to be precipitated, but normal, an opening was made into the right jugular vein. Immediately afterwards the animal sighed, and air entered the open vein with a *lapping* sound, and the escape of frothy black blood. On auscultation of the heart, a double *bruit de souffle*, distinct from that just described, was heard. After an interval of three minutes, the same *bruit humide*, or splashing *(gargouillement)*, which existed at the orifice of the vein was heard in the heart. After the lapse of another five minutes, the introduction of air was synchronous with the inspiration, and its exit with the expiration; but, at the end of another minute and a half, the entrance and exit of the air are synchronous with the pulsations of the heart. Still later, the inspirations are said to be very feeble; a double *bruit de souffle*, resembling that which takes place in certain valvular affections, is heard, and continues nine minutes after the closure of the vein by the finger. This *bruit de souffle* ascertained to depend on the heart, and not on the respiratory movements. The pressure of the stethoscope causes reflux of the blood through the orifice of the vein. The same reflux takes place even when the respiration has become ventral. Quantity of blood lost, about six or eight ounces. The animal is left to itself thirty-eight minutes after the beginning of the experiment. After the lapse of forty-seven minutes, it rises, stretches itself on its belly, raises the head, makes a few steps, and falls. Death four and a half days after the experiment.—*Inspection* thirty-six hours after death. No sign of decomposition. Purulent effusion in the pleuræ, but especially in the right. Right cavities of the heart distended. A digital depression exists at the union of the two ventricles, especially towards the apex. Right auricle and ventricle entirely filled by a firm clot, partly black but chiefly amber coloured, which extends into the two venæ cavæ and into the pulmonary artery; left ventricle contains a soft black clot. The left auricle contains a somewhat firm reddish-grey clot, which extends into the pulmonary veins, without any trace of inflammation. Tricuspid valve slightly red and thickened; mitral valve healthy. No uncommon appearances in the aorta; no emphysema in the lungs. Some of the lobules of the middle lobe of the right lung inflamed, and a puriform mucus exudes from the bronchi. In the opinion of the Commission, the dog did not die from the introduction of air, but in consequence of an accidental inflammation.

*Exp.* 3. Moderate sized bitch. Sounds of the heart before the experiment, distinct and without souffle. Left jugular vein opened at the lower part, where the venous pulse exists. Air enters with a slight *lapping* sound: about a minute after, struggles; anxiety; high, frequent, and convulsive respiration. Three minutes and ten seconds after, *lapping* sound more distinct than at first; slight *bruit humide*. After five minutes, the orifice of the vein being closed with the finger, apparent death, with persistence of the movements of the heart. The finger being removed, the respiration is gradually reestablished, the eyes brighten, and the animal revives; the blood is then removed from the wound, and the orifice dilated. The same introduction of air and exit of frothy blood take place; the respiration becomes somewhat embarrassed, and the animal is in the same state as before the first application of the finger. After twelve minutes, the finger is replaced on the orifice of the vein: the respiration becomes feeble, a sudden convulsive extension of the limbs takes place, the eyes are distorted, and the respiration ceases; the animal drops the head convulsively, the urine and fæces are passed involuntarily, and neither respiration nor contraction of the heart can be heard. Death five or six minutes after the second application of the finger, and a quarter of an hour after the opening of the vein.—*Inspection*. Slight vermicular movement of the heart. Right cavities so distended as to appear at least thrice the size of the left; vena cava also distended; bubbles of air in the cardiac veins. Pressure on the heart causes reflux of frothy blood into the sub-clavicular vein. On opening the right auricle, an escape of frothy blood, of a somewhat ochry colour; the left ventricle filled with a perfect froth. Small quantity of blood, without air, in the left cavities of the heart. Slight emphysema, especially on the anterior edge of the lungs. Right auriculo-ventricular opening nearly twice the size of the left.

*Exp.* 4. Dog of moderate size. Opening made into the jugular at about an inch from the chest. Abundant hemorrhage ensued, with the introduction of some bubbles of air during inspiration. The fore-leg having been separated from the chest, the aperture became gaping, and air immediately entered the vein with a noise. The opening was then closed, and the respiration of the animal being slightly affected, he was left to himself. The animal survived, and after ten days was submitted to another experiment.

*Exp.* 5. Young spaniel bitch, of ordinary size. Opening made into the jugular vein at its lower part. Two minutes after, the *lapping* sound heard. After another minute, struggles, especially when placed in the vertical position. Respiration calm. Large bubbles of air enter and pass out by the wound during the respiratory movements. At the end of three minutes, respiration very anxious and frequent. The animal being now left to itself, and the vein no longer patent, no more air appears to enter, though some blood still flows from the wound. On examining the heart, a distant and prolonged *bruit de râpe* is heard. Auscultation, repeated at the end of three minutes, discovers the same sound. At eleven minutes from the beginning of the experiment, respiration returned to its normal state. After another eight minutes, the *bruit de râpe* less distinct. The animal still loses blood. Twenty-three minutes from the commencement of the experiment, the animal utters a cry, is seized with convulsions, discharges his fæces, and falls into a state of apparent death. The vein being now secured, and cold aspersions employed, the respiration is slightly reestablished. Death in half an hour from the beginning of the experiment.—*Inspection.* Enormous distension of right cavities of the heart, which contain a large quantity of frothy blood.

*Exp.* 6. Old horse. Left jugular opened in the middle of the neck; no air introduced. Another opening made lower down, about eight inches from the sternum: immediate introduction of air, but in small quantity. A third opening having been made at the lowest part of the neck, the air entered immediately, with a very loud sound. Six minutes after the first opening, large bubbles of air pass from the wound. Five minutes after the third opening, convulsive respiration, panting, suffering. Ten minutes after, respiration more and more frequent. Fifteen minutes after, defecation, moaning, anxious respiration. Nineteen minutes after, the animal stamps and stumbles. Twenty minutes after, the legs bend under him; he falls, and passes a white milky urine. Twenty-nine minutes after, the head drops, and the respiration becomes irregular and plaintive. Death in thirty-five minutes from the commencement.—*Inspection.* Heart distended. Coronary veins containing air; froth in the right auricle and ventricle; in the latter, black fluid blood also. Bubbles of air adhere to the walls of the left ventricle. Air-bubbles in the sinuses of the dura mater and in the veins of the convexity of the cerebrum. Clot, with froth on its surface, in the superior longitudinal sinus.

*Exp.* 7. Very small old horse. Left jugular vein opened at its lower part: immediate entrance of air with loud noise; accelerated breathing. The animal, being walked about, lets fall his head, trembles, trips, recovers himself, and falls again, three minutes after the opening of the vein. In this position, he pants, stretches himself out, neighs repeatedly, is seized with convulsions, his nostrils expand, his eyes open widely, and he dies fourteen minutes from the commencement of the experiment.—*Inspection.* Much froth in the right cavities of the heart, and a clot in the right ventricle. No air in the left cavities nor in the coronary veins. Bubbles of air in the veins of the thorax, the inferior vena cava, the femoral veins, those of the convexity of the brain, and in the superior longitudinal sinus. Lungs collapsed; no emphysema.

*Exp.* 8. A mare. An opening, eight lines in length, having been made in the inferior part of the right jugular vein, the sound indicative of the entrance of air into the vein is heard. After two minutes, the respiration embarrassed. After ten minutes, the animal falls on its knees, and makes vain efforts to rise; the head is supported against a tree, and the neck flexed. Respiration more and more frequent and difficult. Twelve minutes after, blood no longer flows from the upper extremity of the vein; the position of the neck also prevents the admission of air. For an hour and a half the animal is left to itself; the respiration is difficult, the body is covered by sweat, and he groans from time to time. A clot which stopped the opening of the vein having been removed, and the neck extended, air again enters. After a few minutes, fixed and haggard eye, very frequent convulsive respiration, tongue protruded from the mouth, extension of the head and limbs. Death one hour and forty-four minutes after the opening of the vein.—*Inspection*. Extreme distension of the right cavities of the heart with a mixture of liquid and frothy blood. No air in the left cavities. Bubbles of air in the veins of the convex surface of the brain. Lungs collapsed; no emphysema.

*Exp.* 9. Mare, fifteen years old. Right jugular vein opened at its lower part. Entrance of air with sounds of gurgling and *glou-glou.* Aperture widely open. After a minute, the animal sighs, the respiration becomes panting, and the upper lip is extended and contracted as in yawning. After fifteen minutes, the animal lies down, rises directly, and again lies down; the respiration becomes more panting, the limbs are extended, the head raised; he struggles, stiffens, yawns, and sighs. Death twenty-eight minutes after the opening of the vein.—*Inspection.* Distension of the right cavities of the heart. Some froth in right auricle; a considerable quantity in right ventricle, as also in the left ventricle. Bubbles of air in large numbers in the veins of the cerebrum and cerebellum, and infiltrated under the arachnoid. Lungs sound.

(*b.*) *Animals debilitated before the experiment.* Experiments 6, 7, 8, 9, 10, 34, 35, 36, 37, of the Commission: the first five on dogs, the last four on horses.

*Exp.* 1. Large dog, weakened by previous loss of blood. Right jugular opened at the lower part. The opening, which was from two to three lines in diameter, being kept extended, the lapping sound was repeatedly and very distinctly heard; the animal stiffens, and respires convulsively. Death at the end of four and a half minutes.—*Inspection.* On opening the pericardium, right cavities observed to be greatly distended; feeble contractions in the auricle, lasting thirty-eight minutes. A reddish froth, resembling lees of wine beat up with air, found in the right auricle and ventricle. There was also free air in both cavities, and a recent clot in the right ventricle. Left ventricle free from blood and air. Frothy blood in pulmonary artery, but not in the pulmonary veins. Bubbles of air in the right common iliac vein.

*Exp.* 2. Dog enfeebled by opening the left crural artery. Pulse, before the loss of blood, from eighty-four to eighty-eight; after the loss of blood, ninety-six to one hundred. The right subclavicular vein being exposed, it is seen to swell during expiration, and become empty during inspiration; but, the breathing becoming tranquil, a movement of flux and reflux is observed, synchronous with the pulsations of the heart. A small opening, a line in diameter, being made into the vein, no lapping sound is heard; but the breathing becomes quickened and embarrassed. Soon after the opening is made, however, some bubbles of air, mixed with blood, escape from the wound. After the lapse of two minutes, the leg being extended, and the wound held open, the *lapping* sound is heard, and the respiration becomes more embarrassed. The lapping sound is synchronous with the inspiration. Five minutes after the opening, the inspiration has become calmer, in spite of the continual introduction of air. After eight minutes, the leg is placed in its natural position, and the wound closed. Auscultation reveals a distinct *bruit de souffle*, accompanied by a *bruit humide.* The animal moans, extends the head, its limbs are convulsed, and its inspirations are panting. Death in twenty-five minutes.—*Inspection,* a quarter of an hour after death. Distension of right cavities of the heart. Air in the right jugular. No contractions of right auricle: on opening this cavity, air and frothy blood escape. Right ventricle contains still more frothy blood, mixed with clots. The left cavities contain an unusually large quantity of blood, partly in clots and free from air. No emphysema of the lungs.

*Exp.* 3. Dog of average size, weakened by loss of blood from the carotid artery. Opening made into the lower part of the jugular vein: air enters with *lapping* sound. After a minute, quickened breathing. After two minutes, cries, struggles, discharge of urine, dropping of the head, convulsive trembling, especially of the right side. At the end of seven minutes, the animal tries to rise, succeeds after the lapse of twelve minutes more, and walks about.

*Exp.* 4. Bitch above the average size, enfeebled by loss of blood. Axillary vein opened. No venous pulse, and no movement corresponding to the respiration observed. The orifice being put on the stretch, the *lapping* sound is heard, and frothy blood issues from the wound; at the same time the breathing is quickened and embarrassed. Sound of the heart imperceptible. Nine minutes after the opening of the vein, the *lapping* sound ceases, though the clots are cleared away with a sponge. Respiration still frequent, but less difficult. At the end of twelve minutes the animal is left to itself, and is to all appearance quite well. The dog is then killed by making two large openings into the thorax.—*Inspection.* Heart contracts by a sort of vermicular movement. Through the walls of that viscus a sort of whirlpool can be distinguished, caused by a mixture of small bubbles of gas with the blood. On opening the right cavities there escapes liquid blood, with a few extremely minute air-bubbles. Small quantity of frothy blood in right ventricle, and still more in the pulmonary artery. Nothing remarkable in other organs.

*Exp.* 5. Spaniel of an ordinary size. The crico-thyroidean membrane and the sub-hyoidean muscles divided previous to the experiment. Sounds of the heart accelerated, but without souffle. Animal debilitated by the abstraction of blood from the right crural vein. The brachial vein having been exposed and opened at the lower margin of the axilla, was found to be set in motion, either by the venous pulse or by the subjacent artery. No introduction of air. The jugular vein is now opened at the part where the venous pulse exists: immediately afterwards, *lapping* sound, and at the end of half a minute the sounds of the heart indicate the presence of air in its cavities. In another half minute, anxiety, struggles. Three minutes after the opening of the jugular, the vein is secured. No longer any *glou-glou* in the heart. The animal left to himself. Forty-eight hours afterwards he is found enfeebled, but can support himself, and walk without much difficulty. He is again left to himself, and killed on the eighth day after the experiment.—*Inspection.* Right auricle greatly distended: it contains some bubbles of air. Bubbles of air also found in the right ventricle. Blackish clots of blood in the left cavities of the heart. The posterior mediastinum distended with air at its upper part; as also the axilla. Bubbles of air in the internal mammary, inferior vena cava, renal, and crural veins; and in the aorta, left iliac, and crural arteries.

*Exp.* 6. Horse debilitated by abstraction of blood from the carotid artery. Right jugular opened at the lower part of the neck. The sound indicative of the introduction of air is immediately heard, the breathing becomes accelerated, and after two minutes the animal falls, struggles violently, trembles, raises the head slightly, and again lets it fall. Some minutes afterwards, groans, discharge of urine. Death nine minutes after the opening of the vein.—*Inspection.* On dividing the skin covering the chest, escape of a large quantity of frothy blood from the veins, and of red blood from the arteries. Much froth and liquid blood in the right cavities of the heart, a large quantity of blood without intermixture of air in the left cavities. A clot in the aorta, with bubbles of air here and there on its surface. Coronary arteries and veins contain bubbles of air. Air-bubbles also in the femoral vessels, and in the inferior vena cava. Air also found in the veins of the convexity of the brain and in one of the ventricles. Lungs collapsed and flattened, free from emphysema.

*Exp.* 7. Horse enfeebled by loss of blood from the carotid. Jugular vein opened at the lower part of the neck, and secured by a ligature above the wound. Immediate entrance of air with usual sound. At the end of two minutes respiration accelerated. Two minutes later the animal is led about. The respiration soon becomes more frequent, he trips, falls on his knees, rests a moment in that position, and then falls on his side. At the end of eleven minutes, the eyes are closed, the nostrils largely open, the respiration difficult, and the limbs convulsed. Death twelve or thirteen minutes after the opening of the vein.—*Inspection.* Great distension of the right cavities of the heart by clots and frothy blood. No air in the left cavities. Air-bubbles in the veins of the convex surface of the cerebrum and in the superior longitudinal sinus. Lungs collapsed, and free from emphysema.

*Exp.* 8. Horse, debilitated by the abstraction of from three to four pounds of blood from the carotid. Large opening into the lower extremity of the jugular, followed by *glou-glou* and *gargouillement*. At the end of two or three minutes, accelerated and embarrassed respiration, sighing, and discharge of urine. At the end of eight minutes, the animal stumbles, as if drunk. Half a minute later, he falls, sighs, groans, and struggles. A minute and a half later, the groans become more plaintive, and the neck and forelegs are extended convulsively. Death thirteen minutes after first introduction of air.—*Inspection*. Right cavities of the heart distended by a large quantity of frothy blood. Air-bubbles on the walls of the left cavities, and in great numbers in the coronary veins. Bloody froth, resembling whipped cream tinged of a rose colour, in the pulmonary artery. Bubbles of air in the aorta. The surface of the cerebrum, cerebellum, and medulla oblongata studded with innumerable bubbles of air; some of which are deposited beneath the arachnoid, others contained in the veins. Nothing remarkable in the lungs.

*Exp*. 9. Horse, debilitated by abstraction of about four pounds of blood from the jugular. That vein being then opened at its lower part, the air enters with a gurgling and pulsating sound. At the end of a minute, embarrassed respiration. Eight minutes after, the animal stumbles, falls on its fore-knees, and then on its side. At the end of another six minutes, he extends his limbs convulsively, and dies sixteen minutes from the opening of the vein.—*Inspection*. Right cavities of the heart distended by bloody froth; the same bloody froth in the pulmonary artery; frothy blood in the coronary arteries and veins. Froth, without blood, in the left cavities; liquid blood and air-bubbles in the aorta. Air-bubbles, also, in the veins of the cerebrum, and in moderate quantity under the arachnoid.

### B. POSITION VERTICAL.

(*a*.) *Animals not debilitated before the experiment*. Experiments 11, 12, 13, 14 of the Commission.

*Exp*. 1. A dog, of moderate size, meager and ill. Opening made into the lower part of the jugular. *Lapping* sound, much more distinct during inspiration. The animal being now placed in the vertical position, utters cries, and struggles. A minute afterwards, respiratory movements suspended; the pulsations of the heart, however, can be felt, and the flux and reflux take place in the vein. The animal makes two or three inspirations and expirations, during which blood issues from the wound. Death three minutes from the opening of the vein.—*Inspection*. Right auricle and ventricle filled with frothy blood. No frothy blood in the inferior cava. Numerous bubbles of air in the left jugular, and in the other veins of the neck as far as the facial.

*Exp*. 2. Healthy spaniel, of ordinary size. The right jugular vein opened. Introduction of air, at first without *lapping* sound; which, however, takes place in half a minute. The wound being filled with blood, the sound ceases. The animal is now placed in a vertical position: at the end of nine minutes, respiration frequent and agitated. The animal being now replaced in the horizontal posture, a large quantity of frothy blood issues from the vein. The hemorrhage is now arrested. The heart's beat irregular and intermittent; respiration convulsive, at one time rapid, at another slow, and even suspended. The animal is again placed in the vertical position, seventeen minutes after the opening of the vein; the aperture is freed from clots, and the air again enters with a *lapping* sound, still more distinct than before. The animal now seems to suffer more; the respiration is become stertorous. Twenty-one minutes from the commencement of the experiment, the animal is left to itself: he walks about, and discharges his fæces. At the end of thirty-six minutes, the wound is closed; the animal having lost but a moderate quantity of blood. At the end of a few seconds he falls on his side, breathing anxiously. At the end of some minutes he rises, and is then left to himself. At the end of three days the dog is killed, by dividing the aorta and vena cava with a bistoury.—*Inspection*. Heart flabby, especially the right side: no air in its cavities. No air in the jugular, coronary, and mammary veins; nor in the sinuses and veins of the brain.

[N.B. The death and dissection of this animal are put down as a separate experiment, with a view, probably, of completing the number of forty experiments. We have thrown these two, No. 12 and 13 of the Commission, into one.]

*Exp.* 3. Large dog. Opening made into right jugular vein, and followed instantly by the introduction of air. The animal being immediately placed in the vertical position, utters cries, loses its senses, and discharges its urine. It is again placed in the horizontal posture, and the mouth of the vein closed: the animal now remains motionless, with its eyes closed and its respiration suspended, having all the signs of apparent death. The ligature is now removed, and a large quantity of blood flows from the wound. After half a minute the respiration returns, at first convulsive, then calm, and the animal recovers. Seven minutes from the beginning of the experiment, the dog being thus recovered, is again placed in a vertical position. At the end of a minute the air again enters with a *lapping* sound: the respiration becomes very difficult, the animal moans, is in a state of complete exhaustion, and discharges its urine and fæces. In four minutes more, no sign of life. The animal being again placed in the horizontal posture, frothy blood issues from the vein, and the breathing is reestablished; with convulsions, first of the diaphragm, then of the whole body, the heart beats slowly but regularly. Death, sixteen minutes from beginning of experiment.—*Inspection.* Frothy blood, in large quantity, in the right cavities of the heart. Small quantity of red blood, without air, in the left cavities. Air in the vein of the cerebrum, and in the superior longitudinal and lateral sinuses. No air in the left jugular.

*Exp.* 4. Dog, thin and ill. After having applied a ligature to the right jugular vein, an opening is made into it near the thorax: air immediately enters with a *lapping* sound, and the breathing is accelerated. During expiration, large bubbles of air pass out from the aperture. The animal is now placed in a vertical position: the air continues to enter, and the respiration becomes quicker and quicker. Ten minutes after the opening made into the vein, cries, struggles, discharge of fæces. On examining the heart, *bruit de souffle.* After twenty-one minutes, the opening of the vein being closed, the respiration becomes more and more irregular and convulsive. Death, twenty-four and twenty-five minutes from commencement of experiment.—*Inspection.* Right cavities of the heart greatly distended by blood, in great part liquid, and less frothy than in other experiments. Origin of pulmonary artery filled with frothy blood. No air in the left cavities.

(*b.*) *Animal debilitated before the experiment.* (Exp. 16 of the Commission.)

*Exp.* Dog enfeebled by a previous experiment. Position horizontal. Large opening made into the lower part of the jugular vein, followed immediately by *bruit de lapement;* no struggles. At the end of half a minute, the animal, being placed in a vertical position, becomes strongly agitated, breathes with difficulty, and discharges its urine. After the lapse of two minutes more, the dog grows stiff, faints, lets fall the head, and again discharges his urine. He is again placed in the horizontal posture, and raised at the end of half a minute. The eye is now fixed and motionless; scarcely any sign of life exists; tetanic spasms, dropping of the head. Death at the end of about six minutes.—*Inspection.* Right cavities of the heart distended. Right auricle contains a clot of blood, filled with air-bubbles. A considerable quantity of frothy blood in the right ventricle. No air in the mammary veins.

### § III. EXPERIMENTS SHOWING THE MODIFYING AND REMEDIAL EFFECTS OF COMPRESSION OF THE CAVITIES, AND OF SUCTION, ON THE INTRODUCTION OF AIR INTO THE VEINS.

*Exp.* 1. Small dog. The jugular being exposed, and the thorax compressed, the vein is found to swell at each compression. During a deep inspiration the vein becomes empty and flattened, and loses the colour due to the blood it contained. The jugular vein is now opened, but without effect; on enlarging the opening, however, a *lapping* sound is heard, and the animal struggles and becomes anxious. During a deep inspiration the *lapping* sound is heard more distinctly, and air-bubbles escape by the opening of the vein. Three minutes after making the opening into the vein the aperture is closed, and the heart exposed. When the heart is untouched, nothing escapes by the opening of the vein, but on compressing the thorax blood, mixed with air-bubbles, escapes from the wound. Death in ten minutes.—*Inspection.* The heart and vessels present the usual appearances, and frothy blood escapes from the right cavities of the heart.

*Exp.* 2. Shepherd's dog. The abdomen and chest are tightly bandaged, and the bandage secured by a tape; the right jugular vein is then opened in the situation of the venous pulse: a distinct *lapping* sound immediately follows, and after some seconds the respiration becomes difficult. At the end of two minutes the animal is placed in the vertical position, and the *lapping* sound becomes more distinct, and more frequent. The respiration is accelerated; anxiety, cries, and groans follow, and the head drops. The animal being now placed on the back, frothy blood issues from the wound. After six minutes the bandage is removed, and the *lapping* sound continues. The animal is now left to itself, but does not rise; being placed on the ground he stands up, walks, and again falls. After fifteen minutes, cries, convulsions, howlings, evacuation of the urine and fæces, dropping of the head, tongue hanging from the mouth. Blood, mixed with some air-bubbles, escapes during all this time. The quantity of blood lost is so considerable as to lead some of the assistants to attribute the death, which took place seventeen minutes from the commencement of the experiment, to this cause.—*Inspection* immediately after death. Distension of the right cavities of the heart with frothy blood. Some air-bubbles in the left cavities, air in the mammary, crural, venæ cavæ, and pulmonary veins, and in the mammary and pulmonary arteries.

*Exp.* 3. Spaniel bitch above the ordinary size. Opening made into the right jugular, and venous pulse observed to exist. After fifteen seconds, introduction of air, with *lapping* sound. The chest is now strongly and suddenly compressed, and a stream of blood, with some bubbles of air, issues from the lower end of the vein. The colour of the blood is brighter than that of venous blood. Two or three more compressions of the chest cause the escape of blood in less quantity and without appreciable mixture with air. After each compression the vein is secured to prevent a fresh introduction of air. The animal did not seem incommoded by the admission of air. The external wound being closed, the animal is left to itself. Two days afterwards, the animal walks about but seems in ill health. Death on the sixth day.—*Inspection* on the day of the death. Large quantity of free air in the right auricle. Clots, and black liquid blood, with numerous air-bubbles, but no froth, in the right ventricle. Numerous air-bubbles in the left ventricle, with a black, half-coagulated blood. No emphysema in the mediastinum. Fluid blood with numerous air-bubbles in the inferior vena cava. Large quantity of air and fluid blood in the crural arteries and veins.

*Exp.* 4. Dog of middle size. Incision made into the right jugular vein at the junction of the middle and lower thirds. Blood issues from the lower end during expiration, but no air enters. A tube of gum elastic is now introduced, as far as the point where the phenomena of flux and reflux are observed. Air now enters, and the respiration becomes difficult. At the end of ten minutes a longer tube is introduced as far as the auricle, and an attempt is made to withdraw the air by means of a syringe. A large quantity of blood, not frothy, but containing about a fifth of its volume of air is withdrawn. The animal has lost a good deal of blood. He is left to himself at the end of half an hour, and lies down with his legs extended, and is unable to move. Convulsions, difficult respiration, death.—*Inspection.* Distension of the right cavities of the heart by blood which is not sensibly frothy. Some frothy blood in the pulmonary artery. Blood without air-bubbles in the left cavities.

*Exp.* 5. Dog of ordinary size. Opening made into the lower part of the right jugular vein. A considerable quantity of blood escapes from the vein and the dog struggles violently. As the air does not enter, a new opening is made into a part of the vein nearer the chest, and the dog is placed in the vertical position. The opening is enlarged but no *lapping* sound is heard. Compression of the chest causes the escape of frothy blood. A little later, the *lapping* sound is heard, but the air evidently finds its way into the vein in smaller quantity than in the previous experiments. A tube of gum elastic is now introduced into the vein as far as the auricle, and blood mixed with foam is removed by means of a syringe. The tube is now removed and the vein secured. The animal which had had some alvine discharges is placed on the ground and walks unsteadily. The dog recovers, and is set at liberty.

*Exp.* 6. Large dog. Laryngotomy performed. Right jugular vein opened below its union with the facial, and about two inches above the position of the venous pulse. No introduction of air, a tube being now introduced into the vein air enters in small quantity during inspiration, and passes out during expiration. The same vein is now opened at its lower part, and the orifice kept gaping; the air enters slowly and silently at first, and then with distinct *lapping* sound. Struggles and accelerated breathing, then again calm respiration. The dog being now held upright and the head drawn back, struggles violently. Twenty-nine minutes after the last opening, a tube is introduced as far as the right auricle, and suction employed by means of a syringe. Pure blood only is extracted. (The syringe is out of repair.) In five minutes more respiration difficult. The dog is now left to itself, the vein being secured. He supports himself a moment, then falls on his side, and has tetanic convulsions of the limbs. Apparent death. A large quantity of water is now thrown over the animal, and he recovers, but still loses blood. Some time after, the respiration is stertorous, the dog stands up, makes a few steps, and then falls again. Convulsive movements with cessation of breathing follow, and apparent death. The animal is left for dead, but recovers on the following day and escapes.

[To these experiments of the commission we will add two experiments made by Nysten, bearing on the same points.]

*Exp.* 7. Large dog. Thirty-two cubic inches of air injected into the jugular vein. After some seconds, pulselessness, deep sighing, and apparent death. The subclavicular vein being now opened, and the chest compressed, a large quantity of air escapes. The animal immediately breathes, the pulse becomes perceptible, and he completely recovers. At the end of three days the dog is killed and no air is found in any part of the vascular system.

*Exp.* 8. Small bitch. Twenty cubic inches of air injected into the jugular. At the same moment, sound indicative of the mixture of air and blood in the right ventricle. After some seconds, cessation of breathing and pulse, and apparent death. A large incision is now made into the interior part of the thorax with a view of inspecting the body, but without any intention of restoring life. The subclavicular vein, as well as many smaller branches being opened, a large quantity of blood mixed with a small portion of air escapes, and the respiration and circulation are reestablished. The animal recovered from the immediate effects, but suffered much from the extensive wound. The third day he was killed, and the heart and large vessels were found free from air, and the lungs healthy. The experiments were frequently repeated with the same result.

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## *On the Influence of Galvanism on the Process of Artificial Digestion.*

By Drs. PURKINJE and PAPPENHEIM, of Breslau.

ON examining with the microscope the products of the artificial digestion of muscular and nervous fibre, Drs. Purkinje and Pappenheim detected a large number of small oblong knotty bodies bearing a considerable resemblance to the seeds of some species of confervæ, and which they were at first inclined to consider as the product of the process, the commencement of a new organization. But on attentive examination of the mucous membrane of the abomasum, or fourth stomach, these minute bodies were found to constitute the parenchyma of innumerable glands, composing almost the entire substance of the mucous membrane. This structure becomes apparent by submitting shreds of the membrane to the action of a concentrated solution of carbonate of potash, by which its tissue becomes hardened. It may then be easily cut into thin segments, which, when inspected with the microscope, display this peculiar structure of the membrane, showing it to be almost wholly composed of a congeries of minute glands. When a portion of the membrane is associated with the requisite quantity of water and muriatic acid to constitute the digestive liquor, the digestive activity of the mixture is in the first place directed against the cellular tissue which connects these innumerable glandules, and which are thus disconnected so as to float freely in the liquor. Our experimenters having convinced themselves that the digestive principle is intimately connected with this mucous glandular membrane, instituted the following experiments with a view to determine whether it alone was sufficient for the process of artificial digestion:

The glandular membrane of the stomach was carefully separated from the subjacent tissues, and portions of it, varying in weight, were put into the same determinate quantity of water along with pieces of coagulated albumen. Artificial digestion did not take place: on the contrary, symptoms of decomposition speedily ensued. It is thus proved that the mucous membrane alone is not capable of producing artificial digestion; but, as this process ensues when a small quantity of muriatic acid is added, it becomes an object to ascertain by what organic process this acid is secreted in the living stomach. Microtomical investigation of this organ discloses no peculiar structure to which the secretion of the acid can be ascribed; and the natural inference is, that the congeries of glandules composing the mucous membrane secretes at once both gastric juice and acid. It occurred to Drs. Purkinje and Pappenheim that possibly the nerves of the stomach, by an influence similar to that of galvanism, might generate the acid, either from the articles of food, or from the saliva and mucus, or from the serum of the blood contained in the vessels of the mucous coat of the stomach; or, lastly, from the runnet or glandular mucous membrane itself. This question was investigated experimentally.

1. Two glasses, containing each two drachms of saliva, connected together by a moist cotton thread, were submitted to the action of a galvanic battery, consisting of thirty pairs of four-inch plates, the wire from the positive pole being introduced into one glass, and that from the negative pole into the other. The odour of chlorine soon became perceptible at the positive pole; and in twenty-four hours the acid reaction was very decided, and was owing to muriatic acid. The nature of the reaction at the negative pole is not mentioned, as it does not bear upon the subject under investigation. Into the acid portion of the saliva, three grains of runnet and three cubes of coagulated albumen were introduced, and the mixture was set aside in a temperature of 98° F. No change was observable in the albumen in the time usually required for artificial digestion; but in eight hours its edges had assumed the characteristic translucency of albumen about to be dissolved; in twenty-two hours the translucency had increased, and the substance of the albumen was as soft as gelatine, but there was no rounding off of the edges and corners. It thus appears, if there be any dynamic process in the stomach having an influence similar to that of galvanism, that part at least of the muriatic acid requisite for digestion may be furnished by the saliva.

2. Into each of two glasses, connected together by a moist cotton thread, one drachm of albumen and three drachms of distilled water were introduced. The mixture was then submitted to the galvanic action. The albumen collected round the wire of the positive pole in large coagulated flakes, and the liquid in its vicinity showed no acid reaction dependent on muriatic acid. In the negative glass there was no appearance of flakes: the liquid, on the contrary, appeared less viscid than at the commencement of the experiment, and showed a slight alkaline reaction, which in twenty-four hours became very powerful; whilst the acid reaction continued weak, probably, as was conjectured, because the acid was expended in coagulating the albumen.

3. Four drachms of nasal mucus were collected and washed with distilled water, and were then rubbed into an emulsion with an equal quantity of distilled water. The mixture was then divided and exposed in equal quantities, in two glasses, to

the galvanic action. The result was the same as in the experiment with albumen. Masses of mucus collected round the positive pole, and the wire had occasionally to be freed from the adhering flakes, that the rest of the liquid might be exposed to the influence of the galvanism. In twenty-four hours the liquid had acquired a tolerably strong acid reaction, dependent on muriatic acid, and the contents of the negative glass appeared less viscid, and showed an alkaline reaction.

4. It is generally supposed (we shall not stop to enquire how far the supposition is correct) that on many organic surfaces, as on the inner surface of the uterus, on the serous and mucous membranes, the serum of the blood is separated directly from the blood contained in the vessels. It thus became a question whether the dynamic effect of the nerves might not be sufficient to separate an acid from the serum of the blood on the surface of the stomach; and in this way to create an agent capable of effecting digestion. A quantity of the serum of human blood was accordingly introduced into two glasses, and exposed to the galvanic influence. In twenty-four hours the serum at the positive pole showed a decided acid reaction, which was proved to be owing to muriatic acid. Into this acid liquor, three grains of runnet and three cubes of albumen were introduced, and the mixture was kept at the temperature of 98° F. The runnet was dissolved, and the albumen became soft and greasy, but it did not undergo solution. In order to ascertain whether the presence of serum operates as an obstacle to artificial digestion, four and a half grains of runnet were introduced into a glass containing two drachms of distilled water, along with three drops of muriatic acid, and a like quantity of albumen as in the preceding experiment. Two drachms of serum were then added: the changes undergone by the albumen were the same as in the preceding experiment; and it thus appeared that the presence of serum impedes artificial digestion, as without it the solution of the albumen would have been complete.

5. It still remained to determine whether, through the dynamic influence of the nerves, the acid necessary for artificial digestion might not be prepared from materials provided by the peculiar organic structure of the runnet itself. Three grains of dried pulverized runnet were mixed with two drachms of distilled water and exposed in two glasses to the galvanic action. The contents of one glass showed an acid, those of the other an alkaline reaction. Into each a piece of albumen was introduced; that in the acid liquid presented translucent edges in about sixteen hours, and in two hours more it was completely dissolved. No apparent change had taken place in the other. Thus it appears that runnet with the assistance of galvanism is capable of yielding acid sufficient to produce artificial digestion. A quantity of digestive liquor was prepared in the same way by galvanism, and divided into two parts. The one was replaced in communication with the galvanic battery, the other was set aside in the temperature of 98° F. Albumen was introduced into each, and in both artificial digestion ensued, but in the former more rapidly than in the latter. The solution of the problem whether the more rapid digestion in the former case was owing to the concurrent influence of the galvanism or to a greater secretion of acid, is reserved till a future opportunity. The enquiry whether the acid obtained from the runnet is the product of chlorides occurring in its composition, or whether the runnet freed from all chlorides is still capable of yielding an acid is likewise deferred.

From the above experiments it may be concluded:

1. That the assumption of a peculiar organ for the secretion of muriatic acid in the stomach is unnecessary.

2. That the secretions which are mixed with the food, namely, the saliva and mucus, and possibly also the serous exudation from the blood-vessels of the mucous coat of the stomach, yield chloride of sodium in quantities sufficient for the digestion of coagulated albumen.

3. That if the nervous action in the stomach is either identical with, or analogous to galvanism, it would be sufficient to account for the secretion of the quantity of muriatic acid requisite for digestion, without the assumption of a special organ of secretion.

In examining the influence of various mechanical agents in furthering artificial digestion it was found that comminution of the coagulated albumen, agitation of the mixture, or an increased pressure, in imitation of that produced upon the contents of the stomach by the muscles of the abdominal parietes, facilitated digestion The increased pressure was effected by the weight of the fluid in a barometer tube.

*Müller's Archiv.* 1838. Heft i.

## PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

### *Cauterization of the Pharynx in Croup.* By Dr. FELIX HATIN.

IT appears this remedy was first tried by Dr. Peronneau in the treatment of laryngeal and pharyngeal inflammations, and Dr. Hatin has since used it in four cases of incipient croup [?] with complete success. The first case given is that of a child five years of age, who was seized with that peculiar hoarse cough so difficult to compare, but so easily recognized when once heard, and which indicates the commencement of croup. Leeches were immediately applied; and on fears being expressed to the father regarding the termination, he related the case of a child who had been cured of an attack of croup by cauterization practised by M. Peronneau. M. P. was then called to the present case, and the following course was adopted. The child was placed upon the father's knees, who with one hand fixed the arms, and with the other retained the head against his chest. The operator placed himself in front, holding in his left hand an instrument necessary to keep the mouth open and to depress the tongue, and in the right a *porte-pierre*, bent like a sound and containing a piece of nitrate of silver, projecting some lines. The tongue being depressed, the tube containing the nitrate of silver was passed into the posterior fauces and rapidly passed over all points for a second or two; the two instruments were then withdrawn to allow of respiration. Some minutes after, a second cauterization, similar to the first, finished the operation. The child did not complain of much pain or uneasiness. The next morning, after a quiet night, the cough was simply catarrhal, and the patient out of danger. Upon looking into the throat, the tonsils, soft palate, posterior wall of the pharynx, and all the points accessible to the sight were covered with a white eschar, which after a few days disappeared, leaving a bright red appearance, not causing sufficient pain to embarrass deglutition.

Another case is related, occurring in the son of M. Imard director of the Hospital "La Pitié." This child, nine years and a half old, was seized in the night with the premonitory symptoms of croup, and Dr. Hatin was summoned in the morning. He at once determined to practise cauterization, but first requested the advice of M. Serres, whose opinion was similar to his own. The application was made in exactly the same way as the last case. In the evening the croupy cough had disappeared, and that which remained gradually diminished in intensity.

Two other cases are given in which this method of treatment was equally efficacious. To ensure success, it is necessary that the cauterization should be performed during the first few hours of the attack, for if the false membrane occupy the larynx or trachea the application will be ineffectual, at least Dr. H. has employed it in two such cases, and it exerted no influence on the progress of the disease.

*Revue Médicale. October*, 1837.

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### *On the Employment of the Hydrochlorate of Tin.* By Dr. SCHLESSINGER.

Dr. S. recommends this medicine as exerting a peculiar soothing power over the nervous system, and also as a remedy in some diseases of the skin. The dose is from a sixth to a quarter of a grain three or four times a day; or a grain may be dissolved in one drachm of muriatic ether, and five drops taken three or four times a day, and increased daily, till the dose is two grains. Sometimes it increases the symptoms it is given to combat; however, this may be considered as a favorable omen. When the patient complains of dryness of the throat, fever, and gastro-intestinal irritation, the remedy should be suspended, or at least the dose diminished. The first case reported, is that of a young man twenty-two years of age, who, for ten months had been subject to epilepsy. The hydrochlorate of tin dissolved in muriatic ether was given, but also a decoction of bark. The convulsions became stronger, and the skin covered with a miliary eruption. The medicine was continued, and the patient cured in about five weeks. Another case is related of an eruption upon the face and hands, which resisted all the means used. After taking the medicine for three weeks, the symptoms increased, but two weeks after they disappeared, and the disease was completely cured.

*Journal des Connaisances Médico-Chirurgicales. Avril*, 1838.

### *On Epidemic Epilepsy in Schools.* By Dr. MEYER.

THE free school of Biclefeld is a well-aired, not overcrowded room, in which the boys and girls are taught at the same time. A young girl of the name of Arnold had for some time been subject to epileptic fits, and had been repeatedly seized during the school hours, on which account she was forbidden to attend. Apparently restored to health she was again admitted, but on the 8th of August, 1837, she was again seized, and was in consequence carried home. A few days afterwards a strong healthy girl who had occasionally accompanied Arnold home was seized with convulsions in the school-room; on the 14th two other girls, aged respectively twelve and fourteen years, were affected in a like manner; but this did not prevent them from making their appearance at school on the following morning. Scarcely, however, had the business of the day commenced, when not only these two, but likewise three other girls, were affected with epileptic convulsions, and the contagion spread with such rapidity that in less than half an hour above twenty girls were similarly affected.

At first the children experienced a feeling of anxiety; they were then observed to grow pale, there was oppression of the chest, and the head became affected; trembling of the limbs followed with loss of consciousness; the thumbs were bent upon the palms, the eyes were distorted, and the patient gave vent to a sudden anxious cry. The paroxysm in some was of short duration, but in others it continued for hours. None of the boys were attacked. The temperature of the room was about 18° R. (72° F.) at noon. None of the girls attacked, except Arnold, had ever previously had an epileptic paroxysm, and no material cause for the disease could be discovered. Most of the girls were approaching the age of puberty, and they were all of a highly excitable temperament.

Notwithstanding that the girls who had been affected were not allowed to return to the school for a considerable time, new cases afterwards occurred, owing, it was suspected, to some of the girls who were not completely reestablished having been readmitted and having suffered from fresh paroxysms. The disease was treated as purely nervous, with valerian, oxide of zinc, indigo, &c.; but on the whole with little success, for after the lapse of five months there were very few who could be considered as safe against a relapse.

The number of sufferers in the school of Rietberg was not so numerous, but was sufficient to show the facility with which convulsive diseases may be communicated to feeble and excitable constitutions.

A gir., aged twelve years and a half, had during two previous years suffered occasionally from epileptic paroxysms. Towards the end of May, 1837, she had an attack in the school, and shortly afterwards four others were similarly affected. In these cases no premonitory symptoms were observed; the patients uttered a shriek and fell insensible to the ground, convulsions generally followed, sometimes alternating with tonic spasms, but in the four last cases there was no foaming at the mouth, nor were the thumbs contracted. The paroxysms lasted about a quarter of an hour, rarely half an hour. The disease appeared to be little under the influence of medical treatment, at least the remedies employed did not seem to have any beneficial effect. *Medicinische Zeitung.* No. 8. 1838.

### *Treatment of Spermatorrhœa.*

Case i. A young man, æt. twenty-three, suffered both bodily and mental debility from involuntary discharge of semen, every night. Antiphlogistic remedies, and subsequently tonics, were employed without any benefit. The extract of lettuce, prepared in the manner recommended by M. Caventon was given, in doses of from two to eight grains daily. The amelioration was rapid, and the patient was completely cured in a fortnight.

Case ii. A similar case was treated with like success with camphor, in the following formula:—Twenty grains of camphor dissolved in almond oil, and made into twenty pills, by means of gum arabic. One was taken every night, and every fourth day the dose was increased by one pill, until four pills were taken daily. The spermatorrhœa, which had existed during three years, and which had resisted various medicines, completely ceased in eight days after taking the above pills, which were continued eight days more, and the cure was complete.

*Gazette des Hopitaux.* No. 52. 1838.

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### *On Artificial Auscultation.* By M. Petrequin.

The difficulty attending the commencement of the study of auscultation, and the great number of observations required before the student is enabled to appreciate justly the great variety of sounds which may be recognized in the diseased lung, have induced M. Petrequin, surgeon of the Hôtel Dieu at Lyons, to invent a method of artificial auscultation. By a simple contrivance he is enabled to reproduce after death the sounds which are characteristic of the diseases which have proved fatal, and thus to afford a great facility to the student. In his first experiments he removed the lungs from the body, and by applying a pair of bellows to the trachea, he produced in the healthy lung a vesicular murmur precisely similar to that heard during life. Afterwards, by injecting liquids of different densities and then inflating as before, he was enabled to produce the several mucous râles or rhonchi. In the case of a man who had died of pneumonia, he inflated the lungs after removing them, and produced crepitous râle with bronchial respiration. He next commenced artificial auscultation of the lungs without removing them from the body. For this purpose he opened the larynx or trachea, and inserting the pipe of the bellows, he carefully closed the opening. He found it convenient to make use of a bellows with a syringe attached, as the sounds became occasionally obscured by mucosities drawn from the minuter air cells, and the syringe served to remove these. M. Petrequin now began to auscultate the bodies of those with whose symptoms he had not been acquainted during life, and he constantly found his diagnosis verified by the inspection which was afterwards made. Thus in cases of pneumonia he found absence of respiratory murmur, and crepitating râle more or less intense according to the degree and period of the inflammation. In hydrothorax he found the pulmonary sounds obscured by the fluid in the chest, and also modified by the position of the body. In phthisis he found dullness, bronchial respiration, and mucous rhonchus or *gargouillement*. Encouraged by this success, he endeavoured to produce an artificial voice in the corpse; but his first efforts were not successful. He introduced a tube into the larynx, and afterwards a metallic funnel into the trachea; but on speaking through them he found that there was no resonance in the chest. At last he succeeded by placing one end of the stethoscope on the larynx of a person speaking in a loud voice, the other end being applied to the division of the bronchi in the subject, and artificial respiration being kept up. This artifice produced resonance of the voice as well as of cough in the chest.

M. Petrequin has found artificial auscultation of the greatest use to himself in the study of the pulmonary sounds. Here (he says) is no patient to be fatigued and have his symptoms aggravated by a long and careful examination; here all distracting noises may be removed, and the experimenter will have the great advantage of being able immediately to verify or to correct his diagnosis, and to assign the physical causes to the respective sounds with a degree of certainty which cannot be obtained during life on account of the organic changes produced by the progress of the disease.

[All this is no doubt very ingenious, and may be of use in some cases to those who are commencing the study of auscultation. Knowing, however, the ever ready and inexhaustible stock of materials for auscultation in the *living* body that exist around every enquirer, we cannot but see in the investigations of M. Petrequin, rather the luxury or wantonness of riches than the resource of poverty: we are, nevertheless, far from wishing to depreciate his labours, and even recommend them to the notice of our students in the dissecting room.]

*Revue Médicale. Mars*, 1838.

## *On the Use of the essential Oil of Turpentine in Diseases of the Eye.*

By Dr. A. TRINCHINETTI.

THE author's experience induces him to place great confidence in the oil of turpentine in the slow and deep-seated inflammations of the eye, especially in those that do not yield to antiphlogistic measures. Cases are given proving its utility in chronic inflammation of the iris or ciliary bodies, and in incipient gangrene of the cornea, all of these following the operation for cataract; in the chronic stage of rheumatic iritis, or even in the outset, if it be mild; in traumatic iritis, ulcers of the cornea, onyx and incipient glaucoma. The oil should be administered in emulsion, the dose varying from half a drachm to four drachms daily.* The phenomena generally following its use are diminution or cessation of pain, a sense of general comfort, contraction of the vessels with gradual disappearance of the inflammatory fulness and lachrymation; the easy dispersion of the matter effused into the anterior chamber or between the lamellæ of the cornea. Occasionally a sensation of weight and burning in the stomach, especially after full doses, was felt, and in some rare cases was sufficiently troublesome to prevent the further administration of the drug. Instead of producing a purgative effect, it caused constipation; the urine became abundant, of violet odour, was passed without pain and deposited a reddish sediment.

*Giornale delle Scienze Med.-Chir.* No. 26. *Agosto*, 1836.

## *On the Employment of Chlorine in Acute and Chronic Bronchitis.*

By Dr. TOULMOUCHE, Physician to the Prison of Rennes.

DR. T. has been in the habit, since 1831, of employing chlorine in the treatment of bronchitis: he has found its effects beneficial in acute as well as in chronic bronchitis, and sometimes in pituitous catarrh. The method of employing it is similar to that generally adopted. A dose, varying from ten to one hundred drops of solution of chlorine, is poured upon hot water in an inhaling vessel, through which the patient breathes: from thirty to forty drops is the dose generally employed. Bronchitis is endemic in the prison of Rennes, which is situated on low and damp ground, surrounded with water. In its acute form, it presents the ordinary symptoms, and requires antiphlogistic treatment. The inhaled chlorine has generally been well borne; and, if it occasionally appeared to irritate at first, its administration was resumed with success in a few days. Chronic bronchitis is particularly frequent in the prison, on account of the dampness of the prisoners' workshops, as well as the generally unhealthy situation. When not complicated with emphysema of the lungs, chronic bronchitis is much benefited by chlorine, which produces no irritation, but alters the character and diminishes the quantity of the bronchial discharge. In cases of pituitous catarrh, or bronchorrœa, where the sputa are generally transparent, but little benefit is derived from chlorine. In addition to the inhalation, a solution of chloruret of soda was occasionally given; but the patients were with difficulty induced to take it, on account of its very bad taste. In four years and a half, the chlorine treatment was employed in 309 cases,

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* The best formula for its exhibition is that proposed by Mr. Carmichael in 1829.

—228 female and 81 male,—although the males were to the females in the prison as 150 to 180. Of 141 cases of acute bronchitis in female prisoners, 134 were cured after a treatment of from five to fifteen days. Of 65 cases of chronic bronchitis, 51 were cured after a treatment of from five to ninety-eight days: 4 died; the rest must be considered as not relieved. When chronic bronchitis was complicated with phthisis, no benefit was derived from inhalation. Of 65 cases of acute bronchitis in males, all were cured after treatment of two to ten days. Of 12 chronic cases, 10 were cured after using the chlorine from ten to forty days. Dr. Toulmouche cannot state precisely the duration of the disease in cases previously treated in the ordinary method; but it is much greater than in those treated with chlorine. *Gazette Médicale, June,* 1838.

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### *Of the Symptoms of the earliest Stage of Phthisis Pulmonalis.* By M. Fournet.

[The following observations are much less novel than the author seems to imagine; and the accuracy of some of them we, moreover, question: still as no point in practical medicine is more important than the one here sought to be elucidated, we give M. Fournet's remarks as we find them, and without comment.]

The endeavour of M. Fournet has been to detect pulmonary tubercles when few in number and in a state of crudity. His investigations have been carried on under the eye of M. Andral, whose testimony is added in support of his opinions.

*General Phenomena.* Instead of that state of happy illusion which accompanies phthisical patients, even when on the verge of the tomb, these patients, in the early stage, are always disturbed, morose, and constantly occupied with melancholy thoughts about themselves; and it is only when the disease has made some progress that the state of confidence is manifested.

*Antecedent History.* M. Fournet regards other circumstances besides hereditary disposition as exercising a strong influence on the development of phthisis; such as excess in spirituous liquors, extreme venery, grief, too hard bodily work, residence in a bad atmosphere, bad food: and he says that when the disease develops itself under the influence of these causes, its form is commonly miliary and its progress rapid; whilst the constitutional phthisis is chronic, and characterized by large tubercles.

*Local Signs.* In the first period of pulmonary phthisis, the soft and easy character of the inspiration and expiration is changed to one of hardness, harshness, with an impression as if the sound was produced with difficulty. The sounds become dry, instead of imparting, as in the normal state, a sensation intermediate to moisture and dryness. The inspiration is less in duration; sometimes is unchanged in intensity; at others, this is increased. The expiration increases successively, both in intensity and duration; and this augmentation is in direct relation with the more or less advanced state of the disease. The changes in the "key" (*timbre*) are subsequent to those in the duration and intensity of the respiratory murmurs, and to the harshness and dryness of them. They are always first heard in expiration, and afterwards during inspiration. Two sounds, described by M. Fournet as "bruit de froissement pulmonaire" and "bruit de craquement," belong equally to the first period of phthisis. There is a dry and a moist condition of the "bruit de craquement." The dry *craquement* commonly exists only during inspiration, and it extends also to the act of expiration in proportion as its character changes from dryness to moisture. The "bruit de froissement pulmonaire" is scarcely heard except during inspiration. M. Fournet has further established the fact that the sounds of the heart are louder in that clavicular region where the tubercular infiltration is the more advanced: and this fact is of considerable importance, if, as is stated by M. Fournet, the disease, even in its early period, is more advanced on the right side; since, in this case, the transmission of the sounds of the heart more forcibly beneath the right clavicle is contrary to the normal condition.

In percussion, M. Fournet thinks that as much is to be learned by touch as by auscultation. The vocal vibration is diminished in direct proportion to the degree of tubercular infiltration; excepting when a cavity is formed, which increases the vibration. M. Fournet has found, also, in very many cases of phthisis in their earliest period, that there is a sensible diminution of size of the summit of the chest; and, consequently, a flattening in front, and a sinking more or less considerable of the supra and sub-clavicular regions. He measures this degree of sinking of the summit of the chest by stretching a straight line between two fixed points, the middle of the clavicle and the nipple, and by measuring the radius of the arc of the circle which subtends this straight line. This mode of measurement is more exact than that which is practised above the moveable mass of the great pectoral and dorsal muscles. And, lastly, M. Fournet has analyzed the sensations experienced by patients in different parts of the chest, and he has ascertained the existence of a sensation of difficult, straitened, and incomplete respiration on that side where the tubercular infiltration was at its maximum: and this side becomes affected with a painful resonance when the patient speaks or coughs somewhat forcibly, or when percussion is practised comparatively upon the corresponding parts of the thorax. The voice, too, becomes feeble, is formed with effort, is veiled, somewhat hoarse, and of a deeper tone. Of these signs, some appear sooner than others. It is by a comparison of them, and of their relations and connexions one with another, the predominance of one over another, &c., that the diagnosis is formed: and on the above grounds the author, eight months since, diagnosticated the existence of infiltrated tubercles in the summit of the lungs of a strong and healthy young man, aged seventeen, who had never had hæmoptysis, was not born of tubercular parents, but had been admitted under the care of M. Andral for an eruptive fever. At the present time this patient has all the signs of cavities in the upper part of the chest.

*Bulletin de l'Academie Royale de Médecine.* No. 13. 1838.

## SURGERY.

### *On Chopart's Method of Amputating the Foot.* By M. Blandin.

At the sitting of the Academy on the 29th of May, M. Blandin presented a memoir on the amputation of the foot at the articulation of the os calcis and astragalus with the scaphoid and cuboid bones. This operation, so desirable in cases of disease of the tarsal bone, has been generally objected to on account of the danger of retraction of the calcaneum; the action of the tendo-achillis being no longer counterbalanced by the tibialis and peroneus anticus, the tendons of which are divided in the operation. M. Blandin has performed the operation several times with success, and his expectations have never been disappointed by the retraction of the heel, and consequent depression of the cicatrix. Having recently had the opportunity of examining the stump in three individuals who have undergone this operation, he thus describes the anatomical disposition of the parts: "The tendons of the anterior muscles of the leg present, at the cut extremity, a very curious disposition, which has not hitherto been noticed. These tendons terminate upon the head of the astragalus and upon the cuboidal surface of the calcaneum, which have been deprived of their articulating cartilages by absorption; they there contract a very important insertion, which enables them to continue to transmit to the foot the action of the muscles to which they belong. In addition to this, by means of the cicatrix, they are continuous with those of the superficial muscles of the sole, which act upon the calcaneum by their remaining insertion; so that the anterior muscles of the leg, and those of the sole of the foot, form but a single system, the different parts of which concur in preventing the elevation of the heel, and enable the foot to have the degree of flexion necessary for the functions which it is still called upon to perform. So that we have, in fact, *digastric* muscles, the middle tendons of which are formed by those of the dorsal and plantar muscles united, whilst the opposed bellies are constituted by the fleshy fibres of the anterior muscles of the leg and the inferior of the foot. A bursa mucosa, formed of the remains of the synovial membranes of the articulation destroyed, is placed beneath the middle tendons of these anormal digastrics, and facilitates their motion upon the astragalus and calcaneum. We find these two powers opposing the retraction of the heel, which would otherwise appear inevitable: 1st, the new insertion of the anterior muscles of the leg into the calcaneum and astragalus; 2d, the continuity accidentally established by means of the cicatrix between the anterior muscles of the leg and those of the sole, giving the former an additional adhesion upon the inferior surface of the heel, and thus enabling them to counterbalance the action of the muscles of the calf." M. Blandin concludes by stating that retraction of the heel after this operation is rare, and would probably recur still more rarely if care were taken to preserve the tendons sufficiently long: he gives his decided testimony in favour of the operation.

In the discussion which followed, M. Larrey denied the general utility of Clopart's operation, and stated that he had seen the retraction of the heel so great that the patient could only walk by supporting the knee on an artificial leg. M. Velpeau considered the retraction as of rare occurrence, but had seen two cases in which it had produced much inconvenience. M. Blandin, in reply, stated that he had performed the operation eleven times without the occurrence of this accident. *Bulletin de l'Academie. June,* 1838.

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## *On Rupture of the Perineum in Females.* By Professor DIEFFENBACH, of Berlin.

IN this important paper Professor Dieffenbach first deprecates the general practice of leaving slight ruptures of the perineum to the unaided efforts of nature. On examining the perineum after the healing of a rupture extending backwards for half an inch or a whole inch, no corresponding cicatrix is seen, but the perineum is found contracted and diminished in breadth, and the labia are drawn backwards in such a manner as to enlarge the orifice of the vagina. In slight cases this is of little consequence; but, when the rupture has been extensive, relaxation of the vagina probably ensues, with protrusion of its mucous coat, and frequently also prolapsus of the uterus. In straining at stool, the anterior wall of the rectum bulges forwards into the vagina; and in other instances the superior wall of the vagina and neck of the bladder sink into its cavity. The fæces are still retained, but flatus often escapes unexpectedly. But still these evils are small compared with those which ensue in cases where the perineum, vagina, and rectum have been torn in such a manner that the vagina and rectum form but one cavity. Professor Dieffenbach recommends, therefore, that, even in slight cases of rupture, the edges of the wound should be stitched together, and union sought by the first intention, in order to avoid deformity of the parts. The following cases are given in illustration.

CASE I. A young woman, aged twenty-six, in giving birth to her first child, suffered from rupture of the perineum, extending backwards about an inch. The edges of the wound were cleaned, and three sutures applied. Two of the sutures were removed on the third day, and the other on the fourth. The union was complete.

CASE II. The perineum of a woman, aged thirty, was torn to the extent of an inch and a half, in giving birth to her third child. Ten hours after the accident the sutures were applied, and were removed on the fourth and fifth days; complete reunion having been effected.

[Two similar cases are detailed, which we will not transcribe.]

CASE V. In a young woman, aged thirty, who had led a very irregular life, there was found incipient prolapsus of the uterus; the genital organs were very much relaxed and very large, and the perineum was extremely narrow. It could not be ascertained whether this state was the consequence of simple dilatation, or whether rupture of the perineum had previously existed. The posterior edges of the labia were made raw, and then approximated and retained in situ by means of eight sutures. Two additional sutures were employed to unite the mucous membrane of the vagina. The operation was completely successful: the perineum acquired considerable breadth, and the vagina became so narrow that, after her recovery, the patient resumed with great success her previous vocation.

CASE VI. In a strong, robust young woman, aged twenty-six, in labour with her first child, the perineum, part of the vagina, and several inches of the rectum, were unfortunately torn. Six or eight hours after the accident, a suture was passed through the edges of the vaginal wound, and by means of it the edges of the wound in the posterior wall of the vagina were brought into view and united. In the same way the wound in the rectum was drawn forwards and stitched, and the parts were then allowed to resume their natural position. The perineum was then united by powerful sutures, and moderate antiphlogistic treatment pursued. The obesity of the patient was a great obstacle to the success of the operation: it succeeded so far, however, that the cavities of the vagina and rectum were separated from each other, and the power of retaining the fæces restored; but the reunion of the greater part of the perineum did not take place.

[Four other cases are given: two of them were of recent occurrence, the other two had existed for years. In the former two, the operation was completely successful; and partial success was obtained in the latter.]

The treatment pursued was in general antiphlogistic, and, during the first six, eight, or ten days after the operation, the bowels were kept constipated by repeated doses of opium. When passage of the bowels could no longer be avoided, an elastic catheter was introduced into the rectum, and a stream of warm water, holding a small quantity of soap in solution, was thrown up; by which means the scybalous mass was broken, and passed without injuring the parts. The catheter was employed, in general, four times daily to empty the bladder.

*Medicinische Zeitung.* No. 52. 1837.

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## *On the beneficial Action of Acetate of Lead in incarcerated Hernia.*

### By Dr. HUXTHAUSEN.

CASE I. A woman, aged seventy-six, weakened by chronic cough, and having an unreduced inguinal hernia, was suddenly attacked with severe cutting pains in the bowels, accompanied by vomiting, at eleven o'clock in the evening of the 7th May, 1837. She was seen by her medical attendant at nine o'clock on the following morning, who found the symptoms to be dependent upon the hernia having become incarcerated. It was about the size of a hen's egg, hard, and painful upon pressure. As the attempts at reposition failed, an injection of eight ounces of a solution of acetate of lead (Acet. plumb. ʒj., Aq. ℥vj.) was administered, a spoonful of castor oil given internally, and ice applied to the tumour. At twelve o'clock the reposition followed of its own accord.

CASE II. A robust woman, aged thirty-two, affected with chronic crural hernia, was very subject to hysterical attacks. On the 13th and 14th September, 1837, she had two violent paroxysms, in consequence of which the hernia became incarcerated. The tumour was of the size of a goose's egg, distended and painful. All attempts at reposition having failed, eight ounces of the solution of the acetate were given in an injection, ice was applied to the tumour, and a powder, composed of acetate of morphia, gr. ⅛; nitrate of bismuth, gr. iii.; sugar, ℈ss.; and chamomile oil, gtt i., administered every hour. The ice was removed in half an hour, as it seemed to increase the pain. From this time the symptoms were relieved, the vomiting ceased, the tumour gradually decreased in size, and in five hours the reposition was complete.

CASE III. A boy, aged twelve, fell on the evening of the 15th October; and, when seen on the following morning, complained of great pain in the right inguinal region, and was in a state of high fever. On examination, an incarcerated hernia, about the size of a goose's egg, hard and extremely painful, was detected. Reposition was attempted, but failed: twelve leeches were then applied to the tumour, a spoonful of castor oil given internally, and six ounces of the solution of the acetate administered in injection. At eleven o'clock, little benefit resulting from these measures, six ounces additional of the solution of the acetate were given in injection. Improvement now took place so rapidly that at one o'clock spontaneous reposition ensued.

[Although the small number of cases here given, and the impossibility of exactly measuring the influence of the other agents employed, forbid us to draw positive conclusions as to the efficacy of this mode of administering lead in hernia, still the subject deserves the attention of surgeons.]

*Medicinische Zeitung.* No. 7. 1838.

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### *On Hemlock Baths in Cutaneous Diseases.* By Dr. Fantonetti.

M. Fantonetti has proved the good effects of hemlock baths in acute and chronic affections of the skin: he has used them with success in erythema, impetigo, psoriasis, lichen, and they have even alleviated the pains of gout. He regards the baths and lotions of the decoction or infusion as sedative, resolutive, and desiccative. This remedy acts promptly, and, used in a proper manner, never produces injurious effects. The bath is prepared either by infusing or boiling eight or ten small handfuls (*pincées*) of dried or fresh hemlock in eight or ten quarts of water, which is to be then poured into the bath, heated to the temperature of 90° or 92° Fahr. The patient should remain in the bath an hour or two, and should be surrounded by a blanket brought close around the neck, to prevent the vapour producing vertigo or pains in the head. M. F. supposes the hemlock to act from the alkaloid principle which it contains, and which explains, he asserts, the reason why the decoction and the infusion are equally efficacious. This alkaloid principle does not evaporate in the same manner as the volatile parts of aromatic plants, which are employed for similar purposes.

*Revue Médicale. October,* 1837.

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### *Dislocation of all the Metatarsal Bones upon the Tarsus.* By M. Mazet.

The wheel of a heavy laden cart passed over the foot of a young man, aged nineteen. When examined, the foot appeared twisted on itself, the convexity of the curve being above and outwards. On the dorsal surface of the foot was a wound, across which was a bony projection, elevating the extensor brevis digitorum, and the tendons of the common extensors of the toes, apparently formed by the posterior extremities of the three middle metatarsal bones. External to this projection was another, but this was covered by skin. The bones of the leg were not broken, and the toes were uninjured. It being believed that several bones were bruised, the leg was amputated. The patient died of phlebitis. The amputated limb was dissected, and the following were its appearances: The most external digitation of the extensor brevis was torn; the tendon of the tibialis anticus was partially ruptured, about an inch from its termination; none of the muscles in the sole of the foot were at all torn; but the interossei, contained in the first and in the fourth space, were ruptured. All the metatarsal bones were dislocated from their tarsal articulations, although they were not displaced together towards the same point, and did not maintain their normal relations to one another: the second, third, and fourth were dislocated together towards the dorsal surface, so that their tarsal extremities rested on the superior surfaces of the cuneiform bones; the first and the fifth were quite separated from their usual connexion, and were situated thus: the posterior extremity of the first rested on the inner side of the first cuneiform bone,—that is to say, it was dislocated inwards, carrying with it and stretching the tendon of the peroneus longus muscle; the fifth metatarsal bone, which had suffered the most considerably, was dislocated at its posterior extremity, which is not connected to the bones of the tarsus by either tendon or ligament. This bone was turned on its axis, so that its internal surface was upwards; and, in addition, it was fractured about three-quarters of its length from its tarsal articulation.

*Gazette des Hôpitaux. Février,* 1838.

# MIDWIFERY.

## *On Puerperal Fever.* By Dr. A. GRUBER.

DR. GRUBER recognizes different forms of puerperal fever, dependent upon different affections of different parts; he is alive to the difficulty of distinguishing these different forms from each other during life, but contends that there are two general causes of the disease, the one inflammation, the other mortification from defect of action. In inflammation of the uterus, its muscular texture becomes swollen and hard and its increased size is attributable to no want of contraction but to increased thickness of its parietes. In mortification of the uterus, on the other hand, its texture becomes thin and weak, its substance wastes away, it loses its power of contraction and its cavity is increased in size. The inflamed uterus retains a spherical form but when affected by gangrene it becomes flat or even wrinkled. Inflammation of the uterus is accompanied by pain and restlessness, but when gangrene is present there is no pain, either at the onset, or in the progress of the disease. In inflammation, too, there is great tenderness and increase of heat in the inflamed parts; in gangrene tenderness is absent and the heat is often diminished. Examination after death of those who have died of inflammation of the uterus does not always detect disease of the mucous membrane of the intestinal canal; after death from mortification, however, the mucous membrane of the stomach, of the lower part of the ileum, and of the colon on both sides is found changed, and is usually softened, loose, of a mottled white and grey colour. Lastly, in inflammation of the uterus antiphlogistic measures are of great service, in gangrene they are evidently injurious. Dr. Gruber contends that gangrene of the uterus is not necessarily the result of inflammation but often exists independently of it, as is proved both by the symptoms during life, and by the appearances after death. When the peritoneum is the seat of the disease and there is low action, the severe pain, restlessness, and anxiety, which are the chief diagnostic symptoms of the acute inflammation of the membrane, are entirely absent. The progress of the disease is always slower in this case and death does not commonly take place till the fourth or fifth week. In every case of this kind which the author observed, putrescence of the uterus was present, and the mucous membrane of the stomach and intestines was reduced to a pulp. The diseased peritoneum was of a black colour, loose, with a rough granular surface, easily detached from the subserous cellular membrane which was found in the same state. The cavity of the peritoneum contained some ounces of a dark brown fluid without any trace of cheesy or membranous deposits. This state of the peritoneum was always accompanied by gangrene of the uterus and was invariably most strongly marked in the immediate neighbourhood of that organ. With regard to the mixed character of the contents of the peritoneal cavity, Dr. G. thinks that the cheesy matter may be regarded as the product of the inflamed membrane, and the serous effusion of the uninflamed portions; the former is found wherever there have been symptoms of peritoneal inflammation during life, the latter where they have been entirely absent. We will add one other remark of Dr. Gruber that chlorosis strongly disposes to gangrene of the uterus; twelve years' experience have convinced him of the truth of this observation. The author draws no practical inferences from his views of the pathology of puerperal fever.

*Zeitschrift für die gesammte Medicin.* Band v. Heft 2.

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## *Case of Spontaneous Rupture of the Uterus.* By M. GENDRIN.

A WOMAN, aged twenty-four, was admitted at La Pitié under the care of M. Gendrin. She was in the sixth month of pregnancy, having had a favorable accouchement of her first child three years previously. At the fifth month of her second pregnancy a slight hemorrhage came on, followed by uterine contractions. These symptoms recurred frequently and reduced her to a state of great weakness. She came into the hospital about the end of December, 1836, very pale and thin. On examination the fundus of the uterus was found to have reached the umbilicus, the back and feet were easily recognized through the abdominal parietes; the pulsations of the fœtal heart were heard on the right side; the movements of the child were felt by the hand placed on the abdomen. The head was presumed to be in the second position. The neck of the uterus was not obliterated; it was soft. Pains were felt at long intervals, and a slight muco-sanguineous discharge existed. She went on thus for several days. On the night of 2d January, 1837, violent pains came on, which were very acutely felt, and did not entirely cease between the contractions: in the morning she was sure she could not go on all day without relief; the movements of the child were no longer felt; the os uteri being scarcely at all dilated: a dose of ergot was given, an emollient enema having been previously administered; but she sunk into a state of exhaustion, and died at three o'clock in the afternoon.

*Dissection*, forty hours after death. On opening the abdomen, the body of the child was found in contact with the intestines, the back directed forwards, the feet upwards; it had escaped through a longitudinal laceration in the right side of the uterus: close to the cervix, where the head had rested, was another laceration with uneven edges, not extending through the peritoneal coat. The placenta was attached over the neck of the uterus, and was partially separated. The head and placenta alone remained in the uterus. A great quantity of fluid blood mixed with liquor amnii, and some coagula were contained in the peritoneal cavity. The structure of the womb appeared to be rather softened. The pelvis was well formed and sufficiently capacious. The fœtus appeared to be about six months and a half old; it showed no sign of putrefaction. In this case the lacerations corresponded to those parts of the uterus where the head and feet had rested.

The accident was probably caused by the long continuance of slight uterine contraction, which gradually thinned the walls of the organ until they gave way, not all at once, but by degrees. In what other way can it be explained, there being no obstacle in the conformation of the pelvis?

*Gazette Médicale de Paris. February*, 1837.

## MEDICAL JURISPRUDENCE.

### *Important Cases of Death after Wounds and Injuries.*

### First Series. *Death after, but not from bodily Injuries.*

[There are few cases in which the value of medical evidence is more strikingly seen than those where persons die after having sustained greater or less bodily injury, and, on inspection, disease sufficient to account for death is found in one or more important organs. Who but a cautious medical practitioner is qualified to pass an opinion in such cases? and yet coroners in this country are now frequently in the habit of disposing of them without calling for any medical opinion.]

Case i. By Dr. Meyer. A man, who had misconducted himself before a magistrate, was struck by that functionary twice on the face with the flat of his hand. One blow was slight, the other pretty severe; but neither ecchymosis nor swelling followed, and the deceased did not seem to suffer from them in any degree. He remained in the office for some minutes afterwards, signed his name to a paper, and then went home. On the next day the deceased went to his usual work, but complained of his head: his occupation was that of a miner. His companions did not observe anything particular in his manner. It was not until the thirteenth day that he sought for medical advice: he then complained much of his head, and attributed the pain which he suffered to the blows inflicted on him by the magistrate. Some medicines were prescribed for him: he passed a quiet night, but the next morning he suffered from occasional fits of the most excruciating pain. The following day, the fourteenth from the time that he was struck, he expired somewhat suddenly and unexpectedly. The magistrate was charged with having caused the death of the man by striking him on the face; and an inspection was ordered, to determine the cause of death.

The exterior of the body was free from any mark of violence. In the cranium there was no trace of fracture or of extravasation of blood; but, connected with the dura mater, on the posterior surface of the petrous portion of the right temporal bone, there was a round tumour, about the size of a hazel-nut, and of a greyish green colour. The fungous excrescence, on being opened, was found to contain a few drops of yellow coloured pus: it was connected with the bone by means of a small peduncle, but the bone was not diseased, nor was there any sign of redness or inflammation in the parts around. The tumour was evidently of long standing. The softness of the brain, from putrefaction, prevented any satisfactory inference being drawn as to its healthy or morbid condition. The thorax and abdomen presented nothing abnormal.

The inspectors, in giving an opinion as to the cause of death, observe, blows on the face may cause death, either by concussion, extravasation of blood, or by exciting inflammation of the cerebral substance: these dangerous effects are only likely to follow where the blows have been very severe; here, however, they were slight. The deceased obviously did not die of concussion, since symptoms of concussion immediately follow the violence producing it; while he remained sensible and capable of exerting himself after the violence. There was no appearance of extravasation of blood or inflammation of the brain; nor were the symptoms under which he suffered previous to death those indicative of either of these conditions. The fungus on the dura mater could not have arisen from the violence used towards the individual, fourteen days before his death: it had doubtless been forming for many months previously, and seemed to be wholly independent of any external causes. The growth of a tumour of this description is sometimes very insidious, and it often causes death suddenly by compression, when it has attained a certain size. The deceased, although he attributed the pain in his head entirely to the blows received from the magistrate, was proved to have complained at intervals of severe pain in that part for more than a year before his death. He was a man of irregular habits, and had been engaged in many quarrels, in which he had received injuries on the head. These facts rendered it highly probable that the fungus had its origin a long period prior to death. The conclusions to which the reporter of the case came were: 1, The death of the deceased did not proceed from the blows inflicted on his face by the accused. 2. Death was to be ascribed exclusively to the fungus on the dura mater, which must have been formed long antecedent to the period at which he received these blows.

On this the accused was immediately discharged from all responsibility for the death.

CASE II. By DR. GRAFF. On the 29th November, 1835, a man, æt. 60, was struggling and fighting with another, when he was violently thrown down, and his neck compressed by his antagonist almost to strangulation. He raised himself, and wished to renew the fight, but soon afterwards fell senseless, and was obliged to be taken home. On the 2d December he felt ill, and kept his bed until the 4th. On the 6th he was up, but appeared to be wandering in his mind: he did not, however, make any complaint of illness. On the morning of the 7th he was found dead in bed: his death took place, therefore, about a week after the fight.

An inspection of the body was ordered, since it was supposed he had been killed by his antagonist. Externally there were no marks of violence, but simply a few scattered patches of lividity. In the head there was great fulness of the vessels of the brain, with general vascularity of that organ. The cavity of the chest was found to contain upwards of a pint of watery serum. The lungs were strongly adherent to the sides: they were of a dark reddish colour, partly hepatized, and partly in a state of suppuration. The pulmonary artery was ossified. In the right ventricle of the heart there was a polypus of considerable length, extending through the right auricle into the superior cava; it was fixed in the ventricle by a base of more than an inch in extent, and divided at the opposite end into four parts; it was of a firm and compact texture. A similar polypus was found in the left ventricle. The viscera of the abdomen presented no abnormal appearances. There were inguinal herniæ on both sides, and the sac on the left side contained a portion of small intestine. The medical examiners gave the following opinion in this case: 1. That serious and extensive disease, evidently of old standing, existed in some of the most important organs of the body: this referred more especially to the morbid changes in the lungs, and the polypi in the heart. 2. The deceased could not have lived long, even had he not been engaged in the fight a week before his death. 3. Had the deceased been perfectly well, the fight would probably have had no effect on him: there was no mark of external violence, except a slight sugillation on the skin of the occiput. The only conceivable danger to which he was exposed during the fight was from the compression of his neck, while lying on the ground: this might have operated by impeding the cerebral circulation. 4. The deceased died of apoplexy, and, although morbid causes existed in his body to favour the occurrence of this disease, yet, from the symptoms under which he laboured at the time of the quarrel, and subsequently, there was reason to think the fatal attack had been brought on, or at least accelerated, by the violence with which he was treated. The reporter then enters into a discussion, according to the singular principles of German jurisprudence, respecting the precise grade of mortality which should be assigned to the injuries received by the deceased. He comes to the conclusion that they must either have been *individually* mortal, (i.e. from the diseased state of the individual,) or *accidentally* mortal; but which of the two was the more likely he could not say.

[The cause of death is not here very clearly made out. The disease of the lungs was certainly sufficient to account for death; but the description of the symptoms under which the deceased laboured subsequently to the fight is so imperfect as to lead only to a conjectural opinion. The "polypi" in the heart were probably nothing more than consolidated fibrin, and were entirely of post-mortem origin. Although it may be doubtful as to what was the immediate cause of death, it seems pretty certain, from the deceased never having effectually recovered after the fight, that it was at least accelerated by the personal injuries which he had sustained.]

Case III. By Dr. Hohnbaum. On the 6th January, 1835, the deceased, æt. 20, was violently beaten by some companions on the head and chest. A slight hemorrhage resulted from these injuries. On the same day he was taken ill, and was obliged to keep his bed. Nothing seems to have been known of his condition until the 8th April following, when he first applied for medical advice: he was then evidently labouring under diseased lungs. On the 16th May, the disease had so far advanced that all hope of his recovery was abandoned; but he lingered on until the 7th November following, when he died: *ten months* after the receipt of the injuries which, it was alleged, had caused his death. A judicial inspection of the body was ordered to be made.

Externally, there was merely cadaverous lividity. The right side of the chest appeared flattened, as it is often seen in phthisical subjects. On laying open the cavity, the right lung was found much collapsed, and placed posteriorly: it was filled with tubercles, in various states of softening and suppuration. The left lung presented traces of inflammation and strong adhesions: in its upper lobe was an abscess, containing much matter. On this side of the chest about five ounces of water were discovered. No particular appearances were met with in the head or abdomen. The result of the inspection left no doubt that the deceased had died of phthisis: but it appeared that the deceased had been in good health up to the time of the maltreatment, and it was therefore a question how far this fatal disease might have originated from the violence which it was proved he had sustained ten months previously. Against this view of the origin of the disease it was alleged that no mechanical injury had been done to the chest, sufficiently to account for the production of phthisis: there had been no fracture, laceration, or extravasation, and the inflammation found in the left lung on inspection might have been a simple accompaniment of tubercular disease. The morbid appearances indicated phthisis of long standing, and the deceased was undoubtedly of a phthisical diathesis. This disease, besides, might have been going on long before the violence, and yet have remained unperceived by his friends. The only way in which the maltreatment could have brought about the disease was by inducing chronic inflammation of the lungs in a body already predisposed to phthisis; but it is doubtful whether the violence actually operated in this manner. In conclusion, death was ascribed to phthisis proceeding from constitutional causes, the fatal effects of which had probably been accelerated by the ill treatment, as well as by the want of proper medical assistance for a long period afterwards.

[There is no doubt that in this case death was due to phthisis, and not to the violence used: the same amount of violence, applied to a non-phthisical subject, would probably have been attended with no serious effects. We cannot look upon phthisis as even likely to be a secondary consequence of mechanical injury to the chest, unless there existed a strong tendency to the disease. The appearance of phthisis under these circumstances ought to be regarded more as a coincidence than as an effect. The searching for these latent causes of death in persons who have died after having been maltreated, is a subject of great importance in legal medicine; since the prejudices of the vulgar always lead them to connect death with the bodily injuries alone, more especially where, as in this case, the disease had not strikingly manifested itself by symptoms previous to the receipt of the violence.]

### SECOND SERIES. *Death from external Injury, without external Signs.*

[The following cases illustrate a very important fact in medical jurisprudence,—namely, that severe internal injuries, proving rapidly mortal, may result from mechanical violence applied to the body, without being indicated by corresponding external marks.]

CASE I. By Dr. MEYER. Two men were in the act of lowering a heavy log of wood to the ground, when it slipped from their shoulders, and fell on a little girl who was walking behind them unperceived. The child was knocked down, and died in a few minutes without uttering a cry. The child was about two years and a half old. On an inspection of the body, there was not *the slightest trace of violence to be found externally*. The head was observed to be preternaturally moveable; a circumstance which was afterwards discovered to depend upon an injury to the neck. On cutting into the neck, no extravasated blood was found, but the connexion between the fifth and sixth cervical vertebræ was destroyed. The fifth vertebra had been carried forwards, and its connecting ligaments were stretched without being lacerated: there was no fracture. The spinal cord was not torn, but it had evidently been stretched; and on it, at this part, was found a coagulum of blood. No particular appearances were met with in the chest. When the abdomen was opened, about six ounces of black liquid blood escaped. On removing this, a rent, two and a half inches in length, was found in the anterior wall of the stomach, extending nearly transversely from the cardia to its fundus. The contents of the organ had become partially extravasated in the abdomen. The liver, which was large for a child, was transversely lacerated from before to behind, through the thickest part of the right lobe. No other appearances requiring notice presented themselves.

CASE II. By Dr. HOHNBAUM. A child two years of age was accidentally run over by a waggon and killed. A judicial inspection of the body was ordered to be made. There were slight lacerations on the skin of the scalp, but the skin covering the chest and abdomen was entire, and that of the chest presented only a faint linear ecchymosis. The brain was normal, but its vessels and sinuses were somewhat congested. On opening the chest, the left lung was found entirely torn through in its upper lobe, the upper third of the organ hanging to the lower two-thirds only by a few fibres. The edges of the fissure were as even as if the lung had been cut with a knife: all the vessels were torn through, and about three ounces of dark liquid blood had become extravasated in the cavity. There was no fracture in any of the ribs. The heart and the viscera of the abdomen were healthy. On the left side of the chest, corresponding to the situation of the laceration through the lung, was a slight ecchymosis. The skin covering the intercostal spaces from the third to the ninth rib was of a bluish colour. With this exception the skin over the ribs and dorsal vertebræ offered not the slightest mark of external violence. There was no doubt of the injury having caused death; but the most remarkable part of the case was, the slight marks externally and the absence of fracture in the ribs coupled with so complete a laceration of the lungs. The reasons why the ribs did not break under the violence might be the following. The waggon passed quickly over the chest—perhaps in a moment. The ribs in a young child are soft, yielding, and not easily broken, on which account they may, in resisting fracture, have become pressed more closely against the lung lying beneath. The tendency to rupture in the lungs might have been increased by the terror under which the deceased laboured, having caused them to become filled with air and blood. After discussing the question as to the grade of mortality which this wound should occupy, the reporter states that it must be reckoned among the *absolutely* mortal.

[These cases are deserving of attention, especially the first. Had they occurred in England, it is pretty certain that the valuable inference to which they lead would have been wholly lost; since coroners are not in the habit of summoning medical witnesses, in cases where death is so obviously due to accident. Nevertheless, the question as to how far serious internal injuries are necessarily accompanied by marks of external violence, has been more than once raised in our courts of law; and as we may conceive has sometimes received a negative, at others an affirmative answer. Now, nothing is more certain than that most severe blows may be inflicted upon cavities, especially on the abdomen, without ecchymosis or laceration of the skin resulting. The question is one of fact and not of theory, nor is it to be answered merely from *personal* experience, the occurrence of such cases being purely accidental. Trials for causing death by a rupture of the bladder have taken place, in which the witnesses denied that a blow was struck simply because there was no ecchymosis externally: but no inference could be more erroneous; since there are many cases on record of the bladder or intestines having been lacerated by violence directly applied to the abdomen, and yet the skin had sustained no change to indicate such serious injuries. Portal and Chaussier long ago endeavoured to draw the attention of the profession to this point; but their observations seem to have been unknown to, or neglected by those who have been called on judicially to express an opinion on the subject. If the question were put in a court of law: Can a crushing force produce a laceration of the stomach and liver without leaving marks of its operation externally? many would probably answer in the negative: but the valuable case recorded by Dr. Meyer, supported as it might be by numerous others, shows that such an answer would be highly incorrect and might lead to serious consequences.]

*Henke's Zeitschrift.* 1837.

## ANIMAL CHEMISTRY.

### *On the Chemical Composition of Human Lymph.* By R. T. Marchand and C. Colberg, of Halle.

It is very difficult to obtain a sufficient quantity of pure human lymph to subject to satisfactory examination: and, when it is remembered how great are the differences of opinion respecting the composition of the blood, although this fluid may be obtained in large quantities, and may be examined in the body itself, the difficulties attending a chemical examination of lymph may well be appreciated: and yet a correct chemical analysis of the composition of lymph, as compared with that of the blood, is a subject of considerable importance; for, by this means, the quantity of lymph contained in the blood may be estimated; since the afflux of lymph to the blood, and its existence in that fluid in an unchanged state, (at least the lymph granules,) is a fact which has been satisfactorily proved.

The examination of lymph was conducted by each of the above experimenters, independently of the other. The lymph which was selected for experiment was taken from a wound on the back of the foot, which very obstinately resisted the healing process. The quantity thus obtained was but small; its specific gravity was 1.037. After having been set apart for some time, there collected at the bottom of the vessel a cobwebby coagulum of fibrin, which, when filtered, dissolved in ether, and then dried in a water bath, amounted to 0.52 per cent. The fluid which remained, of a somewhat opaline appearance, and of a light yellow colour, was about the consistence of almond oil. When mixed with thirty parts of water, the appearance was still slightly opaline, but there was no deposit after long standing. Alcohol and bichloride of mercury immediately precipitated tender, white flocculi. The fluid had a strongly alkaline reaction, restoring the reddened litmus; a circumstance of interest, and probably serving to explain the strongly alkaline reaction which is found in purulent matter upon fresh secreting surfaces, which quality ceases in pus taken from these surfaces, which is neutral. When warmed in the water bath to about 204° Fahr., the lymph coagulated completely, from the presence of albumen. Heated to 212° F., and allowed to remain for some time at this temperature, it became a hard grey mass, which could be easily powdered. 6.798 grains left a residuum of 0.209 grains of solid constitutents, = 3.074 per cent. When this residuum was treated with ether, it suffered a loss of 0.018 grains; and, when the ether was evaporated from a watch-glass by the gentlest heat, there remained reddish coloured globules of fatty matter, which, when dissolved in alcohol, and examined by a powerful microscope, appeared to be converted into an oily and crystalline substance. By increasing the temperature, this fatty matter was evaporated; the vapour possessing a very disagreeable smell, and producing considerable irritation of the eyes. The material which had been exhausted by ether was next treated with boiling water, leaving a residuum of 0.065 grains, consisting of albumen and fibrin. The filtered fluid was quite blue; and, after evaporation in a water bath, there remained a whitish yellow, saltish residuum, which, evaporated to dryness, showed a feeble alkaline reaction. Heated to a red heat, there remained a residuum of 0.105 grains, which effervesced with acids, and possessed a much more evident alkalinity; probably owing to decomposition of a lactate of soda, although the existence of a carbonate of an alkali was very probable. In the solution of the residuum, there was produced by nitrate of silver a white precipitate, insoluble in nitric acid, soluble in ammonia. A few drops of the chloride of platinum threw down a slight precipitate, which had increased in quantity on the following day. Chloride of barium caused a copious precipitate, which was partially soluble in hydrochloric acid. In the solution when filtered, and which was quite clear, caustic ammonia again caused instantaneously a copious turbidity. Another part of the evaporated lymph was, as above mentioned, treated with ether, with water, and, lastly, with alcohol. The residuum gave rise to a strong, dark yellow coloration in the outer part of the flame of a blowpipe. In the alcoholic solution, tincture of galls produced a brownish yellow, flocculent precipitate, (osmazome.) The remains of that portion which had been treated with alcohol, water, and ether, and the residue of the alcoholic solution, were evaporated to dryness, heated red hot, and reduced to an ash. This was dissolved in water, which took up all but a few trifling grey flocculi, which, dissolved in a small quantity of hydrochloric and nitric acids, produced a blue precipitate with a solution of the ferrocyanate of potass. This, too, was very marked in the aqueous solution. Oxalate of ammonia produced a precipitate, having the characteristics of oxalate of lime; and, in the residuum of the filtered fluid, the blowpipe proved the existence of soda and potass.

From what has been said, the following may be inferred to be the constituents of human lymph:

| | |
|---|---|
| Water | 96,926 |
| Fibrin | 0,520 |
| Albumen | 0,434 |
| Osmazome (and loss) | 0,312 |
| Fatty oil<br>Crystalline fat | 0,264 |
| Chloride of sodium<br>Chloride of potassium<br>Carbonate and lactate of an alkali<br>Sulphate of lime<br>Phosphate of lime and oxide of iron | 1,544 |
| | 100,000 |

From the above-mentioned experiments, as well as from analyses of lymph both of man and animals by other chemists, it is evident that there exists a very near resemblance between the chemical composition of lymph and of blood. In both there are nearly the same salts; in both are there albumen, fibrin, osmazome, and probably a very similar fat. The proportion of albumen in the blood is, indeed, considerably greater than in lymph; instead of which there appears to be here a much larger quantity of fibrin, but we cannot certainly say that this is identical with that of the blood. The colouring matter of the blood is also wanting in lymph. Meanwhile it cannot be denied that most of the above constituents form also a great part of the other animal fluids, if we except those which are excreted. Albumen and fat we may indeed always meet with, but not fibrine; although it is very probable that this has been overlooked when its presence has not been suspected, as may be concluded from the observations which have been made on fibrinous urine.

*Archiv für Anatomie, Physiologie, &c., von Dr. J. Müller.* No. 2. 1838.

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## *Microscopical Researches on the Composition of Vaccine Fluid.* By M. Dubois.

M. Dubois has found that the crystalline appearance found in vaccine lymph, and alluded to in our eleventh Number, is owing to the presence of hydrochlorate of ammonia: that these crystals form very rapidly, and with great beauty, when the vaccine virus is quite free from any organized matters, bits of membrane, globules of pus or of blood. These crystals were immediately dissipated by the action of heat. M. Pelletier also states that he is satisfied of the existence of hydrochlorate of ammonia in vaccine lymph.

*Bulletin de l'Academie de Médecine.* No. 16. 1838.

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## *Microscopical Examination of Pus, Mucus, and Effused Fluids.*
### By Dr. Mandt.

Beneath the microscope, pus appears to be globules swimming in a fluid. If the globules are separated from the fluid in which they swim, by filtration, the limpid portion which passes through (i. e. the serum,) presents no trace of globules when examined by the microscope. Its appearance is exactly that of albumen dissolved in water. When heated it immediately coagulates, &c. The separation of pus into globules and fluid often takes place spontaneously, when a very fluid pus is left to itself. Mucus and various effusions, the serum which is found in the cellular membrane and cavities in dropsy, &c., are formed in exactly the same manner. The globules which have not passed through the filter are of two kinds: one, commonly spoken of as globules of pus, mucus, saliva, &c., are larger; their diameter being about one-hundredth of a millimètre. When these globules are compared with those of coagulated fibrin, either in the clot or in false membranes, or in the fibrils which are formed by the fibrin of blood when it is agitated with some albumen which prevents the fibrin forming itself into more solid membranes; they are found to be all of the same size, the same appearance, the same form, and they exhibit the same phenomena with chemical agents. Effusions, whatever may have been the time of their continuance, exhibit the same globules. Dr. Mandt proposes to call these *fibrinous globules:* they owe their existence to the fibrin secreted from the blood, and coagulated external to the vessels. Hence it follows, that the presence of these fibrinous globules does not prove that the liquid in which they are found contains pus; and that any inference in favour of the existence of pus drawn from them might induce an error. The second kind of globules, (the diameter of which varies from one four-hundredth to one five-hundredth of a millimètre) which are found mixed with the globules of pus, belong to the globules of albumen coagulated by the salts of the serum. Their number is greater in proportion as the serum abounds in salts. They are sometimes found among the globules of fat of different diameters. It may therefore be inferred that pus and mucus do not differ in their main constituent parts; that water, albumen, and fibrinous globules chiefly constitute them, and that it is but the relative proportion of these parts and the salts which exist in these fluids, which can serve to establish a difference. All the experiments which have hitherto been made to ascertain the distinction between pus and mucus, have therefore been inconclusive. These secretions owe their origin to the principal constituents of the blood, by means of exosmose. They are all found in these fluids, excepting the proper globules of the blood (colouring matter) which are unable to escape from the pores of the blood-vessels, in consequence of their size. It is thus demonstrated that we lose continually by the secretions (the mucus, urine, saliva, &c.) a portion of the fibrine contained in the blood.

*Revue Médicale Française et Etrangère. December,* 1837.

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# II. THE AMERICAN AND COLONIAL JOURNALS.

## MEDICINE.

### *Case of Pneumonia, with Polypiform Concretion in the Heart.*

By Professor DUNGLISON.

THE following case of polypiform concretion proves, with others, that coagula, of the nature of those usually met with in the cavities of the heart on dissection, may form during life, and that the impediment to the circulation through that viscus may be indicated by physical evidences. The semiorganized condition of the concretion was probably owing to the existence of endocarditis or inflammation of the lining membrane of the heart, which facilitated the union between the fibrous portion of the blood and the endocardium or lining membrane of the ventricle; and gave occasion to the semblance at least of organization, which the concretion presented.

Peter Clows, seaman, admitted into the infirmary on the 25th of March. On the 20th, while apparently in perfect health, he was seized with severe pain in the head and vertigo, lasting for some time, and followed by intermittent chills and flushes of heat; great difficulty of breathing, arising, from his account, more from inability to expand the chest than from any particular pain experienced from the attempt. These symptoms continued until his admittance, when his case presented the following appearances. The tongue had a heavy and dark-coloured fur; the eyes were much suffused, and with a yellowish brown tinge; skin hot; great general pain, accompanied with nausea and a considerable degree of languor; breathing very painful, added to a peculiar hitch in respiration; pulse laboured, and very small,—this last symptom arising, no doubt, from the exertion of walking to the house, as in the evening, after some hours' rest, it had become full and bounding; abdomen soft and tender to the touch. Bled on the evening of his entrance, about four o'clock, from a large orifice, to about thirty ounces. Blood, the following morning, presented a thick inflammatory buff. All the symptoms were much relieved by the venesection. As his bowels were very inactive, a powder, composed of fifteen grains of submurias hydrargyri and twenty-five grains of jalap, were administered to him, which brought away, with the most happy effect, a large, dark, and highly offensive stool. As the bleeding of the preceding day had been so beneficial, and as the pain of the head, heat of skin, and difficulty of breathing, still in a measure remained, he was bled, on the morning of the 26th, to about twenty ounces; the blood, after subsidence, also bearing the buffy coat; it was not, however, so heavy as the former. On the application of the stethoscope, the respiratory murmur was totally inaudible in any part of the right lung, very distinct on the left side; percussion on the right gave the dull heavy circumscribed sound of a solid mass; that on the left was hollow and sonorous. The sound on the left side of the heart was so loud as to countenance the idea of dilatation of the cavities with thinness of the parietes; whilst on the right side the sound was indistinct, but there was a peculiar *bruit*, appearing, as it were, compounded of the "bellows' sound" with the *frémissement cataire*, and evidently indicating the existence of some impediment to the circulation of the blood through the right heart. He was put on nauseating treatment; antimonial solution being given every three hours.

As the abdomen was extremely tender to the touch, and as he suffered severely from breathing entirely with the diaphragm, six cupping-glasses were applied, which afforded some slight relief. He complained of a great deal of uneasiness about the heart, which the previous treatment had but slightly benefited. Towards evening, the symptoms became very unfavorable, the anxiety about the præcordium was much increased; pulse very irritable, and so quick as not to be counted; skin covered with a cold clammy sweat; the stupor and coma, which from the first had been prominent symptoms in the case, were much aggravated. In short, he was evidently dying; stimulants were diligently administered through the night, but without effect; he died next morning at eleven o'clock.

*Necroscopy*, four hours after death. Skin extraordinarily hot. On examining the chest, the right lung was found twice as large as in the healthy state, and completely hepatized throughout its whole extent. On cutting into it, a thin muco-purulent matter exuded from every part of it. The left lung, though exhibiting some marks of inflammation, was otherwise healthy throughout. On taking out the heart, the right pulmonary artery was found completely plugged up by a semi-organized mass, having the appearance of muscle, and extending through the semilunar valves, and attaching itself strongly to the columnæ carneæ of the right ventricle. Whether this production was caused by the engorgement of the lung, thus giving rise to a remora of the blood and deposition of fibrine, I do not pretend to say; it is, however, an interesting fact, and may throw some light on the nature of polypus of the heart. From the symptoms and auscultation during life, and its organized appearance, it certainly could not have been formed during the last struggle. *American Medical Intelligencer. July* 1, 1837.

## SURGERY.

### *Operations for the Excision of the Ribs.* By Dr. Warren, of Boston.

Case 1. The patient was a gentleman, forty years of age, a patient of Dr. Bigelow. In the early part of his life he was subject to severe attacks of asthma; this he has been free from the last twelve years, during which time he has enjoyed good health, until the summer of 1834, when he had the typhus fever. On his recovery, after an illness of six weeks, he was troubled with a severe pain in the right side, in the region of the angle of the sixth and seventh ribs. A small tumour shortly made its appearance at this point, which, after having remained stationary for some months, finally opened and discharged a quantity of sanious pus. In the spring of 1835, Dr. Warren was consulted on account of the fistulous cavity which had remained since the opening of the abscess. Upon examination, a small aperture was discovered on the side of the chest, large enough to admit a common-sized probe, and situated over the point where the sixth and seventh ribs are joined to their cartilages. Upon passing a probe into this aperture, it penetrated rather more than an inch in a direction downwards, and backwards, where it was resisted by a firm, but not osseous substance. This examination gave great pain. During the following winter, the pains about the ribs increased, the respiration began to be more or less affected, and the patient was occasionally troubled with hiccough.

Finding himself prevented from attending to his business, gradually losing strength, and, added to this, much suffering attending the disease, he finally determined to submit himself to an operation. The patient was placed on a table, in a horizontal position. An incision four inches in length, was made in an oblique direction, over the sixth rib, joined by two incisions at either extremity of the first, so as by this means to form two quadrangular flaps. The integuments were now dissected up, a matter of some difficulty, from their very strong adhesions to the parts below, and a firm cartilaginous substance exposed, forming a large tumour, destroying the natural appearance of the parts, and rendering it difficult to determine in which direction the diseased ribs and cartilages lay. After much time and patience, this substance was carefully removed, partly by shaving it off with the scalpel, and by occasionally removing portions of it with the cutting forceps, and the diseased parts at length exposed. Both the sixth and seventh ribs were found to be carious, their cartilages also being involved. The ribs were now carefully isolated from the surrounding parts, a probe passed under them, and the pleura, which was much thickened, and a part of the diaphragm, separated from their internal surfaces. Three inches of the seventh rib, and its cartilage, with two inches of the sixth, were removed by the chain-saw and cutting forceps. There was very little hemorrhage, and no appearance of bleeding from the intercostal arteries, which undoubtedly had become obliterated in the course of the disease. The external wound, which had been somewhat enlarged in the course of the operation, was brought together by two or three points of suture, and by adhesive straps. Although the operation lasted a long time, and some parts of it were exceedingly painful, it was borne with great fortitude. The patient did not seem greatly exhausted.

On the second day after the operation, he had a severe pleuritic attack. This yielded to a copious bleeding, and in the course of two weeks he was able to walk about his chamber. From exerting himself too much, he was attacked with an erythematous affection between the wound and os ilii. This terminated in a partial suppuration. From the consequences of this attack he very slowly recovered. The following summer he went into the country, still suffering occasionally with some pain about the ribs. During his residence in the country, his health gradually improved; and on his return to town, the wound had entirely healed, and he is now in the full enjoyment of health.

One of the greatest difficulties in the operation was the removal of the cartilaginous formation, which covered the ribs, and destroyed all the anatomical signs which were necessary for prosecuting the operation with anything like facility. This occupied much time, and the parts, contrary to what would naturally be supposed, were endowed with an excessive degree of sensibility. A portion of the diaphragm and pleura, was exposed by the operation. The latter, during the slow inflammation which had been going on, had become greatly thickened; if this had not been the case, it would have been very difficult to have separated the pleura, without cutting into the cavity of the chest.

CASE II. The patient was a stout, healthy person, a surveyor, from Upper Canada. Six years since, and without any previous injury that he was aware of, a small hard tumour appeared over the angle of the ninth or tenth rib. From that time to the present, it has gradually increased in size—having, however, daily less of that bony hardness, which characterized it at first.

It was of a circular form, about six inches in diameter, and having an elevation.

above the ribs of between two and three inches. Its situation was on the lower part of the chest, covering a portion of the seventh, eighth, ninth, and tenth ribs, to all of which it appeared attached, but more firmly to the ninth, which was most affected by its movements. The skin covering the tumour was perfectly natural; both in colour and consistence, and the patient complained of no inconvenience or suffering from the disease.

The operation was performed on the 27th of March, 1837, in the following manner. The patient was placed on a table a little inclined to the right, with a pillow under the chest, so as to cause a projection of the left side of the thorax. The operation was commenced by a longitudinal incision about five inches in length, directly over the tumour. This was joined by another transverse incision at right angles with its central part—the two incisions being well represented by the letter T. The flap thus formed being dissected up, the insertions of the external oblique were exposed and dissected off, not without some difficulty, however, on account of its strong contractions. The latissimus dorsi was now discovered passing over the outer edge of the tumour, and the same, and in fact more trouble was experienced in dividing this muscle than the preceding; the dissection of it was excessively painful to the patient, and was followed by some hemorrhage. The tumour being at length perfectly exposed was found to originate from the ninth rib, but was strongly adherent to the seventh, eighth, and tenth. A knife was now carefully insinuated under the tumour, and its adhesions to the ribs dissected off, great care being taken not to cut through the intercostal muscle and penetrate the chest. As the attachment of the tumour to the ninth rib was not by its whole base, it was thought that more of the rib might be saved by first detaching the tumour and afterwards cutting out the rib, than by removing both the tumour and rib together. It was therefore cut off from the rib at about an inch distance from its cartilage, and the morbid origin of the tumour exposed. The intercostals were now cut through, the diaphragm carefully separated from the rib and pleura, and a director passed under at the points where the rib was to be divided. The bone was next cut through with the cutting forceps, and about two inches in length of it removed, with a portion of its cartilage. The diaphragm immediately rose up, forming a hernia between the ribs. The hemorrhage was not great—most of it being from the divided muscles. No artery bled sufficiently to require being secured by ligature. The wound was brought together by sutures, and adhesive plasters. The patient was very little exhausted by the operation. A considerable degree of febrile excitement followed, requiring the employment of copious bleeding. The wound has united nearly throughout by the first intention, and the patient is rapidly gaining strength, without the occurrence of any bad symptoms.

In this case, unlike the preceding, very little thickening of the parts lying under the ribs had taken place. This rendered the difficulty and danger much greater in separating the pleura and diaphragm from their adhesions to the ribs, which, however, was finally accomplished without penetrating the chest.

*Boston Medical and Surgical Journal.* May 3, 1837.

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### *Two Cases of Double Amputation of the Legs, from Frost-Bite.*

By J. A. Sewell, m.d., Surgeon to the Hôtel Dieu, Quebec.

I. James Brown, æt. twenty-seven, was admitted into the Hôtel Dieu in May, 1833, under the care of Dr. Morrin. He was a sailor, and had been shipwrecked the previous autumn in the Gulf of St. Lawrence, and was exposed for a considerable time to intense cold by which both legs were frozen: he got with difficulty to the nearest habitation, where he remained all the winter without medical assistance, enduring extreme privations and sufferings. On admission, both limbs were found to be in a complete state of sphacelation, the lower ends of the tibiæ and fibulæ, together with the bones and ligaments of the ankle joints being entirely exposed, and his whole general appearance indicating great constitutional disturbance. It was at once apparent that amputation alone could save life. The question, however, whether one limb should be removed, and some time allowed for the system to rally before the second operation should be undertaken, or, on the other hand, whether the double amputation should be simultaneously performed, could not so readily be decided. After due deliberation it was determined that the latter plan should be adopted, and the two operations were accordingly performed at the same moment, by Dr. Morrin and the late Dr. Hall. The operations occupied about the usual time, and the man recovered without one untoward symptom, and was discharged perfectly cured within the month.

II. A young Canadian came under Dr. James A. Sewell's care in March last, in the Hôtel Dieu. He was of very intemperate habits, and one night in January, when intoxicated, lost his way on the ice between Quebec and the island of Orleans. He was found the next morning with both feet completely frozen. When Dr. S. first saw him, mortification had extended about midway up each leg, the bones of which were, to a considerable extent, perfectly denuded, the feet remaining attached by the lateral ligaments alone. He was at this time losing ground rapidly; the appetite entirely gone, with profuse night sweats and colliquative diarrhœa; he expressed an earnest desire for the immediate removal of the legs. Double amputation, as in the foregoing case, was accordingly performed by Drs. Sewell and Perrant. The first dressings were removed on the fifth day, when both stumps were found almost completely united by the first intention. The man recovered rapidly and perfectly, and was discharged in about five weeks from the day of the operation.

[Whether it is advisable to perform double amputation in cases requiring it, simultaneously or successively, is a question to be decided by experience alone: these cases, as far as they go, are valuable data towards forming an opinion, and they do much credit to the surgical skill and intrepidity of Dr. Sewell and his coadjutors. It should be remembered the patients although reduced were both young men.]

*The Boston Medical and Surgical Journal. November* 15, 1837.

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# III. THE BRITISH JOURNALS.

(FOR THE QUARTER ENDING AUGUST 31, 1838.)

## PHYSIOLOGY.

### *Observations on the Fluid in the Vesiculæ Seminales of Man.* By JOHN DAVY, M.D. F.R.S., Assistant Inspector of Army Hospitals.

[THIS is an interesting communication, and adds considerably to our knowledge of an obscure subject. Like all the writings of Dr. Davy, it bears the stamp of accurate observation and sober deduction. Its object is to endeavour to settle the difference of opinion existing among physiologists respecting the nature of the fluid of the vesiculæ seminales; viz. "whether it is secreted by the testes or by the vesiculæ; and whether, in consequence, the vesiculæ are to be viewed chiefly as reservoirs or merely as glands?" With the view of throwing light on this subject, Dr. Davy examined the fluid in the vesiculæ and in the vasa deferentia after death, in a variety of cases at the Military Hospital at Chatham, of which he is superintendent. Twenty of these examinations are here detailed. The following extracts exhibit, 1, the inferences respecting the questions in dispute, deducible from the observations; 2, the effects of disease on the spermatic fluid; and, 3, the important bearing of such enquiries on certain questions in medical jurisprudence.]

1. The first inference that appears to me unavoidable is, that the vesiculæ are seminal reservoirs, according to the old opinion on the subject, and that which is still most commonly entertained by the continental physiologists: and next, that they are not merely reservoirs, but are also secreting organs, furnishing mucus, and perhaps some other fluid, for admixture with the semen.

The first inference is supported by the general resemblance, in several cases, of the fluid of the vasa deferentia and of the vesiculæ, and of the existence of the characteristic spermatic animalcules in the fluid of the vesiculæ, in every instance in which they were detected in the fluid of the vasa deferentia. Hunter does not mention having used the microscope in his enquiry. If he had, he could hardly have failed to have arrived at a different conclusion.

The second inference is supported by there being a certain difference in almost every case between the fluid of the vesiculæ and that of the vasa deferentia, and especially by the circumstance that the difference of quality is most perceptible in the fluid of the fundus; where most out of the way of being readily mixed with the fluid of the testes. What the exact difference of qualities is between the fluid of the vesiculæ and of the vasa deferentia, and, it may be added, of the vasa deferentia and of the testes, in perfect health, remains to be ascertained. It can be determined only by careful examination and comparison in the instances of criminals who have been executed, or of persons who have been killed by accident, not labouring under chronic disease, and in the vigour of life. I am disposed to think that the difference will not be found very considerable; and that between the fluid of the vesiculæ and of the vasa deferentia it will consist chiefly in the former being more dilute, and perhaps more bland and mucous. . . .

2. Relative to the effects of disease on the fluid of the vesiculæ seminales, and on the spermatic fluid generally, the instances brought forward are too few to admit of extensive induction. They seem to show, first, that chronic wasting diseases, terminating in death, arrest the secretion of the testes, or the production of those animalcules, on which there is much reason to infer the active power of the semen depends; 2dly, that the contents of the vesiculæ and vasa deferentia, under the influence of disease, retain longer their characteristic qualities than the contents of the tubuli; and, 3dly, that there is least fluid in the vesiculæ and in the vasa deferentia, and that it is most altered in instances of chronic diseases of the abdominal viscera, and especially of the intestines. . . .

3. Admitting that spermatic animalcules are characteristic of and essential to healthy spermatic fluid, in certain doubtful criminal cases, probably decisive evidence may be obtained by means of microscopical examination. The spermatic fluid undergoes change rapidly when exposed to the air, and even soon becomes putrid; but the spermatic animalcules, I find, resist change in a remarkable manner. In one instance distinct remains of these animalcules were observed in putrid fluid, which had been kept ten weeks, at a temperature varying between fifty and sixty degrees of Fahrenheit. In another instance some fluid of the vesiculæ was applied to linen, and wrapped in paper, and put by in a close drawer. It was examined the following day, at the end of a week, and after eighteen days; and each time animalcules were discovered under the microscope. The mode of making the trial was by saturating a small portion of the smeared linen with a few drops of water, and gently pressing out a drop for the experiment. Fragments of the animalcules were very distinct, and sufficiently characteristic; and, on careful inspection, an entire animalcule, here and there, was observed. The application of these facts to the purposes of evidence does not require any comment.

*Edinburgh Med. and Surg. Journal. July* 1, 1828.

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## *An Experimental Enquiry into the Influence of Nitrogen on the Growth of Plants.* By ROBERT RIGG, Esq.

THE author, after briefly alluding to a former paper laid before the Royal Society, describing the chemical changes which occur during the germination of seeds, and some of the decompositions of vegetable matter, proceeds, in the present paper, to trace a connexion between the phenomena exhibited during the growth of plants, and the direct agency of nitrogen. The experiments by which the author supports his views are arranged in separate tables, so drawn out as to indicate not only the quantities of carbon, oxygen, hydrogen, nitrogen, and residual matter, in about 120 different vegetable substances, but also the quantity of nitrogen in each compound, when compared with 1000 parts by weight of carbon in the same substance. The most important of these tables are those which exhibit the chemical constitution of the germs, cotyledons, and rootlets of seeds; the elements of the roots and trunks of trees; and the characters of the various parts of plants, especially of the leaves, at different periods of their growth. From this extensive series, which is stated to form but a small portion of the experiments made by the author in this department of chemical research, it appears that nitrogen and residual matter are invariably the most abundant in those parts of plants which perform the most important offices in vegetable physiology: and hence the author is disposed to infer that nitrogen, (being the element which, more than any other, is permanent in its character,) when coupled with residual matter, is the moving agent, acting under the living principle of the plant, and moulding into shape the other elements. The method of ultimate analysis adopted by the author enables him, as he conceives, to detect very minute errors, and therefore to speak with certainty as to the accuracy and value of every experiment.

*Proceedings of the Royal Society.* *May* 31, 1838.

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### *Researches on Suppuration.* By GEORGE GULLIVER, Esq., Assistant Surgeon to the Royal Regiment of Horse Guards.

THE author, in consequence of some theoretical views of the suppurative process, was led to undertake an examination of the blood in the different forms of fever accompanying inflammation and suppuration; and the result has been the detection of globules of pus in that fluid in almost every instance where there had existed, during life, either suppuration or great tumefaction of the external parts without the presence of pus. The means by which he detected pus in the blood were partly chemical, and partly by the aid of the microscope. Availing himself of the solvent power which water exerts on the globules of the blood, while it has no action on those of pus, he had merely to dilute the suspected blood sufficiently with water, by which means the red globules were made to disappear, while those of pus remained at the bottom of the fluid, and were easily recognized by a good microscope. A number of cases are detailed, from which the general result above stated was deduced. He considers that his experiments tend to establish the conclusion that suppuration is a kind of proximate analysis of the blood. As the fibrin separated from this fluid produces swelling of the part affected, or is attracted to the contiguous tissue for the reparation of the injury, the globules of the blood, altered by stagnation, become useless, and are discharged as excrementitious matter from the system. Such is the constitution of healthy pus; but, when mixed with broken-down fibrin, it assumes the flaky and curdled appearance, with proneness to putrefaction, characterizing unhealthy pus, and the presence of which in the blood is connected with fevers of the inflammatory or typhoid form.

*Proceedings of the Royal Society.* *June* 14, 1838.

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### *On the Differences of the Laws regulating Vital and Physical Phenomena.* By WM. B. CARPENTER, M.R.C.S., late President of the Royal Medical and Royal Physical Societies, Edinburgh.

[THIS paper constitutes a portion of the essay to which was recently adjudged the prize annually raised by the contributions of the students, and awarded by the professors of the university of Edinburgh. It is, like all the writings of Mr. Carpenter, marked by views at once clear and comprehensive; and gives still further evidence of the author's remarkable power of handling abstruse physiological subjects at once logically and agreeably. We are happy to learn that Mr. Carpenter has in the press an original work on "General Physiology;" a subject hitherto most imperfectly treated, and one which, we feel assured, will assume a striking degree of interest in his hands, and fill up a blank, not merely in the course of study of the medical man, but in that of men of science generally. We regret extremely that our limits will permit us to lay before the reader no part of the present valuable contribution, except the *general conclusions* deduced by the author from his enquiries: these are as follows:

1. That the *properties* of any aggregation of matter depend upon the method in which its ultimate molecules are combined and arranged.

2. That the simplicity of our notion of the properties of inorganic matter depends upon the facility of our becoming acquainted with them through the command which we possess over the agencies by whose operation they are manifested.

3. That the vital properties of organized tissues are not less the result of their material constitution; but that, whilst the *materials* of an organized tissue may be prepared by the operation of the ordinary laws of affinity acting under peculiar conditions, the tissue cannot be constructed without the agency of a previously existing vitality; and that hence man is debarred from the most advantageous means of becoming acquainted with the laws of physiology.

4. That *vital properties* are not *added* to matter in the process of organization; but those previously existing, and hitherto inactive, are called out or developed.

*Edinburgh New Philosophical Journal. April*, 1838.

## PATHOLOGY, PRACTICAL MEDICINE, AND THERAPEUTICS.

### *On the State of the Pupil in Typhus, and the Use of Belladonna in certain Cases of Fever.* By Robert J. Graves, M.D.

[This is an interesting and ingenious paper; and, like most of the same author's productions, is characterized by an acute spirit of observation and by originality of views. We regret that our limits permit us to make only a few extracts from it; but these will suffice to show the nature and value of the communication.]

There are two classes of narcotic remedies, which are known to possess alike the power of allaying nervous excitement, abating pain, and producing sleep; but whose effects on the state of the eye are very different, one class causing contraction, the other dilatation of the pupil. Among the former, opium holds the highest rank; among the latter, stramonium and belladonna.

It appears certain, that the state of the brain, attended with contraction of the pupil, must be very different from that which is accompanied by dilatation. In some diseases of the brain, we find the pupils of very small dimensions, in others dilated, and almost wholly insensible to the stimulus of light. It is obvious that these two opposite conditions of the pupil cannot depend on the same condition of the brain. If it be asked, on what particular conditions of that organ do they depend, the answer is, we cannot tell. Pathologists have indeed written and reasoned a great deal on this subject, but they have discovered nothing; they have found nothing on post mortem examination sufficient to explain, in a satisfactory manner, why, in one case of fever with cerebral affection, we have contraction, in another, dilatation of the pupil.

In fever with cerebral disease, one of the most alarming symptoms is marked contraction of the pupil, and were I called to a case in which every other symptom was favorable, but great contraction of the pupil present, I would say that it was a case of extreme danger. A tendency to even moderate contraction of the pupil is a very dangerous symptom in typhus, but a pupil extremely and permanently contracted, or as it has been called a pinhole pupil, is, or used to be, a fatal sign. Although this symptom is so obvious, so easily ascertained, and unhappily so common, for I have within the last few years witnessed it in a great number of fatal cases, yet, strange to say, it has not attracted the attention it deserves, among writers on typhus. No particular notice is taken of it by any author on Irish fever that I have seen. By some it may be mentioned incidentally, but by none is the state of the pupil treated of specially, and in detail.

Whenever, in attending a case of fever, you meet with this contracted state of pupil, even in a slight degree, although your patient may be restless and greatly in want of sleep, beware of opium. I have often seen it tried, and I think scarcely ever without more or less injury to the patient. When opium is administered to a patient in the advanced stage of fever, with symptoms of cerebral derangement, and tendency to contraction of the pupil, you will find that the pupil which has been moderately contracted to-day, will be greatly contracted to-morrow, and that the patient will soon sink into an irrecoverable state of coma. This contracted state of the pupil may exist in its extreme and most marked form in typhus fever, without being necessarily accompanied by headach and delirium; the patients are restless, sleepless, and in a state of remarkable nervous excitement; but they answer questions not unfrequently in a tolerably clear and rational manner, and many of them distinctly affirm that they have no pain in the head. These circumstances may deceive the unwary, but the experienced practitioner, who has witnessed many such cases, will feel that a fatal termination is threatened. Under these circumstances, opium in every shape is injurious, and even tartar emetic fails in controlling or diminishing the pernicious effects of opium. This is somewhat curious, as the combination of tartar emetic and opium seldom fails in relieving cases similar in all respects, except the symptom of contraction of the pupil.

It may appear somewhat extraordinary to think of prescribing for a single symptom, and giving belladonna in a case of fever with contraction of the pupil, merely because it produces dilatation of the pupil, but, as was before observed, it is not unreasonable to suppose that the state of the brain which accompanies dilatation of the pupil is different from that which accompanies contraction, and if belladonna has an effect in producing that cerebral state which is attended with dilatation, it is not going too far to infer, that its administration may do much towards counteracting the opposite condition; neither is it unphysiological to conclude, that if a remedy be capable of counteracting or preventing one very remarkable effect of a certain morbid state of the brain, it may also counteract other symptoms connected with the same condition. Thence it was that I was induced to try belladonna in the advanced stage of fever, accompanied by marked cerebral irritation, and a contracted state of the pupil.

I shall now proceed to make a few remarks with respect to the administration of belladonna in cases of cerebral excitement in fever, accompanied by contraction of the pupil; of stramonium I shall say nothing, having as yet no experience of its effects. I have used the belladonna by itself, or combined with musk, or with opium, or opium and tartar emetic. It is of course of the greatest consequence that the extract of belladonna employed should be extremely good. Its efficacy can be determined, and that in a very short time, (a few minutes at most,) by applying a little, mixed with a sufficient quantity of water, to render it semi-fluid to the eye of a healthy person. If it produces immediate dilatation of the pupil it can be relied on.

Stramonium is also a very powerful narcotic, and like belladonna, must be carefully watched when given internally. The dose of each should, in fever, be small, but may be repeated until the desired effect is produced. Of course belladonna cannot be expected to succeed in all cases, with contraction of the pupils, for, in several, the coexistence of other symptoms precludes the hope of recovery.

Batley's sedative, the black drop, morphia with its various salts, and in short all known preparations of opium, seem to exercise the same influence on the pupil as opium itself, and consequently are all equally objectionable in those cases where the pupil is contracted, or evinces a tendency to become so. Whether this tendency can be neutralized by combining belladonna or stramonium with them, forms a most interesting subject for clinical experiment. As far as I can judge from the result of the cases in which I have tried this combination, it promises to be a most useful addition to the remedies usually employed in fever. This combination evidently saved the life of a gentleman in Parliament-street, whom I attended along with Mr. Sibthorp and Dr. Dwyer, and whose case was apparently hopeless. In the Meath Hospital, draughts consisting of black drop, belladonna, and tartar emetic, have been eminently serviceable; the extract of belladonna which I have used, is, properly speaking, an inspissated juice, and is so called in the last edition of the Dublin Pharmacopœia.

[Two cases successfully treated on this plan are given ;and the author moreover informs us that he has "witnessed others in private practice, and in the Meath Hospital, in which belladonna seemed to produce very favorable results."]

*Dublin Journal of Med. Science. July* 1, 1838.

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### *A Statistical Enquiry on Fever, being an attempt to ascertain the prevalence, susceptibility, intensity, and prognosis, with some observations on the influence of Medical Treatment.* By ARTHUR SAUNDERS THOMSON, M.D. &c.

THIS is a very elaborate paper and most creditable to the ingenuity and industry of the author. We can only find room for the "*Concluding Results*," which are as follows:

1. That the annual ratio of deaths from fever in London, have decreased since the commencement of the eighteenth century. 2. That the susceptibility to be attacked by fever is greatest among individuals under ten years of age, and from twenty to thirty. 3. That the period of life during which the highest ratio of mortality occurs from fever is from forty to fifty. 4. That there is no very apparent difference in regard to one sex being more susceptible to fever than the other. 5. That the annual ratio of deaths by fever is nearly twice as great among the male as the female population. 6. That there is about one death for every fifteen persons attacked by fever. 7. That the intensity of fever increases with the age of the patient about thirty-four per cent. every decennial advance in life. 8. That attacks of fever are one-third more intense among males than females. 9. That fever is most prevalent from July to December inclusive. 10. That the intensity of fever is much greater during January, February, March, April, and May, than at any other part of the year. 11. That, during those months fever is most prevalent, the temperature and quantity of rain is considerably greater than during those months fever is not so prevalent. 12. That, during those months fever is most intense, the temperature and quantity of rain is comparatively low. 13. That medical treatment has a powerful effect in lessening the danger or number of deaths from fever. 14. That early medical treatment shortens the duration of fever. 15. That the mean duration of fever among individuals under forty is shorter than among those above that period of life. 16. That the general prognosis of fever is favorable, there being fourteen chances to one that the patient will recover. 17. That the prognosis of fever becomes less favorable as the patient is advanced in life, the intensity of the disease being nearly twice as great at forty-one years of age as at twenty-one. 18. That the prognosis of fever is one-third more favorable among females than males. 19. That the prognosis of fever is more favorable from June to December than from January to June. 20. That the prognosis of fever is one-half more favorable among patients who come under medical treatment before the seventh day of the disease, than those who are admitted at a later period. 21. That the prognosis of fever is unfavorable when there are cerebral or thoracic complications. 22. That the second week of fever is the most dangerous. Out of a thousand cases passing through this week eighty-two died.

*Edinburgh Med. and Surg. Journal. July* 1, 1838.

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### *On the Treatment of Rheumatic Gout.* By A. L. WIGAN, Esq., Surgeon, Brighton.

[THIS practice as recommended by a gentleman of long experience, we recommend to the notice of our readers: we ourselves have had no experience of the remedy as employed by Mr. Wigan.]

The powdered root of colchicum is the specific on which I depend. It is an old remedy, but the mode of administration is new, and entirely my own. On no one of the many occasions on which it has been used, have I seen the slightest injurious consequence; and I do not now hesitate to pronounce it the most easily managed, the most universally applicable, the safest, and the most certain specific, in the whole compass of our opulent Pharmacopœia; the mode of exhibition being an effectual regulator of its influence, adapting it accurately to the varying temperaments of different patients. Administered in the way I prescribe, and with the limitations stated, I feel quite confident that no one will be disappointed in the results to be expected from it. No doubt there may be complications of this disease, which may render other measures adviseable: cases of plethora, or of inflammatory action, so decidedly marked as to leave no doubt of the propriety of taking away blood. It may be so; I have seen none such; at least none in which I have thought it necessary to use the lancet, though I see no disadvantage in erring on the safe side, and just relieving the tension of the blood-vessels by moderate depletion. Large bleedings I believe to be always injurious; they seem to me to aggravate the tendency to metastasis. The cases which require bleeding must be extremely rare, since not one has presented itself to me during the last six years. I am sorry to say, that when this disorder has gone on for some time, when it has been allowed to spend its violence on the articulations, and especially those of the vertebræ (the most frightful form of the disease), during a fortnight or three weeks, the remedy is by no means so effectual. Still it has a great influence in alleviating the pain and in shortening the duration of the malady, although the transition from intense suffering to perfect ease is less sudden and decided. I have uniformly remarked, that the more violent the attack the greater the number of articulations under its influence; the higher the fever, and the more general the disturbance, the more speedy and the more perfect is the cure. Were I to give details of some of the cases, I should not obtain belief, so much would the statement resemble a quack-doctor's advertisement. I will, however, venture to assert, that on many occasions I have seen the patient one day unable to turn in bed, with ankles and wrists so swollen and inflamed, and even the vertebræ affected to such a degree, that he has entreated me to walk gently, as the mere shaking of the bed produced an agony of suffering; I have seen, I say, a patient in this state, and thirty-six hours afterwards not only perfectly free from pain, but able to walk across the room without assistance.

If the bowels be loaded, I begin by an enema of decoct. aloes, but this does not delay the use of the colchicum. The dose, eight grains every hour, taken in the medium most acceptable to the patient—plain water, sugar and water, apple tea, ginger tea, or other analogous fluids, changing them from time to time, even with the successive doses, if necessary, according as the stomach is more or less irritable, or the palate more or less capricious. The point of saturation, as I call it, (for want of a better term,) is very uncertain, varying so much with different individuals, that although the usual quantity is eight or ten doses, I have known some take fourteen, and others unable to bear more than five. In every case it is to be repeated till active vomiting, profuse purging, or abundant perspiration take place; or at least till the stomach can bear no more. If a slight nausea comes on after three or four doses (I have never seen it so soon as the fourth), a quarter of an hour's delay may be allowed. A lump of sugar, dipped in brandy or eau de Cologne, a wine-glass of soda water, or any thing else the patient wishes for, in small quantity, may be given. Sometimes a small slice of lemon kept in the mouth will turn away the nausea, and enable him to bear a few more doses; the main object in all cases being to get into the stomach the largest quantity that it can be induced to receive. Even two doses taken at once would be rejected by a patient, who will thus gradually bear a dozen. The most usual course of things is this. At the end of the sixth or seventh dose a slight nausea comes on. By keeping quite still, turning away the thoughts by conversation, or listening to an amusing book, coaxing the palate with a slice of lemon, a clove, or some such thing, three or four more doses can be received, when the disgust becomes perhaps unconquerable. After this there is generally sound sleep, with occasional nausea on waking. The pain ceases, but the more active effects of the colchicum do not take place for some hours after the last dose. Distressing as is the state of the patient when under the full influence of the medicine, it still does not exceed an ordinary sea-sickness; and when this has been endured for a few hours, it is succeeded by the Elysium. The inflammation of the joints subsides, and they resume their natural size with miraculous rapidity. The acidity of the perspiration ceases, as well as the peculiar odour, which enables the experienced practitioner to recognize the disease on entering the room, before he has asked a single question. As soon as a cup of souchong tea can be retained, a sound sleep comes on, from which the patient awakes perfectly well. When circumstances will admit of it, I prefer to give a breakfast of bread and butter and tea only, very early in the morning, and two hours afterwards commence the colchicum. No more food will be required that day, but tea may be given abundantly, with bread sopped in it, if required. It will be well to indulge the returning appetite very sparingly on the day following, on which, however, we may allow a small snap of devilled meat, and rice boiled plain as for curry, which will generally be the things most acceptable to the stomach. A small quantity of good curry itself is not objectionable to those who have been accustomed to that luxury. In the subsequent treatment I have no reason to think that any precaution is necessary. The patient may resume his ordinary diet as soon as the appetite dictates. I have never seen a relapse.

The colchicum, it is obvious, should be preserved with care. The best mode, I believe, is to grind it, at the proper season, with twice or thrice its weight of fine sugar, into an impalpable powder, when, should it accidentally become damp, it is safe from injury.

It is better, if possible, that the patient should not be aware of the direct effects expected until they take place, in order that the imagination may not anticipate and interfere with the process. *Lancet, and Med. Gazette. June* 30, 1838.

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### *New Mode of Exhibiting Copaiva.* By Charles Evans, Esq.

The following is the method I have generally adopted, first premising that the article used must be good copaiva, not such an article as many druggists supply under that name:—Put any weight of copaiva into a vessel which will admit of its being conveniently stirred, and gradually add to it, constantly stirring until they are thoroughly incorporated, one-sixteenth of its weight of pure calcined magnesia; set it by for a few days, and it will then be found of sufficient consistence to admit of its being formed into pills or boluses of any shape. When about to use it, wet the hands, spatula, and slab, with water, which will prevent the mass adhering to either: form half-drachm oval boluses; then cut a slip of wafer-paper, such as confectioners use for baking biscuits on; moisten it with water, and roll the bolus in it.

Before taking one, place it in water for a minute or so, for the purpose of softening the envelope, and it will then slip down the œsophagus much easier than a small pill, and leave no trace behind it in the shape of a disagreeable taste: in fact the dose is perfectly insipid, and in this form it seems to be more grateful to the stomach, for I never have observed that loathing of the medicine and food which so frequently occurs during the ordinary exhibition of this medicine.

*Med. Gazette. July* 7, 1838.

## SURGERY.

### *On the Primary Causes of Strangulation, and an improved Mode of performing the Taxis, in Cases of Intestinal Herniæ.* By James O'Beirne, M.D., one of the Surgeons to the Richmond Surgical Hospital, &c., Dublin.

[This is a most valuable practical paper and deserves the attention of all surgeons, although it will present but little novelty to those who are acquainted with the author's excellent work, published some years since, and entitled "New Views of the Process of Defecation." The present article, however, gives more detailed views of the principles and practice laid down in that work respecting the reduction of hernia, and contains, moreover, "the whole of the cases, published and unpublished, successful and unsuccessful, in which the practice has been employed during the last eight years." These cases are sixteen in number, and exhibit, according to Dr. O'Beirne, "positive proofs of this mode of treatment having obviated the necessity of an operation in eleven out of sixteen cases of complete strangulation, where the usual and most efficacious means had previously been resorted to in vain." The following extracts explain Dr. O'B.'s views of the disease, and his peculiar mode of practice.]

Having before us this view of the strangulated and other portions of the intestinal tube, we are enabled to see at once, that, in such cases, various forces oppose our attempts at the taxis, and that these forces are as follow, viz.: the great size of the hernial tumour, compared with that of the ring—its almost immoveable attachment to the latter—its great firmness and incompressibility, its enlargement above as well as below the stricture—*the constant current and pressure of flatus into the constricted and distended intestine*—the acute angles formed by the convolutions within the abdomen, and immediately above the stricture, and the extremely unyielding nature of the ring itself. The consideration of such numerous, various, and powerful forces acting in opposition to our attempts at the taxis, is well calculated to show the difficulties and uncertainty in effecting reduction; the injury likely to be done by using much force; and the little power which the hands, however judiciously applied, can exert over a tumour so situated. It is true, that notwithstanding the opposition of all these forces, such attempts have often proved successful; but I apprehend that in such instances, their success was more owing to an occurrence not observed or enquired into, and which I shall notice hereafter, than to any peculiar tact or manipulation on the part of the operator. But the question which we have now to consider is, how are all these forces and difficulties to be overcome? We see that they are all but the products of distention of the strangulated intestine by flatus, and we know that we can evacuate the flatus by dividing the stricture, or by puncturing or cutting into the intestine itself. But as our object is to avoid the first, while the two last can only be thought of in an argument, the very simple question suggests itself, can we, by any other means, empty the bowel of its gaseous contents? This naturally leads us to survey the state in which the whole of the intestinal canal is placed; and we find, first, that the cavity of the strangulated gut is not obliterated, but permits air to pass freely into and out of it; secondly, that all that portion of the small intestines within the abdomen, and the whole of the cæcum and colon, are dilated, and also afford free passage to air; thirdly, that the rectum is firmly contracted, and that it alone opposes the exit of this elastic fluid. The instant that we obtain this view of the facts, it becomes manifest, that the great object in view may be quickly obtained, merely by introducing a gum elastic tube through the rectum, into the colon, and retaining it in that situation until the flatus contained in the large and small intestines, and ultimately that in the strangulated intestine, is completely evacuated, and the tumour so diminished in size and tension, as to start up into the abdomen, as it often does, *proprio motu*, or be easily reduced by the taxis. But to complete this view, it is necessary to consider the circumstances which may occur, either to cause the complete failure of this mode of assisting the taxis, or merely defeat the first attempt at its employment, and afterwards prevent its complete success. If the strangulated bowel be rendered impervious by any of the causes which I have already mentioned, it is clear that the introduction of the tube cannot possibly have the effect of emptying the hernial tumour; but, as the existence of these causes cannot be ascertained beforehand, the fact only shows the propriety of employing the practice in all cases indiscriminately. If, previous to, or during strangulation, the bowels happen to be loaded with solid or fluid fæces, the lodgements of fæcal matter in the cæcum, the lower portion of the ileum, and the sigmoid flexure of the colon, will necessarily prevent the passage of flatus, and prevent the success of the first introduction of the tube; but this circumstance only shows the necessity of persevering in the practice, assisted by repeated enemata, in order to eventually succeed.

But let this amount of success (eleven cures out of sixteen cases) be calculated or viewed how it may, it is manifestly such as ought to compel every conscientious and unprejudiced surgeon to give the practice a full and fair trial, before he decides on an operation which too frequently terminates fatally, and requires for its performance a degree of skill which many do not possess, and able assistance which may not be at hand. Such a trial causes very little loss of time, does not interfere with the employment of other remedies, is perfectly safe, and may be had recourse to after reasonable attempts at reduction have failed. For the reasons already stated, I have advised the treatment to be employed indiscriminately in all cases of strangulated intestinal hernia; but there is an exception to this general rule, namely, the cases in which the hernial tumour is inelastic at all points, and evidently impacted with hardened excrement which cannot be forced into the bowels within the abdomen. The cases in which its effects are most valuable and striking, are those of small, recent, and strangulated enterocele, in which the constitutional disturbance is so severe and urgent, and the hernial tumour so tender, that the patient will not allow it to be touched, or cannot bear any attempts at the taxis. In cases where the intestine adheres to the sac, although it cannot effect reduction, it may either remove or relieve the symptoms of strangulation. But as far as my experience goes, it is as inapplicable in practice as it is in theory to cases of epiplocele.

In my work on Defecation, I have described the different tubes and syringes which I had employed from time to time, in diseases of the intestinal canal; but, from some strange oversight, have omitted to describe the most improved; and as they are those which I continue to employ in all such cases, it is necessary to supply this serious defect. The gum elastic tube is sixteen inches in length; considerably thicker throughout, and more bulbous at its upper extremity than that of the stomach-pump; at its lower extremity, it has a brass ferule, so made as to fit bayonet-wise into an aperture in a short pipe springing from a small brass syringe; and to give it the necessary firmness, a delicate brass wire runs spirally through its interior. I employ this tube in new-born infants, as well as in adults. The syringe which I use is of brass, about seven inches long, and one inch in diameter, it is easily worked, very portable, and quite sufficient for all purposes connected with intestinal affections. In short, it is the small syringe belonging to the self-injecting apparatus of Mr. Weiss of London, to whose ingenuity I am also indebted for the necessary improvements in the gum elastic tube.

Having known that many intelligent and expert practitioners have failed in introducing the tube to the necessary height, and that others are fearful of using the degree of force sometimes necessary for that purpose, I may be permitted to offer a few simple instructions to the one, and some facts and reasoning to the consideration of the other. As regards the difficulty of introducing the tube to the necessary height, it arises from the non-observance of the following rules. 1st. The instrument should be thrown into cold water until it becomes stiff, then be dried, made perfectly straight, and a few inches of its upper extremity well oiled. It is then to be introduced, the patient lying on the left side, and passed up inch by inch, and as nearly as possible in the course of the rectum. If obstruction be met with, it may be slightly withdrawn, and afterwards passed gently upwards. But if it cannot be introduced higher up without much force, attach to it the syringe, and dipping the point of the latter into the fluid to be injected, let an assistant give two or three rapid strokes of the piston, and so as to bring the full force of a strong and unbroken column of fluid to bear upon the point of resistance, and, while this is doing, let the surgeon urge the tube firmly upwards, and it will generally pass through the obstruction, as if through a narrow ring. 2dly. With respect to the fear of using the moderate degree of force necessary to pass the tube into the colon, it is chiefly attributable to the fatal consequences which are known to have attended the unskilful management of rectum bougies, and the rude introduction of the metallic pipe of the common enema syringe. But the cases are quite different, and I shall now contrast them. The metallic pipe is some inches in length, and quite inflexible; the rectum bougie is short, thick, solid, and possesses little flexibility. With either of these, it is obvious that great injury may be inflicted on the bowel. The gum elastic instrument, on the contrary, is much longer than either, not so thick as the latter, tubular, bulbous at the upper part, flexible to the necessary degree, and, consequently, most unfitted for exerting any great or dangerous degree of force. Besides, its action must be that of dilating, not piercing, the sides or walls of the contracted rectum, for it is grasped, throughout four-fifths of its course, by the muscular coats of the intestine, and compelled to move in the axis of the bowel, and in no other direction. All these points should be well weighed first, and then viewed in connexion with the well known fact, that it requires very great force to rupture living muscular fibre. But the argument which ought to have most weight with those who fear to use the necessary degree of force, is that which I have used in my work, and which increased experience enables me to repeat, namely, the fact, that I have neither met with, nor heard of, a single instance in which the introduction of the gum elastic tube has been followed by an evil or unpleasant consequence.

*Dublin Journal of Med. Science. September* 1, 1838.

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## *Observations arising out of the Results of Amputation in different Countries.*

By BENJ. PHILLIPS, Esq. F.R.S., Surgeon to the St. Marylebone Infirmary.

[THIS paper was read before the Med.-Chirurgical Society in November last. It is a very valuable document, and well calculated to promote the great object of substituting, in medicine, logical deductions founded on positive facts, in place of traditional statements derived from vague suppositions. We regret that our limits will not permit us to lay before our readers the excellent introductory observations of Mr. Phillips: we must, however, find room for the more important facts established by his investigations.]

The amputations included in this enquiry are those of the arm and the forearm, the thigh and the leg. The whole of them have been performed within the last four years in civil hospitals and in the private practice of hospital surgeons. The gross number of cases is 640; and this number embraces all cases, acute, chronic, and the results of violence which have occurred in the practice of the persons by whom the returns have been furnished within the period I have named. Of these cases, 490 are reported "cured," and 150 died, either in consequence of the operation or the progress of the disease; to rescue the patient from which, recourse was had to the operation.

I apprehend that a large number of our professional brethren are unprepared for such a result: I have only met with very few who were at all sensible of the extent of the mortality which occurs. Compared with lithotomy, amputation and the ligature of arteries are often, perhaps commonly, held to be unimportant operations; and yet the results show a very great balance in favour of the success of lithotomy. It is necessary, however, in connexion with this circumstance, that a fact should be borne in mind, by which these results will be modified. A large number of amputations are performed under very unfavorable circumstances. Lithotomy, on the contrary, commonly admits of our choosing a time when the patient is best prepared for it. In other words, the mortality consequent upon lithotomy is often directly caused by the operation: in amputation, by the disease or injury.

I have now shown that the mortality succeeding to amputation is very great,—twenty-three per cent. I shall, therefore, proceed to analyze the gross number, and exhibit the proportion furnished by the different countries implicated in the enquiry. They are as follow:

| | Cases. | | Deaths. | | Per Cent. |
|---|---|---|---|---|---|
| France | 203 | ... | 47 | or | $23\frac{31}{203}$ |
| Germany | 109 | ... | 26 | ... | $23\frac{93}{109}$ |
| America | 95 | ... | 24 | ... | $25\frac{5}{19}$ |
| Great Britain | 233 | ... | 53 | ... | $22\frac{174}{233}$ |
| | 640 | | 150 | | $23\frac{7}{16}$ |

Here is an average number of deaths, amounting to, as near as may be, 23⅓ per cent. If the several countries be taken separately, we find that France is a fraction below this average; that Germany differs only to the amount of a fraction from France; that America only exceeds the average by a little more than two per cent.; and that Great Britain is a fraction below the average.

. . . .

We now proceed to enquire whether the treatment commonly, and in this country almost universally, employed after amputation in diseases, attended by proper suppuration, be fortunate in its results; and whether another method of treatment might not as a rule be advantageously adopted.

I have classed the observations which I possess, so as to show, on the one hand, the cases in which amputation has been performed in acute diseases, or in consequence of injuries; and, on the other, the cases in which it has been employed as a remedy for chronic diseases: but, as chronic disease is a very general term, I will point out the diseases which I have included in it; they are diseased articulations, with considerable purulent discharges, necrosis, caries, and extensive old-standing suppurating surfaces. In our own country, for a considerable number of years, the method of immediate union has been almost exclusively employed. No matter what may have been the disease, it has commonly been attempted. The inconveniences of secondary union were doubtless considerable; so that the system of immediate union, once adopted, should have been exclusively employed, was a result very much in conformity with the revolutions of all systems.

The idea of lessening pain by diminishing the number of dressings, of avoiding profuse suppuration, of rapidly healing a large wound, were powerful elements for the overthrow of the old method. At the same time, its brilliant success in the practice of military surgeons came so strongly in support of the new system, that the abandonment of the system of consecutive union in our own country was, and still is, almost complete. In other countries, the merits of the new system were much less readily appreciated, and the prejudice which was raised against it was considerable. Men clung with pertinacity to the old system, disastrous as were its effects; and, even up to the present moment, many eminent surgeons employ it almost exclusively,—modified, however, it is true; and a very large number employ it much more extensively than we do.

. . . .

I beg attention to the results which I shall now proceed to lay before the Society, in proof of the opinion that there is a large and well-defined class of diseases in which, after amputation, union by the first intention cannot, as at present practised, be so successfully employed as that in which it is consecutively obtained.

This class of diseases I have already particularized; and, of the 640 constituting the gross number on our tables, 213 cases are of this kind.

Of these cases, immediate union was attempted in 117; consecutive union in 96.

Of the 117 cases, 88 only succeeded; the deaths amounted to 29.

Of the 96 cases in which the treatment was by consecutive union, 76 succeeded; the deaths were 20.

Of these cases, Great Britain furnished 86; the other countries included in the observations, 127.

Of the 86 cases, immediate union was attempted in 60; consecutive in 26, and with the following result:—Of the 60 cases, there were 15 deaths; of the 26, there were 5 deaths.

Of the 127 cases, immediate union was attempted in 57 cases; consecutive in 70.

Of the 57 cases, 14 were unsuccessful, (the patients died;) and 43 succeeded. Of the 70 cases where consecutive union was employed, there were 15 deaths. The results, therefore, attendant upon the practice of immediate union are, a mortality amounting to 25 per cent.; upon consecutive union, of nearly 21 per cent.

And there is a singular uniformity attendant upon the results of these two modes of practice, as shown by the returns furnished by our own and the other countries;

and all are strongly confirmatory of the prudence of avoiding immediate union in this large and well-defined class of diseases.

As, I trust, this has been made sufficiently evident, it may now be asked, in what way is this increased fatality, which is attendant upon immediate union, to be explained? With respect to a portion of the cases, the explanation is easy:— they have been produced by phlebitis and purulent absorption; pus being found in the lungs, liver, and other situations. The reason why, in such cases, the tendency to this termination is more frequent than in ordinary cases is, I apprehend, because the disposition to purulent secretion in the diseased organ still continues; and because immediate union might prevent its evacuation, and so cause constitutional disturbance and absorption. In a certain number of cases, "visceral congestion" is occasioned by the suppression of the accustomed secretion; diarrhœa supervenes, and the patient dies. . . .

Having indicated the result, and being strongly impressed with its correctness; having adduced evidence which militates strongly against the practice of immediate union in the class of cases to which I have before particularly alluded; it may be asked whether I have any suggestion to make, by the adoption of which the inconvenience of the present practice may be lessened? . .

I am not an advocate for the substitution of consecutive for immediate union in any class of cases, but I am prepared to recommend a modification of the present method of treatment in a large class. If the causes of the adverse results of immediate union have been correctly indicated, I apprehend that a means of remedying them will be naturally presented. If it be a consequence of the sudden suppression of a secretion to which the economy has been long accustomed, we may fairly assume that, if the secretion can be maintained during a certain variable period of time by artificial means, we may employ the method of immediate union without any apprehension of those complications to which allusion has been made, and at the same time avoid those objections of which consecutive union cannot be relieved. Virtually, the proposition is carried into effect every day. An attempt is made to secure immediate union in such cases: it is unsuccessful; consecutive union obtains; the secretion is indefinitely prolonged, but the evils of consecutive union come into play.

My proposition is stated in a few words. Before the amputation is performed, establish in the vicinity of the point where amputation is to be practised an artificial suppurating surface, either by means of a seton or issue, and let the secretion be maintained for a term varying with the duration of the secretion at the diseased point.

[Mr. Phillips admits that he has no direct evidence of the efficacy of the proposed modification of the practice; but he considers himself fully borne out by the facts adduced, and by legitimate reasoning, to recommend it to the profession.]

*Med. Gazette. June* 9, 1838.

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## *On the Treatment of Small-pox by puncturing the Pustules.*

### By JOHN LANGLEY, Esq.

[ALTHOUGH the treatment recommended in the following extract is by no means new, it is believed to be less known, and still less practised, than it ought to be.]

At the present time, when I regret to observe in this district that that baneful disease, small-pox, has been and is still existing to a considerable extent, any mode of practice which has for its object and effect the amelioration of suffering, &c. ought to be freely promulgated. Since my publication of the following mode of practice, very many cases of small-pox have fallen under my treatment; and of those in which the pustules have by puncture and incision been evacuated, I have every reason to be satisfied that the desirable objects above alluded to have been invariably attained. In almost every case of small-pox where, from the malignity of its character, height of attendant inflammatory fever, and confluent nature of eruption, serious consequences are to be apprehended, upon the pustules acquiring a full and prominent vesicular character, before the lymph assumes its purulent appearance, I puncture with a lancet the most depending part of the inferior edge of the pustule; thereby leaving a perfect superficial covering of cuticle, obviating in a great degree the tendency to irritation resulting from the ingress of atmospheric air into the collapsed cyst, the superjacent cuticle falling as a plaster upon the secreting surface. The expression of relief afforded to the sufferer by this operation is most grateful, and may be somewhat adequately estimated by those who have experienced the comforts of having the contents of a whitlow evacuated. The desquamating process is not only facilitated, but greatly expedited; as, in the course of three or four days, you have an extensive desquamation, occurring almost simultaneously over the whole surface, leaving discoloration, but seldom any depressions: and in cases where, for the sake of experiment, I have thus proceeded with one superior extremity, and neglected the other, the difference of result has been most manifest. The removal of the distressing tension pervading so extensive a surface, and pressing upon such sensitive and irritable tissues, cannot fail to give the *rationale* of the relief afforded, and also the fair presumption that the cerebral irritation, and consequent fever resulting from such a painful condition, must be greatly modified, if not altogether obviated, independently of removing from the inflamed and abraded surfaces an immense quantity of morbific secretion, the reabsorption of which, to a certain extent, may be reasonably inferred. If, from a prejudice, which oft ignorantly prevails, of interfering with the natural course of naturally incidental diseases, certain persons might not grant the propriety of opening all the cysts, (which, by the bye, is no trifling task,) surely they will make trial of the practice upon those parts, the preservation of which is so desirable. *Lancet. June* 30, 1838.

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### *On the Efficacy of Pressure in certain Cases of Venereal Phagedænic Ulceration.* By Hugh Carmichael, A.M., Member of the Royal College of Surgeons in Ireland.

[This is a valuable practical paper, and recommends a mode of treatment, in a most untractable affection, which we doubt not will be found frequently, if not generally, beneficial. Surgeons who have been in the habit of witnessing the excellent effects of pressure in cases of inflamed testicle, erysipelas, and ulcers of the legs, will have no difficulty in admitting the probability of advantage from the extension of the practice to phagedenic syphilitic ulcers. The following extract from Mr. Carmichael's paper contains the general views of the author, and an account of the mode of applying the pressure: they are illustrated by the detail of four cases.]

Great irritability being one of the most prominent features of the disease, I was induced to imagine that pressure (an agency used with so much benefit in ill-conditioned, unmanageable ulcers generally, where morbid sensibility is a very leading character,) might probably, in these, be likewise adopted with some advantage: it was accordingly tried in a case that occurred to me of the most hopeless description, where all the varieties of treatment now generally employed were resorted to without effect, the disease progressing, and rapidly destroying the part; and the success which attended it was so decided that I have since used it in several others, and with such benefit as to establish it, in my mind, as a method highly deserving of attention in these cases. The great obstacle I have experienced in its use is the occasional difficulty of its application, so as to bring the diseased part decidedly under its influence: by a little dexterity, however, we may in most instances succeed in doing so, the operation sometimes requiring more management than at others. The mode of affecting it, of course, will vary according to the part the ulcer to be compressed is situated on: when on the glands or body of the penis, strips of adhesive plaster are the means I have adopted, looped, by passing one of the tails through a slit in the other; the penis is then to be introduced into the loop, and, the ulcer being brought into its bearing, it may be tightened at pleasure; the tails are to be then firmly wound round the penis, and secured. When on other parts, as the forehead, or places similarly circumstanced, pressure may be more easily and decidedly commanded; while on others it may, perhaps, be more difficult to effect it; yet by management, I think, with very few exceptions, it can be accomplished in all. On some occasions, when the required pressure should be more decided, I have employed slips of sheet-lead, placed over any appropriate dressing, and included in the loop. This substance, from its pliant nature, admits of being easily moulded into any form, and can readily be shaped so as to produce effectual compression upon the ulcer. Indeed, I think that the beneficial effects to be derived from pressure, particularly in ulcers, is not so much from the degree of tightness with which it is used, as in the application of a solid, unyielding substance to the surface, probably thereby inducing the absorption and removal of such diseased surface; and, for the reasons just stated, sheet-lead I have found to answer best for that purpose. With respect to the time required for its continuance before its full effects were obtained, it was various: sometimes a few days changing the entire character of the ulcer, from an ill-conditioned, spreading sore, to one of a florid, healthy aspect, with contracting boundaries; while, on other occasions, it required a longer continuance; but in all the amendment was so evident after the second or third day, as decidedly to manifest its salutary influence, and give assurance of a favorable result. In some instances I have been enabled, by means of it alone, to perfect the healing of the ulcer, unaided by any other measures; while in others, (and these the greater number,) its phagedænic nature was only removed; a morbid diathesis still remaining, which appeared incapable of being overcome entirely without more active remedies. In these latter I found mercury to serve all the purposes required; the phagedænic character being first subdued, the regenerated sore rapidly disappearing under its influence, when the system became engaged by it: indeed, it would seem as if its use were necessary in them to complete the cure. In the cases in which I employed it no other means were adopted, and in all it was successful,—I mean so far as subduing the phagedænic disposition: some, however, no doubt, may occur where it could not be used alone or in the first instance; for example, if great inflammation were present, leeching, with a view to the subduing it, according to the suggestions of Mr. Richard Carmichael, who has advised local bleeding in these cases, would, I think, first be necessary; and other circumstances might also be attendant upon it, requiring appropriate remedies before submitting it to pressure: these, however, could only be regarded in the light of preparatory steps, previous to the employment of this latter measure, and which must be considered that whereby effectual benefit is to be obtained: perhaps there may be cases where it would be productive of no advantage, or altogether inadmissible. *Dublin Journal. September,* 1838.

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## *On Chronic Cystitis; with Observations on the Employment of the solid Nitrate of Silver in Catarrh of the Bladder, as practised by* Professor LALLEMAND, *of Montpellier.* By JOHN O'BRYEN, M.D. M.R.C.S.

[THIS paper was read before the Bristol Medical Library Society, and is a valuable communication, inasmuch as it will make more known to the profession in this country a considerable improvement in the treatment of a common and very distressing disease.]

If you reflect (says Dr. O'Bryen,) upon the severity of chronic cystitis, which irresistibly compels a man to get out of bed four or five times per hour during the night, each time subjecting him to excruciating pain, and thus effectually preventing sleep, and which will not permit him to sit one half-hour at his dinner-table without disturbing him; to say nothing of the effect of pain on the general health and the constitution, or the want of exercise on horseback or in a carriage, or even on foot; and then again reflect that, when of long standing, it has been declared incurable, inveterate, and the opprobrium of surgery, though it may be relieved, but, like chronic bronchitis, returns with redoubled severity upon the application of the slightest exciting cause; I feel assured you will agree with me that any mode of treatment, offering for such a disease not relief alone, but a radical cure,

—and that, too, not in months, but weeks, and often in a few days,—deserves our best attention, and its author our meed of praise.

The following is Professor Lallemand's mode of applying the remedy:

He uses an instrument which consists of a large catheter (of pure silver, as, if there is any alloy, the caustic acts upon it,) open at both ends, having two sorts of stilet, according to the part you wish to cauterize; at the extremity of each stilet is a small excavation, containing the caustic, which is first pulverized, and then placed in the excavation over a spirit-lamp, which fuses and moulds it to the cavity. When the instrument is prepared, introduce into the bladder an ordinary catheter, in order to empty it completely. This precaution is strictly necessary, for the urine would dissolve the caustic, and prevent its directly affecting the mucous membrane. When this has been withdrawn, the instrument bearing the caustic is to be introduced (closed), and the moment it has entered the bladder, you are to push the stilet, and rapidly turn the *porte caustique* from side to side two or three times, and then pull the stilet into the instrument, and withdraw it: our object should be to touch the surface in as many points as possible. While the instrument is within, the bladder contracts and grasps it, the kidneys secrete a small quantity of urine, as the lachrymal gland secretes tears when the conjunctiva is cauterized; but this small quantity of liquid, far from being hurtful, is, on the contrary, favorable, as it acts as a vehicle to the portion it does not decompose, and conveys it equally over the surface of the membrane. The patients feel at that moment a sharp pain at the neck of the bladder and in the rectum, described by them as not unlike a pinch, but much more supportable than the continued dull pain of chronic catarrh. There is now an irresistible desire to pass water, and, as the bladder is nearly empty, very little passes; and this causes a burning along the urethra, and is accompanied by some drops of blood. This desire is renewed every moment, causing violent but useless efforts. These gradually decrease; and on the second and third day there is no longer any pain on making water, and a few small grey eschars, like burned paper, come away with the urine. This occurs in a large number of patients; but in some more susceptible the process does not proceed so simply, particularly if you have used the *porte caustique* too long. In this case retention of urine follows, which lasts from three to thirty-six hours: even here we must not be in too great a hurry to use the catheter, as a warm bath, a few narcotic lavements, emollient drinks, some tartrate of soda with infus. sennæ, and sometimes a few leeches, will cause the spasms to yield: if not, some belladonna to the meatus may be tried, always taking care to use antiphlogistics with moderation in the beginning, as inflammation is necessary to the cure. In a majority of cases, one cauterization is sufficient to procure a cure: when it happens otherwise, a second and even a third application may be necessary, but Monsieur L. states that he never saw a case requiring a fourth.

[According to Dr. O'Bryen, the advantages of this mode of practice are very great, when cases suited for it are selected: it is always inadmissible when the kidneys or prostate are diseased.]

Compare this with the modes of treatment hitherto employed: with what perseverance must they not be continued in for months, nay, often for life, to procure even relief. Patients treated as formerly must use great precaution to prevent a relapse: the least fault in diet, the slightest cold or change in the temperature of the air, the most moderate exercise on horseback or in a carriage, may act as a match fires a train, and make the sufferer as bad as ever: in fact, they must be always on their guard, even to enjoy a supportable state of health. This is not the case with cauterization; for, however feeble the digestive organs are, however weak the patient, whether in health or constitution, it may be employed with advantage, as it is never injurious: it acts directly on the diseased surface, and does not affect the other viscera; and the patients, instead of being months and years under treatment to procure even a slight amelioration, may be cured in a few days, or at latest in a few weeks. The cure is not an uncertain one, and the patient may return to his ordinary mode of life without fear.

*Dublin Journal. September,* 1838

# MIDWIFERY.

## *On the best Means of applying Pressure to the Uterus after Delivery.*

By J. L. FENNER, Esq., Surgeon.

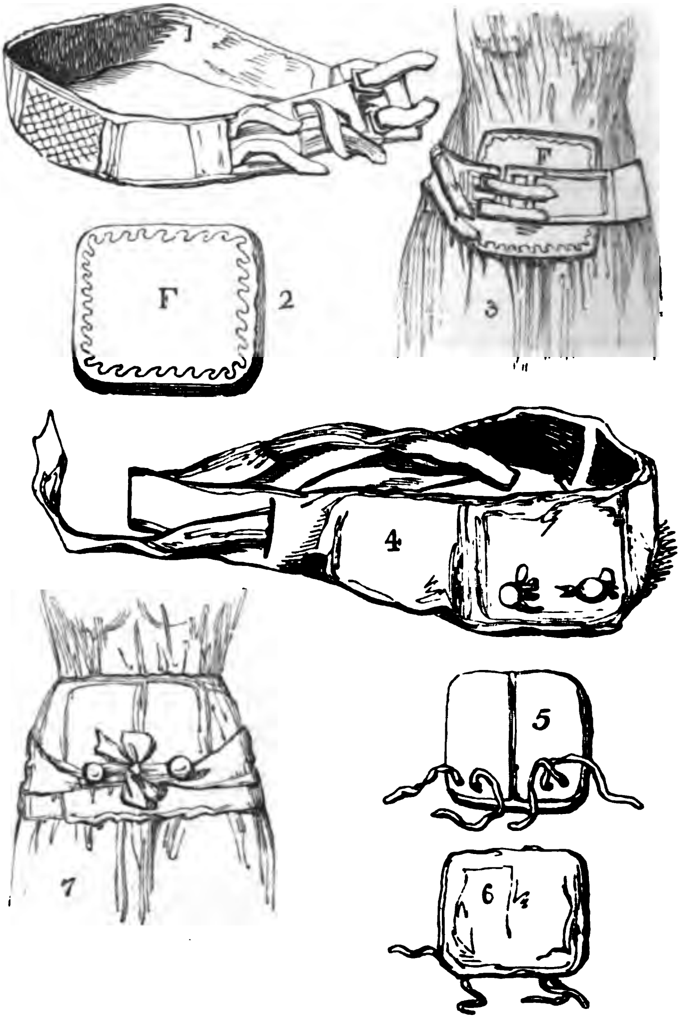
[THIS would be a valuable communication if it had no other merit than that of tending to impress still more strongly on young practitioners the universal necessity of compressing the uterus after delivery. The possession by the accoucheur of such an apparatus as that described by Mr. Fenner would be most desirable, if it served merely, by its material presence, to keep ever present to his mind that great truth. We therefore give the author's description of it, as well as the figures by which the description is illustrated.]

IN forming these bandages, my first object was to make pressure on the region of the uterus by a *firm unyielding substance;* because, by this means, the contraction of that organ was found not only to be more readily produced than by a similar degree of force applied by means of a bandage composed of linen or of any soft substance, but, having been so produced, was more readily maintained contracted. To this principle we must refer the signal benefit derived from pressure by the firmness of the hands in cases of sluggish uterus: but hands and arms soon tire at this employment, and consequently the degree of pressure necessary to the complete contraction of this organ, instead of being steadily continued, becomes relaxed, and hemorrhage occurs, or, if it have been momentarily suspended, is renewed.

Figure 2, in the subjoined engraving, represents a piece of mill-board, obtained from the stationer, seven inches by eight, padded on the inside with two layers of wadding, and covered with flannel or kerseymere. This plate has been previously divided down the middle, as seen in fig. 5; then united by pasting a strip of leather on each side, so as to form a joint; thus enabling it to be folded into half its compass, like a closed book; and, with the band, fig. 1, wrapped round it, to be conveniently put into the pocket. The band, fig. 1, which is made of variable length, to suit the different dimensions of different females, is composed of webbing, three inches wide; is furnished with two buckles, and three sets of straps to regulate its pressure; and has four inches of India-rubber web let into it, so as to combine a degree of elasticity with the force of its pressure. Fig. 3 shows the bandage duly applied; the band being under the crests of the ilia, and carried round the hollow of the back, just at the junction of the sacrum with the spinal column, by which it is prevented slipping upwards. This bandage, from its easy application, I use immediately after the birth of the child, directing the nurse, if there be hemorrhage, to increase the pressure by buckling it tighter. This simple bandage answers well for every purpose proposed, is capable of exerting a great degree of pressure, and of thus facilitating or accelerating the complete contraction of the uterus.

When the patient is comfortably in bed, I usually apply what I call my sash bandage: were it applied previously, it would probably become soiled. This is represented, fig. 7, applied under the crests of the ilia, and carried to the hollow of the back, just above the sacrum. Fig. 5 represents the exterior, and fig. 6 the interior, of exactly the same plate, with the joint as described in fig. 2; but on each side, within two inches of the bottom, are two holes through which a piece of tape is seen passed from the inside, to attach a pearl button on the outside, of the size of half-a-crown, as seen in fig. 4. Fig. 4 shows this bandage before it is applied, folded in half: it is about thirty inches long, and made of white jean doubled; it incloses the plate, fig. 2; it tapers from the width of the plate towards each end, where twelve inches of strong broad tape are attached for tying under the button, as seen in fig. 7.

The bandage is sloped downwards to fit the hollow above the sacrum, and in its posterior portion a slit is made, through which its opposite end is passed. By placing the plate over the region of the uterus, carrying the two ends of the bandage to the hollow above the sacrum, and then bringing them round under the crests of the ilia, drawing them tightly over the plate, and tying the tapes in a firm manner under the buttons, a very effectual resistance is offered to the tendency which otherwise every bandage would have to slip upwards, and recede from the part which should receive pressure.



The specific advantage of the above plan, besides affording an extraordinary degree of comfort to the patient by the support it affords to the relaxed abdominal parietes, thus preserving the natural figure, is found by experience to be the prevention of uterine hemorrhage and its dreadful consequences. Under the pressure which this bandage is capable of producing, even the formation of a coagulum of any size is almost impossible; and thus the accoucheur is enabled to leave his patient in a state of perfect security, which never can be the case if the uterus, though contracted at the time, be left without the support of some such pressure, which is therefore essential in every case in a greater or less degree. The natural expulsion of the placenta will be much accelerated by the systematic pressure.

*Lancet. June* 2, 1838.

# PART FOURTH.

# Medical Intelligence.

## PROVINCIAL MEDICAL AND SURGICAL ASSOCIATION.

The Sixth Anniversary Meeting of this Society was held on Wednesday and Thursday, the 18th and 19th of July, at Bath. On Wednesday morning, at twelve o'clock, a meeting of the council took place, at the rooms of the Bath Royal Literary and Scientific Institution, when the financial and other routine business of the Society was transacted, and preparations made for the general meeting. This took place at seven o'clock in the evening, and was very numerously attended. Dr. Boisragon, the late President, having resigned the chair, it was taken by Dr. Barlow; both gentlemen addressing the meeting in elegant and appropriate speeches. Dr. Hastings, the Secretary, read the Annual Report of the Council, which represented the Society to be in a very flourishing condition, both the funds and the number of members being annually increasing. The actual number of members enrolled previously to the meeting was 1080; and no doubt the number was considerably increased before the close of the reunion at Bath. Besides the ordinary routine business of voting resolutions of thanks, &c., much interesting discussion on various important subjects took place, and committees were appointed to carry out the views of the Association. The two principal of these were, one on the subject of Quackery, and one on Small-pox and Vaccination; both subjects of much interest, and the latter of surpassing importance. At this sitting, also, the following gentlemen were unanimously elected HONORARY CORRESPONDING MEMBERS, on the proposition of Dr. Forbes:

*For Russia*: F. C. Markus, M.D., chief physician to the Galitzin Hospital, Moscow; George Lefevre, M.D., physician to the British embassy, St. Petersburgh.

*For Sweden and Norway*: D. Holst, M.D., professor of Medicine in the Royal Frederick's University, Christiania.

*For Denmark:* C. Otto, M.D., professor of Pharmacology and Forensic Medicine in the University of Copenhagen.

*For Austria:* Burkard Eble, M.D., librarian of the Josephine Academy, Vienna.

*For Holland*: J. L. Schroeder van der Kolk, M.D., professor of Anatomy and Physiology in the University of Utrecht.

*For France:* E. C. A. Louis, M.D., physician to La Pitié, &c., Paris; G. Andral, M.D., professor of Pathology in the Faculty of Medicine, Paris.

*For Italy*: Carlo Francisco Bellingeri, M.D., president of the Medical Faculty in the University of Turin, &c.; Maurizio Bufalini, M.D., professor of Clinical Medicine at the Hospital Santa Maria, Florence.

*For Portugal:* Antonia Jose de Lima Leitaõ, M.D., physician to the Hospital San Lazaro, at Lisbon.

*For the United States of America*: John C. Warren, M.D., professor of Anatomy and Surgery, Harward University, Boston; Robley Dunglison, M.D., professor of Materia Medicine, &c., Jefferson College, Philadelphia.

*For the East Indies:* W. B. O. Shaughnessy, M.D., professor of the Institutes of Medicine in the Medical College, Calcutta.

*For the Brazils:* Luis Vicente De Simoni, M.D., secretary of the Imperial Academy of Medicine of Rio Janeiro.

*For Mexico:* Guillermo Schiede, M.D., member of the Academy of Medicine, Mexico.

*For Australia:* E. C. Hobson, M.D., naturalist to the colony of Van Diemen's Land, Hobart Town.

After the transaction of other necessary business, the meeting adjourned till the following morning; when, shortly after nine o'clock, the members of the Association breakfasted together at the Pulteney Hotel, Sydney Gardens. Arrangements were entered into at the breakfast for the immediate execution, and presentation to Mrs. Hastings, of the portrait of Dr. Hastings; two hundred pounds being already subscribed for that purpose. The artist is not yet decided on.

At one o'clock the members again met at the lecture-room of the Institution, when various interesting subjects were discussed, and a good deal of business done.—Dr. Gregory, of London, made some observations on the varieties and best mode of preserving vaccine lymph; and presented some specimens to the meeting.

Mr. Thompson read a paper on "a peculiar Affection of the Uvula," which will be inserted in the next volume of the Transactions. Dr. Johnstone Aitkin, of Poole, read a communication respecting the use of the sea-plant called *Fucus esculentus*, or "tangle," in the room of the more expensive and less suitable gum bougies, in cases of stricture of the rectum and urethra.

Dr. Theodore Boisragon produced some drawings, executed by himself, of various parts of the human skeleton, and announced his determination to publish them at a very reduced price. We have no doubt of their usefulness to practitioners.

Dr. Conolly, of Cheltenham, then read the Report of the Benevolent Fund Committee, which showed the increasing utility of its operations. The receipts for the past year were—balance, 34*l.* 4*s.*; donations, 84*l.* 6*s.* 6*d.*; subscriptions, 115*l.* 3*s.* total, 233*l.* 13*s.* 6*d.* Expenditure, 89*l.* 5*s.* Total balance, 144*l.* 8*s.* 6*d.* This balance belongs to what is called the "permanent fund," and is not available to the general purposes of the Society, but is applied, in the shape of annuities, to widows and orphans. Much interest was excited by the report and consequent discussions; which, we doubt not, the fund will feel the benefit of before the next meeting of the Association.

Much other business was transacted, but not of a kind to interest the general reader, and the proceedings of the morning were concluded by the delivery of the annual Retrospective Address, by Dr. Malden, of Worcester. This was a very learned and eloquent production, as might be expected from the well-known talents of the author; but, as we shall have an opportunity of noticing it when it is published in the Transactions, we shall give no further account of it at present.

Between six and seven o'clock, the members and their friends dined together at the Town-hall; Dr. Barlow in the chair, supported on the right by the mayor of Bath, and on the left by the Rev. William Marshall, his chaplain: vice-presidents, William Tudor and George Norman, Esqs. The dinner, dessert, and wines, were of the first quality. An excellent band was in attendance; and we can truly state, in newspaper phrase, that the evening was spent in the utmost harmony and enjoyment.

The next anniversary meeting of the Association is to be held at Liverpool, under the presidency of Dr. Jeffreys; and Dr. Symonds, of Bristol, is appointed to prepare the Retrospective Address.

We regret that our limits prevent us from giving, in greater detail, an account of this interesting meeting; but we must find room for the short but admirable speech of Dr. Barlow, on assuming the chair. This we copy from the columns of a newspaper, (the Worcester Journal,) which gave, at the time, an excellent and accurate report of the proceedings. The flattering manner in which Dr. Barlow was received by the Association was in accordance with the high character which he holds in the profession; distinguished, as he is, for his literary and scientific acquirements, his great practical skill, his honorable bearing towards all his brethren, and his active and unbounded benevolence to the sick poor.

### *Dr. Barlow's Address.*

"Gentlemen: In entering on the office which your kindness has assigned to me, my first agreeable duty is to bid you all heartily welcome to this our ancient city, which was never more signally honoured than it is on the present occasion. Cultivated talent and moral worth, especially when combined, must ever receive the respect and regard of all who are capable of appreciating them. For both, our revered profession has ever been eminent; and when they who, even amongst its members distinguish themselves by pressing forward in the career of humane and enlightened endeavour, assemble in such numbers as I rejoice to see now around me, for the purpose of cultivating still further their divine art, and promoting the best interests of humanity,—their presence must confer honour on any place which is graced by such an assemblage. It is not my design, gentlemen, to trespass long on your time or attention in the address from the chair, with which the customs of our Association require me to open the present session. To do so would be an abuse of the privilege which my present situation confers, and prove only an irksome delay of the far more interesting matter which will be speedily submitted to your consideration. In each successive year some change takes place in the circumstances under which your president addresses you. Heretofore, and until the designs for which the Association was instituted had become generally understood, it was the duty of your presidents, in their respective discourses, to dwell on those designs, and the evidences of their fulfilment, so as to make the nature, scope, tendency, and progressive realization familiar to all concerned. Happily this is no longer needed; for the years that have elapsed since we first assembled to found this Association, and the wide diffusion of our Reports and Transactions, have made these designs fully known; while extension of the Association, which, in respect both of numbers and space, has advanced with a rapidity which I may say is unexampled, furnishes assurance the most unequivocal of their being justly appreciated. Were further proof of this needed, the assemblage which I now see before me, congregated from almost every part of the kingdom, must suffice to carry conviction to the most sceptical. And here, gentlemen, I will remark that, so long as we display such evidence of zealous and harmonious cooperation, we may be content to pursue the direct and even tenour of our way, whatever the opposition we may chance to encounter; and, cheered by the consciousness that, so far as our abilities extend, we are pursuing laudable objects from pure motives, may safely disregard objections, such as only ignorance or misconceptions of our designs could urge against us. Practical details and statistical elucidations you will have abundantly in the ulterior proceedings of the present meeting: on all such it would be vain and idle for me to dwell. I prefer, therefore, during the few moments to which my present trespass shall be limited, to direct your attention to those considerations which admit not of statistical exposition, yet which are not the less valuable from requiring to be addressed rather to the mind's eye than to our actual perceptions.—The main objects for which we are associated, as stated in our fundamental constitution, are the advancement of medical science, and the maintenance of the honour and respectability of the profession. These objects are intimately connected; for, unless science be diligently and effectively cultivated, the honour and respectability of the profession would rest on a very slight foundation; and, unless the honour and respectability were otherwise maintained, on the high ground of moral integrity and liberal sentiment, no advance in science could vindicate its claim to that high estimation in which it has through ages been held, and which, I trust, it will ever, even with sensitive jealousy, preserve. The feelings of the sensitive Roman, who would not that his wife should be even suspected of error, are to be commended; and, with similar feelings, it should be our care so to conduct the proceedings of our Association, that not even the suspicion of selfish or sinister designs should attach to us. To the cultivation of medical science our endeavours have been hitherto directed, with an earnestness and steadiness of which it becomes not me here to speak. However little these endeavours may have hitherto produced, they have at least been exerted with a zeal worthy of the cause which called them forth. My present purpose, however, is not to dilate on these efforts or their fruits, but to impress on you all that they who would judge of the value of our Association, even by the efforts already made, or the products which have resulted from them, would form but a very imperfect estimate of the benefits which our Association is conferring, and which it cannot fail eventually to realize. It has been asked, and in a depreciating tone and unfriendly spirit, what have we done? The very question conveys to me the conviction that the party proposing it has no adequate conception of the subject on which he affects to seek information. No one, really imbued with the love of science or the spirit of truth, would even form the conception of judging us by so crude and inadequate a test. It is, no doubt, true that fruits should be the proof by which modes of cultivation should be judged; but, surely, not till time be given for seeds to germinate and plants to fructify. In our cultivation of medical science, it surely cannot be barren of fruits when upwards of one thousand energetic members of a liberal and enlightened profession are incited by the inspiriting stimulus which this Association supplies, to exert their best faculties and most earnest efforts for investigating those truths of nature which it has ever been the object and aim of our profession to explore. In the activity thus aroused, there is an ample assurance that the energies so called forth will not be unprofitable; that to the seed thus sown may we look with full confidence for a rich and abundant harvest. I care not, gentlemen, how slowly this harvest advances; it being enough to satisfy me that it is advancing. I am not impatient for brilliant discoveries, such as the history of science has shown to occur only at intervals few and far between. Science is ever of slow advance; if this is to be judged by the sudden bounds by which consummate genius starts ahead of contemporary talent marking epochs in the history of the science. But it is ever steadily progressive, if we note the slow but sure, the humble, unpretending, but diligent and unwearied labour with which its ordinary votaries endeavour to extend it. Among these humble labourers do we class ourselves; with the merit attaching to such labour we will be content, and on the result of such labour we are satisfied to rely. Should it fall within the inscrutable designs of Providence that some master-mind should spring up amongst us,—some heaven-born genius, destined to achieve the performances and equal the eminence of a Newton or a Harvey, we shall gratefully hail the distinction, assuming only the humble merit of having used our best endeavours to incite and cherish such transcendant talent. But, gentlemen, in the ordinary pursuit of our objects we look not for such results; and on the diligent exercise of ordinary talents are we content to rest our claims for commendation, encouragement, and support. I am led to submit these views to you, gentlemen, believing them to be those of truth and sober reason; for, while I would deprecate all extravagant anticipations and vain boastings, I conceive it essential to the steady progress of our combined exertions that we neither undervalue what we have done nor form an incorrect estimate of what our conjoined labours are capable of effecting.

"On the second head of my present address,—that, namely, which relates to the maintenance of the honour and respectability of the profession, I shall be very brief; for this honour and respectability must ever flow, not from self-elating pretensions or arrogant claims to consideration, but from the professional skill and moral worth of the individual members. As the aggregate of parts constitutes the whole, so must the maintenance of honour and respectability by each individual member of our Association ensure, beyond the possibility of failure, the continuance of these long-enjoyed attributes to the collective body: and, when I consider the high moral qualities which the members of our body on all occasions display, the talents they evince, and the zeal they manifest,—to all of which even the brief records of our Association already bear ample testimony,—I can entertain no fears of our ever, as a profession, descending from that high moral eminence on which the opinions of the world, and the express declaration of several of the sagest and most acute observers of human nature, have for ages placed us. On the conduct of our individual members I confidently rely for preserving unsullied that reputation which the profession has hitherto maintained. I would here only impress one caution respecting those measures in which we may in our corporate capacity be called on to engage. Corporate proceedings are subject to the same laws, and tried by the same test, as individual conduct. All flow from motives more or less elevated or debased; and, where the higher motives can be brought to bear on any point, those of a lower class should never be suffered to prevail. The human mind is endued with various faculties, all suited to this our transient and probationary state of existence. We have various animal incitements to urge us to whatever is necessary for the continuance and enjoyment of life; but we have also moral faculties to control the lower propensities, and judge those actions to which our animal nature may impel us. It too often happens that the dictates of the animal propensities assume the garb and exercise the authority of the moral sentiments; thus misleading into error many who, under the delusion, are scarcely conscious of doing wrong. But an honest scrutiny of motives, an ingenuous reference of each of these to the moral or animal feeling in which it originates, can never fail to detect the deception, and guide into the right path all who desire to pursue it. These comments would here be impertinent, but that they serve to indicate the criterion by which each corporate act of our Association should invariably be judged. Whenever the act is such as all the higher moral feelings of our nature clearly sanction and approve, let us fearlessly perform it, regardless of all that imperfect conception or timidity may urge against it. On the contrary, whenever an animal or selfish impulse can be traced among the influencing motives, however slight the degree in which this may be intermingled, let us ever pause until we separate the moral motives from the dregs which vitiate them, and restore the higher sentiments to that supremacy with which the Creator has endowed them. I have assimilated corporate acts to individual in the motives which incite them, and the tests by which they are to be tried. But there is one difference which deserves to be noticed as important in our present view of the subject. Corporate acts are less redeemable than individual, and therefore require to be still more carefully guarded from error. Hitherto we have been guarded in our corporate measures; and to ensure a continuance of the same circumspection is the end to which the few remarks which I have now offered are directed. With these I now conclude my present trespass on your time and patience, respectfully yet earnestly exhorting the several members of our excellent Association to apply the energies of their own minds to both the important topics to which I have referred. It will gratify me if what I have ventured thus cursorily to submit to them shall, on deliberate reflection, receive the sanction of their own judgments; and still more if, to the sentiments to which I have given utterance, their own feelings shall be found to respond. So far as my judgment and feelings are capable of guiding me, I would say, in cultivating medical science disdain not, through vain aspirations for profound theories or dazzling generalizations, that patient observation of nature and diligent collection of accurate facts, from which all true theory must be derived, all sound generalization deduced; and, in upholding the honour and respectability of the profession, let the measures we collectively sanction ever bear the impress of that high-toned moral feeling which has so long distinguished our profession, and by which its true interests require us ever to abide."

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## BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

The eighth meeting of this admirable Society took place this year at Newcastle, in the week beginning Monday, August 20th, and was most fully attended. Our limits permit us only to notice the proceedings in the Medical Section; and we thankfully acknowledge that we are indebted for the account of these, to The Athenæum, a publication which has always given admirable reports of the meetings, and which deservedly now stands at the head of the weekly literary journals of this country.

### Section E.—MEDICAL SCIENCE.

*President.*—T. E. Headlam, M.D.

*Vice Presidents.*—Prof. Wm. Clark, M.D., John Yelloly, M.D., Mr. John Fife.

*Secretaries.*—Mr. T. M. Greenhow, J. A. Vose, M.D.

*Committee.*—P. M. Roget, M.D., A. B. Granville, M.D., R. S. Sargent, M.D., B. T. Evanson, M.D., C. J. B. Williams, M.D., J. C. Prichard, M.D., Mr. John Baird, W. Lynch, M.D., N. T. Smith, M.D., Mr. William Fife, R. Reed, M.D, Charles Wightman, M.D., J. Carson, jun. M.D., Prof. Owen, J. Reid, M.D., R. D. Thomson, M.D., Jos. Brown, M.D., Mr. Wm. Morrison.

(Monday, *Aug.* 20.)

A paper was read by Mr. T. M. Greenhow, "On the Beneficial Action of Mercury rapidly introduced in certain cases of Neuralgia."

After some general remarks on mercury as an important therapeutic agent, he stated the following conclusions, as generally derivable from its usual effects: 1st. In secondary syphilis, when slowly introduced into the system, much valuable time was frequently lost, as in iritis. 2d. That the injurious effects of it, which were sometimes observed to follow its operation in syphilis, did not follow when administered for liver disease. 3d. In syphilis, if not quickly beneficial, it proved detrimental, being either a "bane or antidote." 4th. That the mode of administration frequently decided which operation followed; and 5th. That in iritis and amaurosis, the beneficial effects were proportioned to the rapidity of its introduction into the system. Two cases were then detailed, in which the rapid introduction of mercury into the system proved completely successful, in removing severe and long-continued neuralgia,—the first occurring after a severe attack of hemiplegia, which was removed by ordinary treatment. The neuralgia recurred once after removal from the lower extremity in another situation, in the shoulder and arm, and was again relieved, and permanently, by the rapid action of calomel and opium. The second case of severe neuralgia occurred in a lady, who previously had given birth to still-born children. The violence and long continuance of the disease resisted all the usual external and internal remedies, and had shattered both mental and bodily health. The disease completely disappeared on the rapid introduction of mercury into the system, though it had previously resisted the slow action of the same remedy. Other painful afflictions were adduced, in proof of the efficacy of this mode of treatment, that could not be suspected of having any connexion with a syphilitic taint in the constitution.

Dr. Granville enquired as to the degree of salivation induced.—Mr. Greenhow replied, that a decided affection of the gums with slightly increased action of the salivary glands was enough, without profuse salivation. — Dr. Brown confirmed the observations contained in the paper, by cases in which mercurial action removed severe neuralgia, not connected with inflammation or syphilis.

Mr. R. M. Glover read a paper, "On the Functions of the Rete Mucosum and Pigmentum Nigrum in the dark races, and particularly in the Negro; with Observations on a paper on the same subject by Sir Everard Home."

The paper commenced by stating, that the degree of development of the rete mucosum and its pigment, determined the power of resisting the excessive heat of the sun in tropical climates, as evinced by the Negro, (the type, in this respect, of the dark races) the European, and the Albino. The *modus operandi* must be discovered by an attention to both the physical and vital properties of this peculiar organization. The doctrine at present taught on the subject is, that the black skin absorbs more heat, but that the cutis vera of the negro is not so liable to inflammation from a high temperature, as that of a European from a lower temperature; and, as the radiation of caloric from black must be greater than from white skins, the possessor of the former must cool more readily, and enjoy greater alternations of heat and cold. The former part of this doctrine is founded on the experiments and deductions of Sir Everard Home, as detailed in his paper in the Philosophical Transactions. A number of experiments detailed in the paper on the vesicatory powers of differently coloured substances, under the concentrated rays of the sun, contradicted the deductions of Sir E. Home; and hence arose the necessity of looking to the vital properties of the skin of the negro, and the mode in which it is likely to be affected by the radiating and absorbing power of the pigment with which he is provided. Blumenbach and Winterbottom state, that the negro perspires more readily and freely than the European; and Davy says, "In the inhabitants of the tropics, the exhalent arteries of the skin seem unusually expanded, and the whole apparatus peculiar to this secretion unusually developed; and I believe that the blood itself is less viscid, more fluid, and flows more readily through the vessels, so as to promote perspiration, and by that means contributing to the cooling of the surface. And being cooled itself, it contributes again, when it flows back upon the heart, to the reduction of the temperature of the internal parts." Were the inhabitant of the tropics not possessed of this peculiar organization, his system could not respond to the stimulus of heat, by a determination of fluid towards the surface. Doubtless, the excessive absorption of heat by his skin, is useful in promoting this effect; but in the system qualified to respond to the stimulus of heat, and not in the organization of the skin alone, must an explanation be sought of the capability of the negro to withstand the heat of tropical regions.

In the next paper, Dr. John Reid gave an account of "An Experimental Investigation of the Functions of the Eighth Pair of Nerves."

This communication contained some of the principal results which Dr. Reid has obtained in prosecuting this enquiry since the last meeting of the Association. Dr. Reid chiefly confined himself to the Pulmonary and Gastric branches of the Nervus Vagus. In a great number of experiments upon dogs, in which the nervi vagi and recurrents were divided, Dr. Reid satisfied himself that, when a sufficient quantity of air reached the lungs, the respirations were at first performed with ease. The only immediate and constant effect observed, was a great diminution in the number of the respiratory movements, which at the same time became slower and more heaving. When the number of respirations were from sixteen to twenty in a minute, they, in general, instantly fell to from six to eight. In one animal, in which they were from twenty-four to twenty-eight, they immediately fell to fourteen; and, in another animal, in which they were twenty-four, they fell to eight. At a longer or shorter period after section of the nerves, the inspirations became more prolonged and heaving, while the expirations continued to be comparatively short and rapid, and attended by a sound caused by the sudden expulsion of the air. After a time, the inspirations become more heaving and prolonged, the blood is less perfectly arterialized in the lungs, and the arteries circulate blood gradually approaching the venous character; the animal becomes dull and stupid, and dies asphyxiated. In tracing the morbid changes upon the lungs, which follow section of the vagi, Dr. Reid has satisfied himself, from the dissection of animals killed at various periods after section of the vagi, that the blood-vessels of the lungs become gradually loaded with blood, and that this, in a few cases, is the only change which can be observed after death. More frequently, however, this congestion of the lungs gives rise to effusion of serum, which, in the bronchial tubes, is rendered frothy by the passage of air through it; to condensation of portions of the lung without any apparent effusion of lymph; and more rarely to true pneumonia. The respiratory murmur is little, if at all, changed for a short time, but it afterwards becomes bronchial; and when the effusion of serum takes place, the true *râle crepitant* may be heard. It appears, however, that these morbid changes in the lungs do not necessarily take place after section of both vagi and recurrents. In a dog, which was killed in the presence of Dr. Alison, twelve days after section of the vagi and recurrents, and which had apparently recovered from the effects of the operation, the lungs were found perfectly healthy, though the cut ends of both vagi were rather more than one inch distant from each other. Dr. Reid believes that the diminished frequency of the respiratory movements is sufficient to explain all the morbid changes in the lungs, which result from section of the nervi vagi. He related several experiments to prove, that the respiratory movements are not arrested by division of the vagi and recurrents after removal of the cerebrum and cerebellum. In these experiments, it was, however, observed, that though section of the vagi did not arrest the respiratory muscular movements, they were much diminished in frequency. While these experiments illustrate the great importance of the par vagum as exciters of respiration, they also prove that there are other nerves which can transmit those impressions to the medulla oblongata, which excite the respiratory movements; and Dr. Reid adduced some facts to show, that one of the most important of these was the larger root of the fifth pair distributed upon the face. Dr. Reid then related four experiments, which he considered to afford satisfactory evidences of the continuance of digestion after section of both vagi, with loss of substance. In three of these, the lacteals and thoracic duct were found full of chyle. From these experiments, he concluded, that, though by division of the vagi a deleterious influence is propagated downward to the stomach, disturbing the functions of the digestive organs; yet, if the animal live long enough, this goes off, and digestion proceeds as before. He related some experiments to prove, that alcohol, opium, and prussic acid exert their deleterious effects as rapidly when injected into the stomach after division of the vagi, as when these nerves are left entire. He then detailed the result of five comparative experiments upon the effects of fatal doses of arsenic upon the watery and mucous secretions from the inner surface of the stomach and intestines. These results were completely at variance with the statement of Sir B. Brodie, that section of the vagi arrests the usual mucous and watery secretions of the stomach and intestines observed after poisoning with arsenic. He stated, that he had of late carefully watched the effects of division of the sympathetic nerve on the eye, and that he had satisfied himself that the contracted state of the pupil, and the partially-closed eyelids, which accompany the inflammation of the conjunctiva, consequent upon section of the sympathetic, take place previously to the inflammation of the conjunctiva, and are independent of it.

Prof. Owen requested to know, if albumen were discovered in the thoracic duct after the division of the vagi nerves. Dr. Reid replied, that no chemical analysis was made of the contents, but true digestion was inferred by him from the return of health and activity to the animal operated on.—Dr. Golding Bird pressed the same objection as Professor Owen, and wished that an experiment on the subject should be followed up by such a chemical investigation as would be decisive.

(TUESDAY, *Aug.* 21.)

The Section did not meet until half-past eleven o'clock, as the greater number of members had gone, by invitation, to be present at the operations performed in the infirmary. The first paper was read by Mr. Torbock on Uterine Hæmorrhage, the object of which was to introduce a new mode of applying pressure in certain forms of the disease. A few cases were adduced in proof of its efficiency.

A conversation ensued, in which the danger of relying on any *merely* mechanical treatment, to the exclusion of fundamental principles previously established, was pointed out by Drs. Granville, Lynch, and Knott.

Dr. Robert D. Thomson then read a paper, by Mr. N. Farr, "On the Law of Recovery and Mortality in Cholera Spasmodica."

Dr. Thomson introduced his subject by making some general remarks upon the importance of applying the precise language of mathematical science to all branches of natural knowledge; and, after apologising for bringing mathematical formulæ before medical men, stated that the only mode of placing medicine on a level with the sciences which are of a demonstrable, and therefore irresistible nature, will be by establishing it on a mathematical basis—for until we can calculate with absolute precision on the natural laws of diseases, and on the action of remedies, we can be only said to labour in our vocation like the ancient mariners, who could never venture beyond the confines of the ocean without suffering the most disastrous consequences. Dr. Thomson had been induced to bring the present subject before the Medical Section at the request of the able author, whose absence he regretted, for the purpose of showing that the law which regulated the disease so well known under the name of Indian cholera was as precise as any of those which guided the heavenly bodies in their courses. It was a curious fact that the state of medicine was so far in arrear in this country, that, although numerous cholera hospitals had been established in this country during the prevalence of that disease, not one of their conductors had paid any attention to the proper mode of registering the features of the disease, so as to render them available for drawing conclusions of any value; and that the only place which had hitherto afforded the proper data for this purpose was Rome, a city to which many persons were ignorantly inclined to deny any credit in a scientific point of view. The subsequent conclusions were deduced from 9372 registered cases, in 1837, published by the Roman Board of Health in their report, for which the author was indebted to Sir James Clark. It affords a striking proof of the importance of Boards of Health, and of the great superiority of the Papal provisions for the health of the inhabitants, and formed a striking contrast to the laws of England. It would be highly desirable, in the absence of such provisions by government, that physicians of hospitals should decide upon a uniform method of registering the principal features of diseases. The principal points to attend to were the *day of the disease*, or day of attack: second, the *day of recovery*, distinguishing males from females; and third, the *day of death*. From data of this kind the extensive tables presented to the meeting were deduced. One of these tables exhibited the numbers dying and recovering on each day after attack, and contained calculations from theoretical considerations, which closely approached the amounts derived from facts. The following table will show this more clearly:—

Out of one hundred cases constantly sick:

| | Deaths. | | Recoveries. | |
|---|---|---|---|---|
| | Observed. | Calculated. | Observed. | Calculated. |
| 5th day | 5.471 | 5.650 | 6.747 | 6.747 |
| 6th | 5.684 | 5.056 | 8.295 | 7.929 |
| 7th | 4.500 | 4.523 | 9.219 | 9.317 |

The following table expresses the probability of recovery and death during the first ten days after attack:

| Days. | Probability of Recovery. | Probability of Death. |
|---|---|---|
| 0 | .422 | .578 |
| 1 | .542 | .458 |
| 2 | .668 | .332 |
| 3 | .729 | .271 |
| 4 | .763 | .237 |
| 5 | .791 | .209 |
| 6 | .821 | .179 |
| 7 | .843 | .157 |
| 8 | .862 | .138 |
| 9 | .873 | .127 |
| 10 | .883 | .115 |

From the tables may be deduced the following problems: viz. 1st, the mean duration of the disease; 2d, the mean future duration of the disease at any period; 3d, the probability of dying at any period of the disease.

Dr. Granville deprecated the attempts to introduce laws mathematically drawn or expressed, into the immediate subjects of medical enquiry such as the present. The laws derivable from physical enquiries, such as astronomy, had their value from the known laws of gravitation, but no similar fundamental law was known in physiology; and the innumerable modifying circumstances, such as differences of age, sex, treatment, and diversity of constitution, would interminably disturb any deductions thus made on the questions of disease.—Dr. Thomson, in answer, referred to the close coincidence of the law (expressed by a mathematical curve) and the observations recorded, and explained some points in the structure of the tables.—Dr. Sargent differed from Dr. Granville as to the impracticability of ever arriving at any approximation to such a law as that now exhibited. It was impossible to view the tables before them, even cursorily, to compare them with the manifest deduction as to regular gradations, and with his own observations on the gradual diminution of the mortality of the dreadful epidemic in several invasions, without coming to the conclusion, that the coincidence or approximation of the observations and calculations must have a foundation in nature. As to the modifying circumstances mentioned, these were difficulties, no doubt, not to be overlooked: and did not disturbing elements and difficulties occur in physical investigations?

Mr. Joseph Blake then read a paper "On the action of various Substances on the Animal Economy, when injected into the Veins," in which were detailed a number of experiments with various substances, and their effect on the vascular system, measured by an ingenious instrument, which the author denominated a Hæmadynameter, formed by a glass tube, having both extremities proceeding from an angle,—one being attached to a scale, measuring the height to which a column of mercury was raised by the action of the current of blood in the artery, into which the other extremity was introduced. The substances introduced, in solution, into the veins were divided into three classes, from their effects. In the first, were those which produced death, by directly acting on the contractility of the heart, amongst which were nitrate of potassa, arseniate of potassa, sub-carbonate of soda, biniodide of arsenic, oxalic acid, and solution of galls; all these acted locally on the heart, and agreed in effecting a change in the colour of the blood, turning it black, probably by forming definite combinations with its constituents. A remarkable difference was observable from the effects of the same substances when absorbed from the stomach. In the second class, were those substances which acted directly on the nervous system; such were strychnia, hydrocyanic acid, and conia. And in the third, were those producing death by affecting the capillary circulation; such were tobacco, euphorbium, and digitalis. The last two classes of substances did not produce any change on the composition of the blood. Several other substances were experimented with, not falling under the above classes, such as morphia and cantharides, the effects of which were the same,—and nitric acid: when the latter was injected into the vein, the column of mercury in the instrument fell from seven inches to one; and after death, the right side of the heart was distended with solid blood.

Dr. Lynch and Mr. Torbock corroborated the observations made in the first class of substances employed, having observed the same changes in the blood produced by acids under different circumstances.

Dr. Barnes then read a paper, containing a report of two cases of well-marked abscess of the lungs from acute inflammation.

(WEDNESDAY, *Aug.* 22.)

Dr. Yelloly (the Chairman) laid before the Section, a model of an improved acoustic instrument, for the purpose of assisting in cases of partial deafness. He alluded to the very defective nature of our present instruments, both as to utility and convenience, and the importance of having some experimental researches made on the subject; and he proposed, that the Section recommend to the General Committee the appointment of a committee to investigate the subject experimentally, and to make a report on the subject, at the next meeting of the Association. Dr. Granville seconded the motion, and proposed that Dr. Yelloly and Dr. Arnott should be named as the Committee.

Professor Owen then read a paper "On the Structure of Teeth, and the resemblance of Ivory to Bone, as illustrated by microscopical examination of the teeth of man, and of various existing and extinct animals."

Mr. Owen commenced by showing, that he had availed himself of the advantage afforded by the British Association—viz., that in the communications brought before a sectional committee, a fuller and more detailed retrospect of the progressive steps which have led to any remarkable discovery, is not only permissible, but peculiarly congenial to its general views and objects; and he therefore entered into a full detail of the recent investigations, especially those of Purkinje, Müller, and Retzius, on the intimate structure of the teeth, and particularly dwelt on the discoveries of the latter author, as regarded the structure of the human tooth. After describing the mode of arrangement of the particles of the earthy salts, which characterizes true bone, Professor Owen proceeded to state, that until a very recent period the analogy of tooth to bone was supposed to extend no farther than related to the chemical composition of the hardening material, while the arrangement of this earthy constituent, as well as its mode of deposition during the growth of the entire tooth, were considered to be wholly different from that of bone, and to agree with the mode of growth of hair, and other so-called extra-vascular parts, with which the teeth undoubtedly closely correspond in the general vital properties. He observed, that the supposed proofs of the laminated structure of teeth, derived from the appearances presented by the teeth of growing animals, fed alternately with madder and ordinary food, and by those which often occur during the progress of decomposition of certain teeth, which are then resolved into a series of concentric or superimposed laminæ, were equally applicable to true bone, and were quite unavailable in illustrating the point under consideration: and that the appearances presented by the superficies of vertical sections of teeth, viewed with the naked eye or a low magnifying power, were due, not to the intervals of separate and superimposed lamellæ, but to the different refractions of light, caused by the undulation of a series of parallel tubes proceeding in a contrary direction to the supposed lamellæ. This apparent lamellated structure, however, is not constant, nor equally plain in different teeth; on the contrary, the fractured surface, or the polished section of the human and many other teeth, presents a silky or iridescent lustre, which has attracted the attention of several anatomists. Professor Owen observed, that Malpighi, in whose works may be detected the germs of almost all the great anatomical truths, which have subsequently been matured and established, conceived that the teeth were composed of minute fibres reticularly interwoven; and Leewenhoek in

1683, had discovered, that the apparent fibres of tooth were, in reality, minute tubes. The tubular structure of ivory was rediscovered by Purkinje, in 1835, who also added several interesting facts regarding the structure of the enamel, and more especially determined the nature of that distinct layer of substance, which surrounds the fang in the simple teeth of man and carnivorous animals. The interesting experiments of Professor Müller, on the nature and contents of the *dental tubuli* were then noticed; and, lastly, a condensed analysis was given of the laborious and accurate microscopical observations of Professor Retzius, as related in the original Swedish memoir of that author, on the structure of the teeth. Besides confirming the fact, that the ivory or bony constituent of a tooth consists of the fundamental *zahn-substanz* of Purkinje, minute tubes lodged in a transparent medium, disposed in a radiated arrangement in lines, proceeding in a direction perpendicular to the superficies of the tooth, Professor Retzius has more particularly observed, and described the dichotomous branching of the primary tubes; the minuter ramuli sent off throughout the course of the main tubes into the clear interspaces; the calcigerous cells with which those fine branches communicate, and the terminal ramifications of the tubuli; their anastomoses with each other, and with calcigerous cells at the superficies of the ivory, or bony part of the tooth. Professor Owen also discussed the opinion advanced by Professor Retzius, as to the function of this elaborate contexture of branched and anastomosing tubes and cells, in conveying, by capillary attraction, a slow current of nutritive or preservative fluid, through the entire substance of the tooth; which fluid might be derived either from the superficies of the pulp in the internal cavity of the tooth, or from the corpuscles or cells of the external layer of cortical substance or *cæmentum*,—with the tubes radiating from which corpuscles, the fine terminal tubes of the ivory anastomose. Professor Owen concluded the critical portion of his communication, by explaining the views entertained by Professor Retzius on the analogy subsisting between tooth and bone, which analogy he then proceeded to illustrate by his own observations on the structure of recent and fossil teeth. [For the descriptions given by Professor Owen of these teeth we cannot find room.]

In the summary of the series of observations which Professor Owen detailed, he observed, that in the human tooth, and similarly highly organized teeth, the analogy of ivory to bone, as to texture, was only seen in the existence and intercommunication of the minute calcigerous tubes and cells; but that there was no trace of medullary or Haversian canals with their characteristic concentric laminæ, unless the entire tooth were regarded as analogous to a single enlarged Haversian canal; the cavity of the simple pulp representing the medullary cavity of the canal; while the tubes, with the appearance of laminæ occasioned by their undulations, were equivalent to the concentric lamellæ and the calcigerous tubes, which, in bone, traverse these lamellæ, and radiate from the Haversian canal. In the teeth of many of the lower animals, however, and especially that of the extinct *Acrodus*, amongst the cartilaginous fishes, the resemblance of the dental tissue to bone was extended to the existence of the characteristic Haversian canals in great numbers. The presence of these canals was explained by the progress of the development of these bone-like teeth, as observed by Prof. Owen in recent cartilaginous fishes. The large pulp, at the commencement of the formation of the tooth, had exercised its ordinary function in the secretion of a close-set series of calcigerous tubes, having a general direction perpendicular to the surface of the tooth, and closely resembling true ivory. The pulp, then, instead of continuing to form similar tubular ivory, by adding to the extremities of the previously formed tubes, become subdivided, or broken up into numerous processes, to which those forming the three fangs of a human grinder are analogous; but each process here becomes the centre of an active formation of similar branched tubes, radiating in all directions from that centre, and anastomosing, by their peripheral branches, with those from contiguous centres, or communicating with interposed calcigerous cells: the cavities containing the above subdivisions of the pulp, like the Haversian canals, containing the processes of medulla in true bone, have had their area diminished in like manner by the successive formation of a series of concentric lamellæ; traversed, as in true bone, by radiating and minutely ramified calcigerous tubes, communicating with each other and with the minute cells in the interspaces.

The resemblance of the pulp canals of the teeth of *Acrodus* and of the medullary canals of bone, is further exemplified in the existence of lateral communications in teeth; and, in function as well as structure, they might be regarded as being identical. With reference to the application of the tubular structure of the teeth to the explanation of their pathology, nothing has hitherto been attempted. Prof. Owen observed, that it was a new and fertile field, which would doubtless be replete with interesting results, and might suggest some good practical improvements in dental surgery. Ordinary decay of the teeth commenced, in the majority of instances, immediately beneath the enamel, in the fine ramifications of the peripheral extremities of the tubes, and proceeded in the direction of the main tubes, and, consequently, by the most direct route to the cavity of the pulp. The decayed substance, in some instances, retains the characteristic tubular structure, which is also observable in the animal basis of healthy teeth after the artificial removal of the earthy salts. The soft condition of the decayed portion of a tooth is well known to all dentists: it depends upon the removal of the earthy salts from the containing tubes and cells, in which process the decay of teeth essentially consists. The main object of the dentist seems, therefore, to be, to detect those appearances in the enamel which indicate the commencement of decay; to break away the enamel, whose natural adhesion to the ivory will be found more or less dissolved at the decayed part; to remove the softened portion of the ivory, and fill up the cavity with gold or other substance. Experience proves what theory cannot account for,—viz. that the progress of the decay is sometimes thus permanently arrested. The calcareous salts are in such cases, as it were, poured out from the extremities of the tubes divided in the operation, and a thin dense layer intervenes between the exposed surface of the ivory and the stopping.

In conclusion, Professor Owen passed in general review over the structures which he had described in detail, and remarked, that through the endless diversity which the teeth of different animals presented, the general law of the tubular structure could be unequivocally traced; and that the general tendency of the modifications observable in descending from man to the lower classes of vertebrate animals, was a nearer and nearer approximation of the substance of the tooth to that of ordinary bone.

Numerous sections of the teeth described, prepared for microscopic examination, were exhibited to the Section.

Dr. Reid then gave a brief notice of his researches "On the Quantity of Air required for Respiration." He pointed out imperfections in previous experiments, particularly that consisting in the small number of individuals experimented on. A great difficulty existed in attempting accurate conclusions, from the diversity of constitutional temperaments, different states of humidity of the atmosphere, the state of insensible perspiration, and also from the admixture of small quantities of foreign gases; in one instance, the admixture of $\frac{1}{5000}$ part sulphuretted hydrogen, was enough to "knock up" a whole room, producing very serious effects. The degree of light was also an important element. Dr. R. stated, that, in recovering from the effect of various oppressive atmospheres, to which he was necessarily at times subjected, and some of which contained ten per cent. of carbonic acid, he always recovered more quickly, and felt in a short time more thoroughly refreshed, if he was exposed, not only to a fresh and free atmosphere, but also to a brilliant light. He added, that at St. Petersburgh, he was informed by Sir James Wylie, that the cases of disease on the dark side of an extensive barrack, were in the proportion of three to one, to those on the side exposed to strong light, and this uniformly so for many years. Dr. Reid explained the mode he had adopted to ventilate the House of Commons, which he illustrated by diagrams, and demonstrated by the exhibition of a glazed model of the House. The current of fresh air could be introduced either from below or from above, diffused uniformly, and not by violent draughts, but as it were insensibly, and was under the most exact control as to quantity. The air, when used for the purposes of respiration, and combustion, was conveyed away in an opposite direction to that in which it had been introduced. In answer to a question, Dr. Reid said, that he had taken no account of the products of the combustion by which the heat and light were produced, as these products should be omitted in all calculations on the subject. They, if possible, should be carried off so as not to interfere with the immediate supply to each individual. For the purpose of raising the temperature, hot water was used in iron tubes, not raised above 150°. Dr. Reid also stated, in answer to other questions, that he had not made any particular observations on the modifying influence of different articles of clothing; but he believed they did modify considerably the question; those being preferable that were of a very porous nature, allowing an *insensible* application of the atmosphere to the cutaneous surface.

Dr. Inglis read a paper containing "Remarks on the Skull of Eugene Aram."

Proofs of the identity of the skull were first adduced. The head was removed from the body, which, according to the sentence, was hung in chains at Knaresborough, by Dr. Hutchinson, of that town, whose widow married a gentleman still living, from whom it passed into the hands of the Rev. Mr. Dalton, from whom Dr. Inglis received it. Mr. Dalton, it appears, sent it to Spurzheim, who mistook it for a female skull. The paper stated that the development of the mental faculties, as indicated by the skull, agreed in a remarkable manner with the character of Aram as recorded. Strictures on the nature and amount of the evidence by which he was condemned were brought forward; and these strictures were enforced by the probable character of Aram, as indicated by the skull. A long and desultory conversation ensued, in which the evidence of the identity of the skull was denied by some members, and acknowledged by others; and the importance and truth of phrenology were alternately asserted and denounced as chimerical and absurd.

### (Thursday, *Aug.* 23.)

Previously to the reading of any papers, three recommendations to the general committee passed the Section:—1st. That a communication should take place with members of the Medico-Chirurgical Society of London, in order that papers read at this Section might be occasionally published in their Transactions. 2d. That application should be made for a grant of 200*l.* from the funds of the Association, for the purpose of bringing over to this country, and retaining here for one year, Alexis, mentioned by Dr. Beaumont, in his work on Digestion, for the purpose of making physiological and chemical researches on the subject of digestion, in connexion with the Chemical Section.* The committee proposed for the investigation were, Drs. Thomson, Prout, and Graham, and Professor Owen. 3d. For the appointment of a committee to communicate with previously appointed committees, for the purpose of furnishing reports on particular subjects for which pecuniary grants were given. This step was considered necessary, from the fact that no communication had been received from some of those committees.

Dr. Granville then laid before the section an improved Stethoscope, the advantages of which were the facility of examining the patient without the necessity of his rising, and also of avoiding the necessity of having the practitioner's head immediately parallel to the part examined: these advantages were effected by the addition of a half ball-and-socket joint attached to the ear-piece, which of course became moveable to a greater or less angle with the cylinder, as circumstances might require.

Dr. Rees read a paper "on the Chemical Nature of the Liquor Amnii." The principal points established were the specific gravity being found lower than the previous statements, being only 1.007, as an average of different results; that the fatty matter discovered in it was different from the fatty matter of the blood; and that urea was discovered in every specimen examined.

Dr. Bird observed, that this last fact tended to support the generally received opinion of German physiologists,—namely, that the liquor amnii was a secretion from the fœtal kidneys.

Mr. Baird detailed a case of successful excision of the elbow-joint. The patient was presented to the Section, and considerable motion was shown to exist in the joint; so much so as to enable him to pursue his ordinary occupation in a glass manufactory.

Dr. R. D. Thomson read a paper "on the Modus Operandi of Nitrate of Silver as a Caustic and Therapeutic Agent."

It was stated that light had no power whatever in decomposing nitrate of silver. In every instance in which this apparently occurred, it had been previously rolled in paper, which afforded an opportunity for it to combine with organic matter, and thus produce the effect upon which the general opinion was founded. A specimen of pure nitrate of silver, placed in a tube hermetically sealed, was exposed on the top of a house for a considerable period, without the slightest discoloration. The specimen was exhibited. To illustrate the action of nitrate of silver when applied to the skin or a diseased surface, Dr. T. added a solution of this substance to a solution of white of egg, or albumen: he found that two distinct compounds resulted, one soluble, the other insoluble in water. He contended that these compounds were formed when it was applied to an organized surface, and that, they being removed, the caustic effect was thus produced; and that, in the internal use of this substance, the doses which could be administered in safety were proportioned to the quantity of albuminous matter contained in the stomach; and that, in the event of a deficiency, the coats of the stomach would be acted upon in the same manner, and death ensue; but that it was impossible for the substance unchanged to become mixed with the blood, and deposited on the surface, as was generally believed.

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* This was proposed by us in our last Number, p. 181.

Mr. Greenhow read a brief memoir on Fractures, for the purpose of introducing to the Section a model of a new sling fracture-bed, applicable to every fracture in the lower extremity, but peculiarly adapted to the treatment of compound fractures of the femur. The following advantages were attainable by the present apparatus: 1st, ease of position; 2d, easy and gradual extension by means of a screw at the termination, beyond the heel of the patient, whose action was connected with the ankle-joint and instep; 3d, facility of examining and dressing the limb in cases of compound fracture, without disturbing the fractured ends; 4th, the freedom of slight motion, enjoyed in such a way as to be of no injury to the process of reparation. Mr. Greenhow detailed some interesting cases treated with this apparatus, demonstrating its peculiar advantages.

(FRIDAY, *Aug.* 25.)

Dr. Bowring was introduced to the Section, for the purpose of communicating some observations on Plague and Quarantine, made during his residence in the East.

Dr. Bowring apologized for coming before the Section, he not being a medical man, but having travelled in the East for the purpose of observation, in reference to our commercial relations, his attention had been naturally directed to the subjects of plague and quarantine; subjects, the importance of which could hardly be overrated, many millions being annually lost to this country from quarantine regulations. The results of his observation had produced a strong conviction of the non-contagion of plague; and he thought it right, therefore, to lay before this Section a few remarks on the subject. He alluded to the very secondary character of the facts on which the prevalent opinions were founded. Some were so absurd as not to be worthy of the slightest attention: such as plague being introduced at Leghorn by the unrolling of a mummy that had been buried for 2000 years,—at Constantinople, by the wing of a bird having touched a kite which a boy was flying from a house-top,—from a cat having been seen to jump into a basket in which were some clothes, from which the disease was subsequently caught. Dr. Bowring said, that physicians residing in the East were rapidly changing their opinions on the subject; but they were prevented, in many instances, from freely expressing them, by the interested Boards of Health, who neither liked to part with their extensive power (even of life and death), or with their salaries. Clot Bey was a decided anti-contagionist, and that after an experience of 8,000 or 10,000 cases. Dr. Bowring mentioned many cases where facts were distorted, or invented, to account for cases of plague from contagion; and in one remarkable instance, where the misrepresentations were exposed, it was denied that the case was plague at all, because they could not maintain their assertion of contact having taken place. The Mussulmans are by their religion non-contagionists, and Dr. Bowring hoped they might never become otherwise, as the aggravation of the calamity would be tenfold if they did. The opinion as to the contagious nature of the disease prevailed principally amongst the Levantines and Franks; but every other superstition was as readily believed by them. He had collected the most solemnly attested evidence of the appearance of peris and genii, and of the intermarriages of the former with mortals: he had collected more strongly attested *facts* on these subjects than on the contagious nature of plague. Dr. Bowring asserted that, from innumerable instances, quarantine appeared to give no security; and was of opinion that these establishments were mere political engines, of great power and convenience in a despotic country. In the lazarettos, the whole correspondence of the East was read. The Russians had a most perfect system of quarantine, yet the plague got into Odessa. In 1831, quarantine and lazaretto establishments were introduced into Egypt, under the superintendence of the consuls; yet the plague got into Egypt. In Jaffa it broke out in the house of the Russian consul spontaneously; and in Jerusalem in a convent, with which there could have been no communication. In the lazarettoes, a little disease was made a great deal of: in one instance, a greater number died from dysentery than from plague. Lazarettos, he contended, rather increased than diminished the evil. If a strict separation could ensure safety, the pacha's harem would escape; yet, in 1835, seven died there of plague. It appeared at one time in Old Cairo, and not in New, and *vice vers* , although there was constant communication: the same was true as to Cairo and Alexandria. The disease never penetrated Nubia, though constantly on the borders, and frequent intercourse taking place. In Cairo, on one occasion, 400 or 500 houses, whose inhabitants had all perished, were subsequently opened, the linen and clothes in them sold in the market-place, without any cases of plague resulting. Clot Bey had again and again inoculated himself without producing the disease. Dr. Boulard wore the clothes of a patient, who died of the disease, for twenty-four hours, without catching it. The following information was communicated to Dr. Bowring, by a physician of long experience, in answer to a series of direct queries, viz.: that it is indigenous in Egypt, never entirely absent, never imported, that it frequently occurs spontaneously, that cordons afford no security, that contact very frequently did not produce it, and that the most cautious frequently suffered from it, that free ventilation was effective in checking the disease, that it was not produced by linen which had been exposed to the infection, and that when a number of persons exposed to its influence removed from the spot, the mortality became much diminished. Dr. Bowring concluded by expressing his own strong conviction on the subject; but he had no object but to promote the discovery of truth, which could only be done by patient and serious enquiry, and by evidence of a primary character.

Dr. Lynch begged leave to propose a resolution, to the effect, that Dr. Bowring's paper should be published.—Mr. Greenhow could not but remark on the strong analogy, which existed between the statements of the learned gentleman on plague and lazarettoes, and the events which occurred in this district relative to cholera and cholera hospitals. He also requested Dr. Bowring to furnish a copy of his observations to the Secretaries.—Dr. Granville opposed the motion of Dr. Lynch. The communication he contended was not strictly medical; it was political, not pathological: he must oppose too the deductions of Dr. Bowring; he was not indeed present at the commencement of his address, but that was the less necessary, as he had heard the whole subject in detail, on board the *Ocean* steam-packet. It was not by declaiming on the superstition of the Levantines, or by the eloquent introduction of Peris, Genii, and Vampires, that a strictly pathological question could be decided, nor by false inferences from bad Boards of Health.—The Chairman interrupted Dr. Granville, by protesting against this line of personal attack, and allusion to what had taken place elsewhere. After some desultory observations, Dr. Lynch's motion, somewhat modified, was passed. Dr. Granville then begged leave to propose that the Committee should make application to Her Majesty's Government for a grant to be applied to the purposes of enquiry into the important subject of the contagious or non-contagious nature of plague. This motion passed unanimously, as also one of thanks to Dr. Bowring.

Mr. Goodsir then read a paper "On the Origin and subsequent Development of the Human Teeth."

The author has observed dention commence by the formation of what he denominates the primitive dental groove, on the floor of which the rudiments of the pulps of the milk teeth appear as globular or conical papillæ; septa afterwards pass from the outer to the inner side of the groove, between the papillæ, and thus each of the latter becomes situated in an open mouthed follicle, which is the primitive condition of the future sac. After the formation of the milk follicles, the lips of the groove still remain prominent; and when in this condition he denominates it the secondary groove. The rudiments of the ten anterior permanent teeth appear as little depressions in the secondary groove, internal to the mouths of the milk follicles. The papillæ of the milk teeth now begin to be moulded into the form of pulps, a change which is synchronous with the closure of the mouths of the follicles by two or more laminæ, which agree in number, shape, and position with the cutting edges and tubercles of the future teeth. The lips and walls of the secondary groove now adhere, except in the situations of the ten depressions for the permanent teeth, and for a small extent posteriorly on each side, where a portion of the primitive dental groove remains in its original condition. In this portion the papillæ and follicle of the first large molar tooth appear, and, after it closes over, the lips of the secondary groove above it adhere, but not the walls; so that there is in this situation a cavity which produces the sacs of the two posterior permanent molars. The first large grinder may, therefore, be considered in some measure a milk tooth. The author observes, that dentition begins, and is always in advance, in the upper jaw, except in the case of the incisive teeth, which, although they appear first, are later in coming to perfection. This he explains by the tardy development of the lateral elements of the intermaxillary system. The author divides dentition into three stages. The first is one with which anatomists have hitherto been unacquainted,—viz. the follicular. The second and third they are familiar with—the saccular and the eruptive. From his researches, he concludes that the human teeth originate from mucous membrane; that the permanent teeth have no connexion with the deciduous set, and that the sac and pulps must be referred to the class of organs denominated bulbs. He anticipates the discovery of the follicular stage in the dentition of all animals, and if so, that it will explain the varying and complicated forms of the pulp and sacs.

Dr. Dalziel brought before the Section a model of an apparatus for the purpose of promoting respiration during sleep. Its object was to diminish the amount of atmospheric pressure on the surface of the body, whilst the patient was respiring atmospheric air of the usual density; and this principle might be applied in various diseased states with benefit, and particularly to the recovery of individuals labouring under suspended animation.—On the motion of Dr. Knott, it was resolved that the apparatus be recommended to the notice of the Humane Society, for the purpose of making trial of its efficacy.

A paper, entitled "Experiments and Observations on the cause of the Sounds of Respiration," by Dr. Spittal, was then read.

The object of this communication was to show, that the theory of Laennec, in regard to the cause of the respiratory sounds,—viz. that all those known by the terms vesicular, bronchial, tracheal, as well as cavernous and amphoric respiratory murmurs, are caused by the friction of the air against the parietes of the air-cells, bronchial tubes, trachea, and of cavities of different dimensions, has never been proved; and that the few experiments which have been advanced in support of it, are far from establishing the conclusions which have been deduced from them; and that it is highly probable that, according to the theory of M. Beau,* these sounds, either owe their existence to, or are in part produced or modified by, the transmission or reverberation of a sound which takes place in the superior respiratory passages, and which has been termed, by M. Beau, the "guttural" respiratory sound. In support of the first theory, it was observed, that the best and almost the only experiment was that of Magendie, in which air was blown into the lungs by means of a pair of bellows, and in which sounds, resembling the respiratory murmur, were perceived, and from which M. Magendie drew the conclusion, that because air passed to and from the lungs during this experiment, as well as during respiration, therefore, the respiratory sounds are produced by the friction of the air against the parietes of the bronchial tubes and air-cells of the lungs. It was stated that the similarity between the sound produced by a pair of bellows, and the guttural sound, was admitted by Laennec; and that it was also observed that a similar sound could be produced by blowing air through almost any tube; differing in tone and degree, according to the diameter or shape of the opening in the tube—the force with which the air is made to issue from it—or the nature of the materials of which it is composed. The experiments of M. Beau, in support of his particular theory, it was noticed, were open to objections, and did not seem to bear out very clearly the conclusions at which he arrives; which may perhaps account for the neglect his view of the subject has met with. For the purpose of obviating these, and showing in a more distinct manner the probable truth of this theory, to a certain extent at least, several experiments were devised, calculated to prove this in a less objectionable manner. In these experiments no stream of air was allowed to pass through those parts the subject of observation, which were only allowed to become, and remain distended, with air; while, at the same time, the sound produced by the issuing of the air from an air-condensing apparatus, or from the mouth,—and which very nearly resembled that of the bellows, which again resembles the guttural sound, —was observed to have passed freely, in one experiment, throughout an artery of eighteen inches in length, and to be perceived very nearly, if not quite, as loud in this as in another artery connected with it, and through which a current of air passed. In another experiment, in which the lungs of a lamb were used, sounds analogous to the tracheal, bronchial, and vesicular, respiratory murmurs were distinctly perceived, although no current of air passed along the air tubes or cells; and in the case of a bladder, attached to one of the great bifurcations of the trachea, a sound louder than that in the bronchial tubes was perceived, when the former was contracted to about an inch and a half or two inches in diameter; feebler when larger; and assuming, as its size was increased, a gentle, shrill, ringing, amphoric character. In these different observations, no current of air passed along the parts the subject of examination, but was conveyed away in a manner which our space will not permit us to describe. These experiments were not advanced to prove that the guttural sound, or that which takes place in the superior respiratory passages, is the only source of the respiratory murmurs; but to show that in all probability it exerts a considerable influence, if not in producing, at least in modifying, the different respiratory sounds, known as the vesicular, bronchial, tracheal, cavernous, and amphoric respiratory murmurs, all of which have hitherto been explained according to the views of Laennec.

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* Archives Générales, Paris, 1834.

(SATURDAY, *Aug.* 25.)

The section met at half-past ten o'clock, to hear some papers, which could not be brought forward for want of time at the former sittings.

Mr. Crawford read a paper "On Anthracosis, occurring in an individual who had worked in a lead mine."

A conversation ensued as to the prevalence of this disease in the coal mines around Newcastle. The opinions seemed contradictory on the subject.

"On the Medicinal and Poisonous Properties of some of the Iodides," by Dr. A. T. Thomson.

The principal preparation whose action was detailed was the iodide of arsenic. Different modes of preparation were pointed out, its characters described, and specimens of the preparation handed round. The action of this medicine in very minute doses, from one-eighth to one-third of a grain, was peculiarly serviceable in lepra vulgaris, and chronic impetigo. A case of numerous tumours resembling carcinoma was found to yield to its continued action, and it was found equally successful in a more decided case of incipient carcinoma. Its action as a poison, when given in overdose, was minutely detailed in a series of experiments on dogs; its action being very similar to that of arsenious acid. Coloured drawings of the morbid effects on the alimentary canal were exhibited; when injected into a vein, its effect was to destroy life by destroying the irritability of the heart.

Dr. Adams read a paper "On the Placental Souffle," detailing some remarkable stethoscopic phenomena occasionally heard in connexion with it.

### MEDICAL RELIEF UNDER THE NEW POOR-LAW.

In our Ninth Number (p. 310) we announced our intention of soon considering in detail the subject of medical relief under the New Poor-law; but we deemed it right to wait the result of the enquiry about to take place before a committee of the House of Commons, directed, among other matters, to this particular subject. That enquiry has taken place; and we have now before us, in the 44th, 45th, and 46th Reports of the Committee, the evidence of the medical witnesses on all the points that have been agitated relating to the medical attendance on the sick poor. The gentlemen examined before the committee were, Sir Astley Cooper, Drs. Kay, Webster, R. D. Thomson, Elliotson, Hall, and Messrs. Rumsey, Ceely, Rowe, Farr, Evans, and Toogood; and, as all these gentlemen, with the exception perhaps of Dr. Kay, the assistant poor-law commissioner, were called by the committee who kindly undertook the management of the enquiry on the part of the British Medical Association and Provincial Medical Association, it is fair to take their evidence as exhibiting all the grievances which the profession had to complain of, and as affording the best suggestions for their relief. To the gentlemen just referred to, and above all to Mr. Wakley, (who conducted the examination of the witnesses, with much ingenuity and good sense, and great moderation,) the profession and the poor are greatly indebted, for the improvements which, we doubt not, will flow from this enquiry. We are happy to find that the improvements which we stated in the notice referred to as being especially wanted,—viz. "the establishment of fixed salaries in place of the system of *tenders*, the subdivision of districts, and the allotment of them with a more constant regard to the residence of the medical men," are likely to be all carried into effect. These were, indeed, the principal improvements recommended by the witnesses examined; the only others of much consequence being the appointment of medical assessors or guardians in each Union, and of one medical commissioner at the general Board in London. It will be seen by the official Report, which we subjoin, that both these recommendations have been rejected by the committee; and it is probable they will not be adopted by the government. We confess we regret extremely that the suggestion respecting a medical commissioner is not likely to be attended to; as we think such an addition to the Board is calculated to be of great advantage, both to the profession and the poor; although we think it of inferior importance to the other changes recommended, or likely to be adopted. We have great doubts, however, as to the other proposition respecting medical guardians: we are by no means sure that the creation of such a local authority might not rather tend to ferment than to allay the heats and jealousies which, we regret to say, too often disturb the harmony of medical men in small communities.

#### *Report of the Committee.* (*Extract.*)

On the subject of medical relief, your committee examined several gentlemen connected with that profession. Mr. Rumsey, Secretary of the Provincial Medical and Surgical Association, complains that the paupers have a difficulty in obtaining medical assistance; that the size of the districts is too large, and the number of medical officers too small. He objects to medical men being controlled by persons incapable of judging of their proficiency; to their appointment by public advertisement and tender rather than on previous services and character; and he says the medical men accept these appointments, however inadequately remunerated, from an apprehension that strangers will be introduced, and that thus they might be deprived, not merely of the care of the poor, but of a part of their private practice. He produced instances of the too great size of medical districts, and of a reduction in the number of medical attendants on the poor; he says that in the former state of the law the attendance was often bad and the pay inadequate, and a want of control was felt, but that the districts were more convenient. The system of appointment by tender did also prevail under the old system, but not to such an extent as under the new. He approves of the weekly returns made to the board of guardians; and he suggests, as the principal points on which an amendment is required, the appointment, the remuneration, and the supervision of the medical officers. He would prefer a separate parochial appointment. If that be not assented to, that at least the districts should be smaller; and he suggests twelve miles square as the extent of a district. With respect to remuneration, he is of opinion that where the payment is by a fixed salary, the cases attended are too numerous, and that too many families receive medical relief; and that, on the contrary, where the payment is per case, the number attended is too small. He agrees, therefore, with Dr. Kay in suggesting that a scale of remuneration, embracing both those modes, should be adopted. This recommendation in substance is, that a list of persons entitled to medical relief, including those who are in the receipt of out-door relief with others who may be deemed necessarily indigent, should be made out and given to the medical officer, and that all those who are included in the list should be entitled to medical relief without the necessity of obtaining any order from any authority, and that the cost of administering this branch of medical relief should be estimated at a certain sum per case, so as to afford an adequate remuneration. Mr. Rumsey proposes, and Dr. Kay acquiesces in the proposal, that the scale of remuneration for those paupers should be about 6s. 6d. or 6s. per case. Mr. Rumsey supposes that this would be about double the present amount of remuneration. With respect to the casual paupers who would not be included in the list of those always entitled to relief, Dr. Kay and Mr. Rumsey think the remuneration should be higher, and that they should be paid for per case; Mr. Rumsey suggests 10*s*. as a reasonable sum, and that midwifery should be paid for separately. Mr. Rumsey afterwards brought forward another scale of remuneration, but which, though calculated upon a different basis, would in his estimation produce nearly the same result. He thinks, generally, that these arrangements must be governed in some degree by local peculiarities and the general rate of medical charges.

For the supervision or control of all medical arrangements, Mr. Rumsey proposes that a medical commissioner should be appointed by the Crown, to act with the present Poor Law Commissioners, and to whom all matters connected with this subject should be referred; and that two medical assessors should be appointed in each union, one by the medical men resident in the union, and the other by the board of guardians, by whom the medical arrangements should be generally settled. In case of dispute, reference to be made to the medical commissioner, whose decision should be final. Mr. Rumsey thinks it very desirable that every encouragement should be given to vaccination. He objects to the medical clubs, where medical men are compelled to attend them. He thinks those arrangements between independent labourers and medical men should be free from any control by the guardians, and be entirely voluntary. He approves of independent medical clubs, and produces the plan of one established in the south of Buckinghamshire.

The other medical witnesses examined by your committee concur generally in the observations and recommendations of Mr. Rumsey. Dr. Webster, president of the British Medical Association, describes himself to have lost his situation at Dulwich because he would not belong to a medical club. He thinks no medical district in a town should comprise a population of more than 10,000. Mr. Ceeley, surgeon of the Aylesbury Infirmary, complains of the introduction of medical men, strangers to that neighbourhood, in consequence of the proposals of the board of guardians not being accepted by the resident practitioners. He says that this was done under the influence of the Assistant Poor Law Commissioner. He admits the evils of the former system, and that there is now more employment for the agricultural labourers, whom he describes to be a sickly race of people, and suffering from insufficient food, especially since the introduction of the new law. The wages in one part of the Union of Aylesbury are not more than 6s. a week. He thinks it of great advantage that the same medical men who attend the rich should also have the care of the poor. He agrees with Mr. Rumsey as to the size of the districts, as to the appointment of a commissioner and assessors, and generally in other particulars. The remuneration for regular paupers should, he thinks, be at the rate of 7s. or 8s. per case, and for casual poor 12s.; fractures and dislocations to form a separate item. He estimates the amount of a reasonable remuneration at from four to seven per cent. on the total expenditure for the poor. He apprehends the danger of drugs of an inferior quality being administered, from inadequate salaries, and advises that a medicine chest containing the medicines most usually required should be placed in every parish to which the medical man could have immediate access, the expense of which he supposes might be from 3*l.* to 5*l.* for a parish of five hundred inhabitants.

Dr. Thomson gave evidence on the adulteration extensively carried on in drugs, and on the danger to which the poor especially may be exposed from the circumstance. Mr. Toogood, a surgeon of great experience from Bridgewater, agreed in the suggestions of the preceding witnesses. He thought that a medical district ought not to be above four or five miles in diameter. His private practice, however, generally extends to a distance of seven or eight miles. Dr. Elliotson and Dr. Marshall Hall express their general concurrence with the other witnesses; and Dr. Hall suggests that the medical officers, besides being of two years' standing in their profession, should have practised for one year at least within the union, or in its neighbourhood.

Mr. Farr, member of the council of the Medical Association, has examined with great care and labour the returns connected with this branch of the subject, which were ordered by the House. His evidence contains many valuable statistical details, and among other results of his enquires he states, that the average area of the present medical districts is about twenty-one square miles; that of the district of Newbury, which is co-extensive with the union, is seventy-two square miles. In many other instances the size of medical districts appears to him to be inconveniently large, where the extreme points are eleven or twelve miles from the residence of the medical officer. In some unions, where the salaries are fixed, sixty-eight persons in one thousand seem to be attended; where the payment is per case, the proportion attended is fifty in one thousand. The importance of medical arrangements for the treatment of the poor may be inferred from the fact, that nearly one-fifth of the deaths in this country take place under the care of the parochial medical officers. Mr. Farr alludes to the superior pay of medical attendance on gaols; and as the present remuneration of the parish surgeon is fixed at about 3s. 3d. per case, he would propose to double it; in all other respects he coincides with the recommendations which had been previously made to your committee.

Sir Astley Cooper, having had an opportunity of seeing the evidence which had been given by the witnesses already referred to, was of opinion that the present medical districts were too large. He objected to the appointment by tender; he thought that it might be advantageous that medical chests containing the usual and most easily compounded medicines should be kept in each parish; and he concluded by stating, that you should choose the same men to attend the poor as you would be satisfied with in your own family; and that the difficulties connected with this subject would be easily settled if the districts were made smaller, suggesting a diameter of about five miles, and if no person were appointed to a medical district who had not passed the examination of Apothecaries' Hall, and of a Board of Midwifery, and received a diploma from the College of Surgeons.

Your committee again examined Dr. Kay; and they directed the evidence which he gave at an earlier stage of their enquiry, upon the subject of the medical attendance of the poor, to be reprinted, in connexion with that of the other witnesses on this branch of the subject; most of the recommendations of the medical gentlemen who had been examined, as far as the administration and the remuneration of medical relief are concerned, were founded on suggestions originally made by Dr. Kay. In his subsequent examination, Dr. Kay advises that separate arrangements should be made for attendance on the workhouse, for which drugs should be provided by the guardians, under the direction of the Poor Law Commissioners, and a few of the surgical instruments in most frequent use. The scale of remuneration for the list of paupers before adverted to should, he thinks, be about 6s. per case, independent of the workhouse. He approves of the proposal with regard to parish medicine chests, but does not think the appointment of a medical commissioner or of medical assessors necessary. In all cases of difficulty, connected either with the general arrangements or with the medical treatment of the poor, recourse may be had, and is now had, to the advice of physicians, either by the board of guardians or by the Poor Law Commissioners.

On reviewing the whole of the evidence connected with medical relief, your committee are of opinion that while most of the witnesses called on this subject agree in representing many of the evils of the old system to have been corrected, yet it does appear that the size of the medical districts is in many instances inconveniently large, and that in some cases the poor have been assigned to the care of too small a number of medical officers. They think also that it may be desirable to discontinue the practice of advertising for tenders from the medical men; this seems to have given offence to a profession, whose feelings and wishes it is important to consult. The committee believe the practice to have been originally adopted experimentally, and merely with a view to ascertain what, in the opinion of those gentlemen, was a reasonable remuneration for their services; but if offers have been made and appointments accepted by the resident surgeons at a rate below a reasonable amount of remuneration, under an apprehension that strangers to the neighbourhood might be introduced, and that a part of their private practice might be lost, together with their attendance on the poor, your committee think that this is a circumstance to be regretted, and they advise the adoption of some different mode of appointment. A state of things may undoubtedly arise in which it may be impossible to come to fair terms with the resident medical men, and when the introduction of a stranger may be indispensable: in that case great care should be taken in the examination of the qualifications of an individual of whose practice the board of guardians could have had no previous experience. The suggestions on this point by a person of the authority and experience of Sir Astley Cooper are undoubtedly entitled to great weight. Your committee also feel it to be most important that the poor should be perfectly satisfied with their medical attendants; and with this view it appears to be desirable, as indeed is almost always the case, that the care of the poor should be confided to the same person who is in the habit of visiting their richer neighbours.

With respect to the amount of remuneration for medical services, your committee do not feel themselves able to give any opinion upon the precise sums which have been named; this question must in some degree be governed by local circumstances, by the number of practitioners, by the nature of the country, by the degree in which the residences of the poor are scattered or near together, and by the general rate of remuneration previously existing in the district. Whatever may be the amount of remuneration fixed in any union, your committee are of opinion that attendance on the sick should be made a parochial charge, each parish paying for its own cases; and that it should never be made a part of the establishment charge, and distributed among the different parishes in proportion to their averages. If it should be determined to adopt the scheme of making out, either at the beginning of the year, or at any other period, a list of the persons who shall be entitled to receive medical relief on their own application, one principal difficulty will consist in determining whose name shall be inserted in the list of the permanent poor. With those already in the receipt of out-door relief there will be no difficulty; but with respect to those who, when in health, can support themselves, but who may be supposed to be unable to meet the losses and expenses of sickness, it will require a very cautious discrimination in completing the list, by which the extent of medical relief to be given and the amount of the remuneration are mainly to be determined. Your committee, however, think that the principle on which those additional names are recommended for insertion is perfectly just; and, as they think that the board of guardians must decide on those, as on all individual cases, they have no reason to believe that their choice will be indiscreetly exercised; and they cannot avoid saying that while they would advise a proper caution to be used, they are of opinion with Mr. Gulson, and other witnesses, that medical relief may with great propriety be given more extensively than any other kind of parochial assistance.

Your committee are not disposed to concur in the suggestions which have been made for the appointment of a medical commissioner by the crown, and of medical assessors by boards of guardians and the resident medical practitioners. The same considerations which govern private individuals in the selection of medical attendants for themselves and their families will, it is hoped, influence boards of guardians in selecting attendants for the poor; the same individuals, in the great majority of instances, will attend the poor in common with the other inhabitants of the district; and in this respect no class of the population will be exposed to any comparative disadvantage; neither does it appear desirable that the appointment of medical officers should be taken from the boards of guardians, and revert to the several parishes of an union. Your committee, from a feeling of respect for the medical profession, and believing that their attendance on the poor has been marked by great liberality and humanity, are anxious that the suggestions which have been made by them should be favorably considered by those who are charged with the administration of the law. They recommend the evidence which they have received on this subject to the attention of the Poor Law Commissioners; and they cannot but hope that arrangements may be made to remove some of the objections reasonably entertained to the present practice, and to put this branch of relief on a footing which shall be satisfactory to the medical men and be conducive to the comfort of the poor.

It has been recommended, and your committee agree in the recommendation, that periodical reports to the boards of guardians as to the state of health prevalent in the medical districts should be required from the medical officers. It appears desirable that great care should also be exercised in requiring that the accounts of the diseases and treatment of the individuals attended should be accurately kept, and in subjecting those accounts to revision, as one means of security that the sick are carefully attended and correctly treated.

Upon the subject of medical relief the committee have agreed to the following resolution:

> That the administration of medical relief to the poor has been in many respects amended under the new law, but that there is still room for further improvement; that the medical districts, in some instances, seem to be inconveniently large; that they should be of such a size as to admit an easy access of the medical man to his patients; and that the remuneration should be such as to ensure proper attention and the best medicines.

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### NEW WORK ON PHYSIOLOGY. BY W. B. CARPENTER.

We are happy to perceive, by our advertising columns, that Mr. Carpenter, of whose high talents and acquirements we have more than once had occasion to speak, has in the press a new work on Physiology, on an extended scale, and on an entirely new plan. We shall be greatly surprised if this work is not found admirably calculated to fill an important blank in physiological science and literature, which has been long deplored, as well by the medical reader as the man of science. The work, we understand, is designed to embrace all the general laws which modern physiological research has attained; and to illustrate their application to every class of organized beings. In the Introduction will be comprised an account of Organized Structures in general, a description and comparison of the elementary tissues of Plants and Animals, and an account of the structure and characters of the principal natural groups of living beings. Under the head of General Physiology will be embraced the laws of vital action, and of the development of organized structures so far as these are yet ascertained. The department of Special Physiology will be devoted to the consideration of each function in detail; the evolution of its organ, from its simplest and most general type to its most complex and specialized form, will be traced through the ascending scale, first of the vegetable and then of the animal kingdom, and in the fœtal development of the higher classes; and a general view of the function will then be given, and the varieties in its performance in the different classes will be reconciled with its fundamental uniformity in all. The work will be illustrated with six copper-plates, as well as with several woodcuts; and we understand that it has been the aim of the author to render it intelligible (by explanation and illustration of technical language) to the general reader, as well as to the professional student.

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### COLLEGE OF SURGEONS.—ADJUDICATION OF THE JACKSONIAN PRIZE.

The Jacksonian prize for the year 1837 has been adjudged to Mr. Samuel Gaskell, of the Royal Infirmary, Manchester, for a "Dissertation on the Nature and Processes of Suppuration and Ulceration."

## ACCOUNT OF A SUPPLY OF FRESH VACCINE VIRUS FROM THE COW.

### BY J. B. ESTLIN, ESQ., OF BRISTOL.

THROUGH the kindness of an acquaintance residing in Gloucestershire, who remembered my wish to procure some fresh vaccine virus from the cow whenever an opportunity offered, a small quantity was sent to me on the 18th of August upon a piece of glass, said to have been taken from a cow, and also some taken from the hand of a person who had contracted sores by milking the diseased cows. I lost no time in investigating the source of this unexpected supply, and on the morning of the 21st ult. visited a farm-yard in which were about twenty-five cows, nearly all of which had been attacked with an eruption upon the teats during the four preceding weeks; the last of those thus affected had become so about a fortnight previous to my visit. Irregularly circular scabs existed upon the teats of many; in some the surfaces were raw from having been rubbed by recent milking: from none of the sores did any fluid exude, and most of them had been dressed with an ointment into the composition of which acetate of copper appeared to enter. The duration of the disease in the cows I ascertained to have been about a fortnight from the time that each had become affected. The hands of those who had been engaged in milking them presented sores in various stages; in one or two persons an eschar only remained, in others soreness still existed; in a boy of about thirteen years of age there was a large inflamed vesicle between the finger and thumb of the right hand, occupying all the space from the third joint of the finger to the second of the thumb: the skin was yellow at this part, as if a quantity of pus had been under it; such, however, was not the case, for from a small opening through the thickened and yellow cuticle the boy squeezed out a perfectly limpid fluid, with which I charged some glasses. This vesicle had made its appearance six days before, at which time he had felt ill. The others before mentioned, who had been engaged in milking (all of them females), reported that they had felt very ill before they discovered the "gathering" on their fingers; one represented herself as having been so much indisposed with headach, pain in the back and loins, and general weakness, as to be apprehensive she was going to have a serious fever: all described the sores as extremely painful, and as having occasioned enlargement and tenderness in the axillary glands. The course of the disease in them, as in the cows, was about a fortnight; and though it was known to them that the cows had something the matter with their teats, several days elapsed before the milkers suspected their malady to depend upon the disorder in the animals: never having witnessed the complaint before in the cows, they were ignorant of the nature of the existing disease. All the persons I have now spoken of had been vaccinated in infancy, one of them by Dr. Jenner. I next inspected three children who had been inoculated, by means of a needle, with lymph taken from one of those who had received the disease from the cows. One, a boy of fourteen years of age, who had gone through vaccination formerly, had four large circular mahogany-coloured scabs upon his arm, with the outer red line of an extensive areola still remaining. He was vaccinated about a fortnight before I saw him; the areola, I was informed, had appeared upon the eighth or ninth day, and he had been very unwell from the complaint. The next was a child who had been vaccinated from one of the milkers only three days before; scarcely any inflammation or prominence were observable about the punctures, and I have since learned that inoculation produced no effect. The third case was a little girl, Jane ——, about five years old, who had never had the cow-pox or small-pox. She had been vaccinated for the first time from the discharge taken from the hand of one of the milkers, during the activity of the complaint, on the 11th of August. I saw her, therefore, on the eleventh day of the disease. On one arm were three large, fine, prominent, circular vesicles, flattened in the centre, and with some areola; on the other arm was one vesicle, much larger, and less circular. I was informed that, for three or four days after she was vaccinated, it was difficult to decide if the infection had taken effect; and I have subsequently learned that the areola, which I saw on the eleventh day, continued to increase till the thirteenth day, and that the child had been "very poorly." From this little girl I took a supply of lymph which was quite limpid, and flowed very freely. . . .

On my return to Bristol I employed, as soon as was practicable, the lymph with which I was furnished.

The matter from the cow produced no effect, though tried on several children, nor did that from the boy's hand. Of those vaccinated with the lymph from Jane ———, two only out of many were infected. One of these patients had one well-formed vesicle, the other had two. In both, the disease was late in coming on; in one of them no redness appeared at the base of the vesicle till the tenth day, and the areola was not fully formed till the thirteenth day. In this case, however (Sarah Owen's), each vesicle was very perfect, rising abruptly from the arm, its upper part almost overhanging the base; its surface was much flattened, and it yielded freely limpid fluid when punctured before the areola appeared. On the thirteenth day the child's body and extremities were covered with a rash, in patches, much elevated from the skin, and she was constitutionally indisposed. On the fifteenth day the surface of the vesicle was becoming brown, and the areola, rash, and general indisposition had disappeared. From these two children many others were vaccinated, and now a second set has been inoculated from these last. In the majority of cases the vesicles have been inflamed round their base about the fourth or fifth day, and the areola has become extensive on the ninth. The areola usually continues for three or four days. In some cases it has been considerable on the eighth day. The vesicles are large, very well marked, and yield an abundant supply of clear lymph, and in every case there has been a good deal of constitutional disturbance. Some who have been vaccinated upon one arm with lymph taken on the eight day from the other arm, have exhibited in the second vaccination a small vesicle surrounded with a miniature areola, appearing and subsiding with that upon the opposite arm. *Med. Gaz.* Sept. 15, 1838.

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### ON THE MEDICAL INSTITUTIONS OF CAIRO. BY CLOT BEY.

The hospital and school of medicine formerly existing at Abou-zabel have lately been removed to Cairo. Here also are formed an establishment for pharmacy, a new botanical garden, a museum for natural history, a civil hospital, and a "maternité." The removal of the hospital and school was rendered necessary by the distance at which it was formerly situated from Cairo. A large sum of money had been spent on the former establishment. It was required to find in Cairo a building sufficiently large to contain from one thousand to fifteen hundred patients, accommodation for three hundred students, and rooms appropriate for their instruction. The great college of Khasz and Hein, situated between old Cairo and Boulac, and opposite the island of Rhoda, at a little distance from the capital, has been chosen for the above purpose. The building is surrounded by fine walks. Its form is square: it has two stories above the ground-floor. All the wings consist of a double row of wards, separated by a corridor. Each wing is divided into four wards, each ward containing fifty beds. The first floor consists of vaulted chambers, which are used as stores. In the centre of the building is a large court, planted with trees. Connected with the south wing are four large buildings, separated one from the other. The first is employed for amphitheatres, chemical laboratories, cabinets of physics and natural history; the second for dormitories and refectories; the third for pharmacy; the fourth for kitchens, baths, and waiting places. At Abou-zabel there was a very excellent botanical garden. A portion of the island of Rhoda, so notorious for its gardens, and which rival the finest in Europe, is to be devoted to botanical purposes; Ibrahim Pacha being very much attached to agriculture, and therefore anxious to encourage botany. Already, under the direction of M. Regis, the young Arabians have made rapid progress in the study of natural history, and Egypt will, not long hence, be possessed of this science to a degree not at present at all suspected. The specimens contained in the cabinet of natural history have been collected from Egypt, Syria, Candia, Ardjas, Yemen, &c.; and applications have been made in France, England, Russia, Germany, Italy, &c. for an interchange of specimens, which has been the means of enlarging the collection. A building termed the Moristan was the miserable abode of insane patients, where they were chained in stone cells. Ibrahim Pacha had these unfortunate individuals removed to a civil hospital, where they are most assiduously cared for, better fed and better lodged. The necessity of a *maternité* has been strongly felt. The negresses and Abyssinian women have hitherto learned the art of midwifery in a school at Abou-zabel. Thirteen of them have already learned to write Arabic very correctly: they have studied the art of midwifery in a treatise translated into that language, together with demonstrations on an anatomical figure conducted by an European female teacher; and a professor to whom the teaching of the art is intrusted. A small hospital for females annexed to their school has afforded them an opportunity of superintending some deliveries; together with the practice of bleeding, vaccination, and surgical dressing. They have been instructed in some simple medical subjects, and in the preparation of some medicines. A distinguished student from the Maternité of Paris, (Madame Gault,) has been attached to the establishment as "accoucheuse en chef." She has found the female students very much advanced, and very capable of advancement. Their capacity for learning astonishes, particularly where it is considered how small a degree of intelligence is usually attributed to the negro race. The females here spoken of, are generally Abyssinians, and constitute a separate race; but among the negresses who study in the school, there is as much intelligence as among those of other races who are in the habit of considering them, in this respect, as beings of an inferior order. This is especially the case among the negresses of Sennar and of Meroni. There was therefore no obstacle to the formation of a midwifery hospital at Cairo. It is placed near the civil hospital. The females of the capital and of the provinces will be admitted, instructed, fed, and clothed, at the expense of the government. They will receive rewards in the same manner as the students of the school of medicine. Orphans, and the daughters of soldiers who have died or who are in active service, will be chiefly selected as students. The capital will furnish twenty students, and each province will supply four; making upwards of a hundred. A body of educated midwives will thus be formed, who will replace the ignorant and superstitious women to whom the duties of midwifery now devolve. The following is an instance of their ignorance and superstition. A poor woman had been three days in labour. Epithems, pessaries, the most absurd and dangerous applications had been employed: amulets had been tried, when a midwife suggested the efficacious means *of making a child dance between the legs of the patient,* in order to excite the child in the womb of the mother, and effect its delivery! These ignorant midwives are the cause of much evil both to the mothers and children. They possess secret remedies for curing barrenness: they have others, far more efficacious, for producing abortion; a crime which they commit without compunction. When a female does not wish to become a mother, the destruction of the fœtus is regarded by the midwives as a natural act, for which they are neither responsible to God or to society. They therefore persist without pity in this work of destruction. The discontinuance of this class of females will follow the formation of an educated class of midwives, who will fulfil another function of public utility, by treating the secret diseases to which females are subject, and for which they are prevented by a false modesty from applying to physicians. Prejudice is still so strong on this point, that a man would rather allow his daughter or his wife to die, than derogate from principles consecrated even in Arabian treatises of medicine. Fanaticism, by excluding females from society, has deprived them of the assistance of medicine, as well as of their paradise.

The above statement shows how much has been done in Egypt, in a little time. The great merit of all this belongs first of all to the viceroy; but his son, Ibrahim Pacha, has contributed very much to its accomplishment. The minister of public instruction has shown himself, in these matters, as in all others, worthy of the office which he has been called upon to fill.

*Gazette des Hopitaux. Janvier,* 1838.

## PRUSSIAN REGULATIONS REGARDING LEECHES.

The growing scarcity of leeches in Germany has led to a large importation of Hungarian leeches into that country. The German and Hungarian leech, although belonging to the same genus, are of different species. The back of the former *Sanguisuga medicinalis*, Savigny,) is of an olive green, and has six rust-coloured longitudinal strips, generally dotted with black; and the belly is of a greenish yellow, spotted black. The back of the Hungarian leech (*Sanguisuga officinalis*, Savigny,) is green, or dark green, with six longitudinal rust-coloured strips; and the belly is olive green, and not spotted.

It was the general opinion that the Hungarian leech was inferior to the German leech in energy; but, in order to determine the point with certainty, exact experiments were instituted at the Charité hospital, from which it appeared that one German leech was nearly equal to two Hungarian. The great difference in the size of leeches also attracted the attention of the experimenters; and the Prussian government has, in consequence, published an ordinance, by which apothecaries are bound to divide their leeches into three classes:

1. *Sanguis ponderis minimi*, or leeches not exceeding thirty grains in weight.
2. *S. pond. med.*, or leeches weighing between thirty and sixty grains.
7. *S. pond. maxim.*, or leeches weighing between sixty and ninety grains.

Leeches above ninety grains in weight are not to be used in medicine, unless expressly ordered; and practitioners are required to state in their prescriptions the size and species of the leeches wanted. *Medicinisch Zeitung*. No. 2. 1838.

## OBITUARY.

### JOHN SIMS, M.D.

Died, on Thursday, the 19th July, at seven P.M., aged forty-six, Dr. Sims, of 37, Cavendish square, one of the most zealous and disinterested members of the medical profession, to which he may be said to have fallen a sacrifice. About six years ago he had a most dangerous illness, produced by the absorption of poison while dissecting, during the prosecution of researches on morbid anatomy; a study in which he was much interested. From this severe attack he narrowly escaped. The result of some of his subsequent labours was a series of valuable papers on the morbid anatomy of the brain, which were read at the Medical and Chirurgical Society, and published in a late volume of the Transactions of that Society. The attack which proved fatal was a malignant fever of a low typhoid character, which he is supposed to have caught at the St. Marylebone Infirmary, to which institution he has for many years been physician. The skill and disinterested kindness which he displayed in the execution of his duties at the infirmary, as well as to gratuitous patients in his private practice, procured for him universal respect and esteem.

## BOOKS RECEIVED FOR REVIEW.

### ENGLISH.

1. The Village Pastor's Surgical and Medical Guide: in Letters from an old Physician to a young Clergyman, his Son. By Fenwick Skrimshire, M.D., Physician to the Peterborough Infirmary.—London, 1838. 8vo. pp. 425. 8s.

2. A Manual of the Diseases of the Eye; or, a Treatise on Ophthalmology. By S. Littell, M.D. Revised and enlarged by Hugh Houston, M.R.C.S.L.—London, 1838. 12mo. pp. 307. 5s.

3. Intermarriage; or the Mode in which, and the Causes why, Beauty, Health, and Intellect result from certain Unions, and Deformity, Disease, and Insanity, from others, &c. Illustrated by Drawings of Parents and Progeny. By Alexander Walker.—London, 1838. 8vo. pp. 442. 14s.

4. Treatise on Physical Education; specially adapted to young Ladies. By A. M. Bureaud Riofrey, M.D. Second Edition, considerably enlarged; with Plates.—London, 1838. 8vo. pp. 574.

5. A Treatise on Inflammation. By James Macartney, M.D. F.R.S. F.L.S. M.R.I.A. F.S.S.L., Professor of Anatomy to the Royal Hibernian Society.—4to. pp. 214; 2 plates. 15s.

6. Medico-Chirurgical Transactions, published by the Royal Medical and Chirurgical Society of London. Second Series. Vol. III.—London, 1838. 8vo. pp. 453. 15s.

7. First Principles of Medicine. By Archibald Billing, M.D. A.M., &c. Third

Edition, considerably enlarged and improved. — London, 1838. 8vo. pp. 288. 10s. 6d.

8. Counter-Irritation, its Principles and Practice, illustrated by one hundred Cases of the most painful and important Diseases effectually cured by external Applications. By A. B. Granville, M.D. F.R.S.—London, 1838. 8vo. pp. 360. 10s. 6d.

9. A Fact in the Natural History of Children, hitherto unobserved; which explains much concerning Infantile Diseases and Mortality. By John Gardner, Surgeon. —London, 1838. 8vo. pp. 23. 1s.

10. General View of the Sources of Difficulty and Fallacy in Diagnosis, (being the Article Practice of Physic, from the 7th Edition of the Encyclopædia Britannica. By W. Thomson, M.D.—4to. pp. 22.

11. Observations on the Management of Mad-houses; illustrated by Occurrences in the West-Riding and Middlesex Asylums. By Caleb Crowther, M.D., formerly senior Physician to the West-Riding Pauper Lunatic Asylum.—London, 1838. 12mo. pp. 145. 2s. 6d.

12. A Treatise on the Nature and Treatment of Hooping-cough and its Complications, illustrated by Cases: with an Appendix, containing Hints on the Management of Children. By G. H. Roe, M.D. Oxon. Physician to the Westminster Hospital.—London, 1838. 8vo. pp. 258. 8s.

13. The Power, Wisdom, and Goodness of God, as displayed in the Animal Creation; showing the remarkable Agreement between this Department of Nature and Revelation. In a Series of Letters. By C. M. Burnett, Esq., Member of the Royal College of Surgeons.—London, 1838. 8vo. pp. 549; with Plates. 15s.

14. The Cyclopædia of Anatomy and Physiology. Part XIV. Gland—Hearing (Organ of).—August, 1838. 5s.

15. Transactions of the Medical and Physical Society of Bombay. Vol. I.—Bombay, 1838.

16. Elements of Physiology; being an Account of the Laws and Principles of the Animal Economy, especially in reference to the Constitution of Man. By T. J. Aitkin, M.D. &c. — London, 1838. 8vo. pp. 514. 9s.

17. Report of the Directors of the Dundee Lunatic Asylum, for the year ending May 31, 1838.—Dundee, 1838. 8vo. pp. 36.

18. Statistical Report on the Sickness, Mortality, and Invaliding among the Troops in the West Indies. Prepared from the Records of the Army Medical Department and War-office Returns. By Captain Tulloch.—London, 1838. Folio, pp. 143.

19. On Urinary Diseases, their Consequences and Treatment. By Robt. Willis, M.D. &c.—8vo. pp. xxxvi. and 408.

20. Physician's Report to the Managers of the Lunatic Asylum of Aberdeen, for the year ending 1st May, 1838.—Aberdeen, 1838. 8vo. pp. 14.

21. The Second and Third Fasciculi of Anatomical Drawings, selected from the Collection of Morbid Anatomy in the Army Medical Museum at Chatham.—London, 1834—1838. Atlas; 19 Plates.

22. The Structure, Diseases, and Treatment of the Teeth, considered with a View to the Abolition, in all common Cases, of the pernicious Practice of Tooth-drawing. By Wm. Wardroper, M.R.C.S. — London, 1838. 8vo. pp. 59. 3s.

23. Lectures on Lithotomy, delivered at the New York Hospital, December, 1837. By A. H. Stevens, M.D., Surgeon of the New York Hospital. —New York, 1838. 8vo. pp. 93; 3 Plates.

24. A Clinical Lecture on the Primary Treatment of Injuries, delivered at the New York Hospital, November, 1837. By A. H. Stevens, M.D. &c.—New York, 1837. 8vo. pp. 34.

25. Remarks on the Preservation of the Teeth from Infancy to Age. By D. W. Jobson, Surgeon-Dentist to the Queen.—London, 1838. 12mo. pp. 94. 1s.

26. The Science of the Cerebro-Spinal Phenomena attempted. By J. S. Waugh, M.D., Annan.—London, 1838. 12mo. pp. 172. 6s.

27. Chemistry of Organic Bodies.—Vegetables. By Thomas Thomson, M.D. F.R.S., Professor of Chemistry in the University of Glasgow, &c.—London, 1838. 8vo. pp. 1076. 24s.

28. An Experimental Essay on the Physiology of the Blood, for which a Gold Medal was awarded by the Medical Faculty of the University of Edinburgh. By Charles Maitland, M.D.—Edinburgh, 1838. 8vo. pp. 90. 2s.

29. The Medical Portrait Gallery; with Biographical Memoirs of the most celebrated Physicians, Surgeons, &c. By T. J. Pettigrew, F.R.S. &c. Parts IV. V. VI. VII., containing Portraits and Memoirs of Blundell, Caius, Morgagni, Radcliffe, Bichât, Cooper, Copland, Cooke, Mead, William Hunter, Jenner, Baron.—Price 3s. each.

30. Analytical Tables, adapted to the Purposes of General and Forensic Chemistry, &c. Translated from the French of Professor Devergie. By M. H. Lynch, M.D. &c. Two large Tables.—Newcastle-on-Tyne, 1838.

31. A Treatise on Neuralgia. By

Richard Rowland, M.D., Physician to the City Dispensary.—London, 1838. 8vo. pp. 173. 6s.

32. Observations on the Preservation of Sight, and on the Choice, Use, and Abuse of Spectacles, Reading-glasses, &c. Third Edition. By J. H. Curtis, Esq., Oculist and Aurist.—London, 1838. 12mo. pp. 74. 1s.

33. Anatomical Tables; containing concise Descriptions of the Muscles, Ligaments, Fasciæ, Blood-vessels, and Nerves. Intended for the use of Students. By T. Nunnely, Lecturer on Anatomy, &c. Leeds.—London, 1838. 12mo. pp. 240. 4s. 6d.

34. An Essay on the Relation between the Respiratory and Circulating Functions. By Charles Hooper, M.D.—Boston, 1838. 8vo. pp. 74.

35. The Boston Medical and Surgical Journal, for July and August, 1838.*

FOREIGN.

1. Ischl e Venezia Memoria del Dott. C. V. L. Brera, &c.—Venezia, 1838. 8vo. pp. 295.

2. Indagini a stabilire quale possa essere il miglior metodo di Cura pel Cholera-Morbus d'oggi giorno, dirette a' giovini medici Italiano. Memoria del Dot. Emiliani, Professore di Medicina nella R. Universita di Modena.—Bologna, 1837. 4to. pp. 100.

3. Ueber Schleim-und Eiterbildung und ihr Verhältniss zur Oberhaut. Von Dr. Henle.—Berlin, 1838. 12mo. pp. 62.

4. Statistica di Coloro che furono presi dal Cholera Asiatico in Roma nell' Anno 1837, umiliata alla Santità di nostro Signore Papa Gregorio XVI. Dalla Commissione Straordinaria di Pubblica Incolumità.—Roma, 1838. Fol. pp. 145.

5. Historisch-kritische Darstellung der Pockenseuchen des gesammten Imf-und Revaccinationswesens im Königreiche Würtemberg innerhalb der fünf Jahre, Juli, 1831, bis Juni, 1836. Von Professor Dr. Franz Heim.—Stuttgart, 1838. 8vo. pp. 651.

6. Resultat der Revaccination in dem Königl. Wurtembergischen Militär in den Jahren, 1833, 1834, und 1835. Von Professor Dr. Franz Heim.—Ludwigsburg, 1836. 8vo. pp. 100.

7. Dell' Influenza del Clima nel prevenire e curare le Malattie chroniche, &c. Di Giacomo Clark, M.D. &c. Versione dall Inglese con aggiunte del Dott. Giuseppe Girolami.—Firenze, 1837. 8vo. pp. 260.

8. Saggi Clinici riguardanti forme le piu frequente dell' umano infermare Opera empirico-induttiva del Dott. F. G. Geronimi. Vol. I.—Cremona, 1837. 8vo. pp. 160.

9. Du Varicocele, et, en particulier, de la Cure radicale de cette Affection. Par H. Landouzy.—Paris, 1838. 8vo. pp. 126.

10. De Cholera Asiatica et præsertim de Epidemia Gothoburgensi Adnotationes nonnullæ, quas conscripsit Dr. J. G. Jacobsen.—Upsaliæ, 1838. 8vo. pp. 46.

11. Haandbog i Toxikologien. Af C. Otto, M.D., Professor ved Kjobenhavns Universitet.—Kjobenhavn, 1838. 8vo. pp. 504.

12. Recherches anatomiques sur l'Emphysème pulmonaire. Par le Dr. Lombard, Medecin de l'Hôpital de Genève.—Genève, 1837. 4to. pp. 20.

13. Die Lehre von der Erkenntniss und Behandlung der Lungen- und Herzkrankheiten. Von Dr. P. J. Philipp.—Berlin, 1838. 8vo. pp. 522.

14. Die Behandlung der Hundswuth in politzeilicher, prophylaktischer und therapeutischer Hinsicht. Von Dr. J. B. Sauter.—Konstanz, 1838. 8vo. pp. 179.

15. Over de Overeenkomst en het Verschil Tusschen de Jicht en de Scrophulosis, vooral met Betrekking tot de Longtering; eene Voorlezing door A. A. Sebastian, M.D. Hoogleeraar in de Geneeskunde aan de Hoogeschool te Groningen.—Groningen, 1838.—8vo. pp. 102.

16. Tractatus de Eclampsia. Auctore E. S. Stein, M.D.—Hagæ Comitum, 1837. 8vo. pp. 149.

17. A. A. Sebastian Oratio de Animo et ingenio gentium suis de Medicina meritis evidentissimo, publice habita.—Groningæ, 1837. 4to. pp. 27.

18. Ricerche Patologiche interne alle Idropi. Del Dottore Giovanni Gandolfi.—Firenze, 1836. 8vo. pp. 295.

19. Memorie della Società Medico-chirurgica di Bologna. Vol. I.—Bologna, 1838. 8vo. pp. 560.

20. Taschenbuch zu gerichtlich-medicinischen Untersuchungen für Aertze, Wundärzte, und Justiz-Beamte. Von J. C. F. Rolffs, M.D., &c.—Köln, 1838. 12mo. pp. 212.

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# INDEX TO VOL. VI.

OF THE

# BRITISH AND FOREIGN MEDICAL REVIEW.

---

PAGE
Abortion, singular case of . 236
treatises on . 81
Abscess, penetrating the chest . 234
treatment of . . 156
Absorption, cutaneous, Dr. Madden on 332
Acetate of lead in hernia . . 537
Addison, Dr. on electricity . 72
Adhesion, Sir C. Bell on . . 156
Air into the veins, introduction of 455
experiments on . 517
in respiration, quantity used 580
Albuminous fluids, Dr. Babington on 74
Alcock, Mr. on the Spanish Legion 448
Amussat, M. on air in the veins, 455-527
Amputation, Sir C. Bell on . 166
results of . 561
of the foot . . 535
Amsterdam college and medical school 277
Anatomical Sketches, by Wormald, &c. 202
Anchylosis, new treatment of . 254
Aneurism, popliteal, case of . 67
Arteries, M. Bizot's Researches on 41
Ashwell, Dr. on uterine diseases . 71
Auscultation, artificial . 532
Babington, Dr. on albuminous fluids 74
Barlow, Dr. his address . . 570
Dr. H. on the causes of disease 201
Barton, Dr. his treatment of anchylosis 254
Baron, Dr. his Life of Dr. Jenner 477
Baths of hemlock in skin diseases 538
Beaumont, Dr. on digestion . 172
Belladonna in typhus fever . 554
Bell, Sir C. his institutes of Surgery 154
Belmas, Dr. on the cure of hernia . 341
Berres, Dr. on the nerves . 393
Bidder, Dr. his neurological researches 204
Bile, healthy and calculous, analysis of 247
Bizot, M. his researches on the heart 41
Bladder, diseases of, Mr. Coulson on 509
Blandin, M. on amputation of the foot 535
Blood-globules, on the motions of the 219
Blood, analysis of . . 248, 443
chemistry of . . 431
Bloodletting, on the abuse of . 515
Blizard, Mr. obituary of . 288
Bonnet, M. on the cure of hernia 345
Books received for review . 291, 594

PAGE
Botany, British, Dr. Macreight on 511
Bougies, new material for . 259
Bouillaud, M. on rheumatism . 352
on air in the veins 517
Brain, case of tumour of . 226
on the structure of . 393, 400
exploration of . . 144
Bright, Dr. on abdominal tumours 72
British Legion in Spain, history of 448
Association, report of the 573
Bronchitis, use of chlorine in . 533
Bronchocele, relative prevalence of 27
Burdach, M. on the nerves . 394
Burke, Mr. on fractures . 308
Burne, Dr. on hydrophobia . 268
Cancer, relative prevalence of . 20
of the uterus, Dr. Churchill on 90
Capillary vessels, anatomy of . 218
Carbonic acid, murder by . 241
Carbonate of iron in hooping cough 222
Carlsbad waters in tic douloureux 511
Carmichael, Mr. on venereal ulcers 564
Carotid artery, ligature of . 233
Carpenter, Mr. on vital phenomena 553
on Physiology 553, 590
Cataract, new mode of diagnosticating 231
M. Maunoir on . 50
Cervix uteri, inflammation of . 89
Chase, Dr. on the radical cure of hernia 341
Cheltenham, medical topography of 384
Chemistry, Mitscherlich's treatise on 505
Children, Dr. Ure on diseases of . 516
Chlorine in bronchitis . . 533
Cholera, statistics of, in Berlin . 240
on the laws of . . 576
Chomel, M. on gout and rheumatism 352
Churchill, Dr. on diseases of females 86
Clavicle, luxation of, downwards . 229
Climate of Exeter . . 383
influence of, on health . 148
Clot-Bey on Medicine in Cairo . 592
Club-foot, Sir C. Bell on . 163
cured by operation . 272
Cock, Mr. on the pneumo-gastric nerve, 60
Colchicum in rheumatic gout . 556
Colquhoun, Mr. on a case of idiocy 122
Combe, Dr. on digestion . . 172

PAGE
Conquest, Dr. on tapping the head . 265
Cooper, Mr. B. his surgical cases . 66
Copland, Dr. on abortion . 81
Copaiva, new mode of administering 557
Cormack, Dr. on air in the veins . 455
Coulson, Mr. on diseases of the bladder, 509
Counter-irritation, Dr. Granville on 498
Creosote, on the topical use of 270, 235
Cross, Dr. on delirium tremens . 329
Croup, cauterization in . 530
Crystalline lens, anatomy of . 205
Cyanosis from absence of one lung 220
Cystitis, use of nitrate of silver in 565
D'Amador, M. on pathological anatomy, 298
Davy, Dr. on the vesiculæ seminales 551
Death after wounds, not from . 540
Deckmann, Dr. obituary of . 290
Delirium tremens, works on . 330
  statistics of . 323
  description of 325
  treatment of . 328
Dendy, Mr. on cutaneous diseases 425
Denis, M. on the chemistry of the blood, 431
Desgenettes, M. obituary of . 290
Deventer, college of . . 280
Dewees, Dr. on abortion . . 81
Diagnosis, M. Piorry on . 128
Dick, Dr. on porrigo . . 425
Dieffenbach on ruptured perinæum 536
Digestion, artificial, galvanism in . 527
  experiments on . 172
Disease, moral causes of . . 201
Diseases of the Landsend and Worcester 8
Dislocation of the humerus . 231
  of the tarsus . 538
Donné, Dr. on the milk of nurses . 181
Dropsy, relative prevalence of . 17
  on elder berries in . . 221
Dropsical effusions, urea in . 249
Dugès, M. on abortion . . 81
Dunglison, Dr. on polypus of the heart 547
Ear, Mr. Webster on the . 199
Egypt, influence of its climate on health, 151
Ehrenberg, M. on the brain . 393
Electricity, animal, remarkable case of 249
  as a remedy . . 72
Elm-bark, bougies &c. made of . 239
Estlin on vaccine virus . 591
Embryotomy, remarkable case of . 236
Emmert, M. on the nerves . 394
Emphysema of the lungs . 28
Empyema, case of . . 266
Encephalitis, symptoms of . 144
Epidemic constitution, on animals 220
Epiglottis, acute inflammation of 261
Epilepsy, epidemic in schools . 531
Eruptions, relative prevalence of . 27
Evans, Mr. on copaiva . 559
Exeter, medical topography of . 382
Experiments on air in the veins . 517
Eye, on disease of . . 73
  turpentine in diseases of . 533
Farcy in man . . . 214
PAGE
Farr, Mr. on the laws of cholera . 586
Feminiscence . . . . 75-79
Fenner, Mr. on abdominal pressure 567
Fergus, Dr. obituary of . 288
Fevers, relative prevalence of . 9
Fever, typhus, Dr. Graves on . 554
  statistics of . . 555
  puerperal, Dr. Gruber on . 539
Finck, Dr. on the cure of hernia . 341
Foot, amputation of . . 535
  dislocation of . . 538
Fractures, statistics of, in America 266
  in London 310
  on the treatment of . 308
Fractures of the ribs, spontaneous 227
Franecker, college of . . 280
Frostbite, amputation from . 539
Galen, analysis of his writings . 385
Galvanism, effect of, in digestion 527
Ganglions, structure of . . 404
Gangrene, senile . . 157
  of the hand . 66
Gastric juice, experiments on . 172
  nature of . 177
Gerdy, M. on the cure of hernia . 348
Glands, sudorific, account of . 514
Glanders in man . . 113
Glaucoma, Dr. Mackenzie on . 274
Glottis, scalded . . 270
Glover, Mr. on the rete mucosum 574
Gout, on the nature and cure of . 352
Granville, Dr. on counter-irritation 498
Graves, Dr. on the pupil in fever . 554
Greenhow, Mr. on neuralgia . 573
Groningen, University of . 277
Gulliver, Mr. on suppuration . 558
Gurlt, Dr. his physiology . 515
Guy's Hospital Reports . 60
Hallmann, M. on the temporal bone 98
Hammick, Dr. his Mitscherlich . 605
Hancock, Mr. his translation of Velpeau 163
Handyside, Dr. his case of suicide 473
Harrison, Dr. obituary of . 289
Health, Sir A. Carlisle on . 166
Heart, exploration of . . 132
  researches on . . 41
  rheumatic affections of . 362
  hypertrophy of, treatment of 387
  diseases of, prevalence of . 20
  polypus of . . 547
Hernia, on acetate of lead in . 537
Hemorrhage, Sir C. Bell on . . 157
Hemlock baths in diseases of the skin 538
Hernia, improved mode of reducing 358
  on the radical cure of . 341
  Mr. Hilles on . . . 563
  cases of . . . 68-70
Hilton, Mr. on the laryngeal nerves 41
Hindoo medicine, Dr. Royle on . 566
Hoegh, Dr. on delirium tremens . 330
Holland, present state of medicine in . 275
Holst, Dr. on tumours of the brain . 286
Hooping-cough, carbonate of iron in 222

PAGE
Hunger, theory of . . . 179
Hunterian oration, Mr. Travers's . 191
Hunter, John, his anticipations . 192
Hutchinson, Mr. on tic douloureux 511
Hydrocephalus, on tapping the head in 265
Hydrophobia, cases of . . 268
Hysteria, Mr. Prichard on . 512
Idiocy, cases of alleged . 122
Immoveable apparatus in fractures 310
objections to 315
India, mortality of troops in . 151
Inflammation, Mr. Macilwain on . 122
Inflammation of the bladder, Mr. Coulson on . . . 510
Intestinal diseases, prevalence of . 26
Intestinal glands, anatomy of . 218
Intussusception, treated by inflation 274
Inversion of the uterus . 96, 236
Ipecacuanha, new preparation of . 247
Jenner, Dr. his life, by Dr. Baron 477
Journals, American and Colonial . 260
Key, Mr. his case of hernia . 70
Kidd, Professor, on the works of Galen 385
Krause, Dr. his treatise on anatomy 204
anatomical observations 218
Lallemand, M. on cystitis . 565
Landsend, medical topography of 3
Langley, Mr. on puncturing small-pox 563
Larrey, M. his treatment of fractures 312
Laryngotomy, case of . . 270
Laryngeal nerves, Mr. Hilton on . 61
Le Cann, M. on the blood . 431
Lee, Dr. on abortion . . 81
Leeches, Prussian regulations regarding . . . . 594
Legs, amputation of both . 550
Leyden, university of . . 279
Life, Mr. Macilwain on . 100
Liquor amnii, analysis of . 248
Liver, exploration of . . 140
London, medical topography of . 187
Lonsdale, Mr. on fractures . 308
Lotion, Dr. Granville's antidynous 500
Louis, M. on emphysema of the lungs 28
Lungs, diseases of, relative prevalence of 20
exploration of . . 137
of infants, experiments on 62
Lunatic Asylums, statistics of 239-240
Lusus naturæ, remarkable . 381
Lymph, analysis of . . 544
Macilwain, Mr. on medicine, &c. 98
Mackenzie, Dr. on glaucoma . 274
Macreight, Dr. his British botany 511
Madden, Dr. on cutaneous absorption 332
Malgaigne, M. on dislocations . 231
Mammalia, domestic, physiology of 513
Marsh, Dr. on the epiglottis . 261
Massalien, Dr. on the facial nerves 200
Maunoir, M. on cataract . . 50
Medical institutions in Caire . 592
Medicine and surgery one science . 98
Medical portrait gallery . 194
relief under the new poor-law 586
Medicine, on the improvement of . 196
is it a science? . 102
Hindoo, account of . 506
Mehliss, Dr. on virilescence, &c. . 75
Memoirs of the Paris Society . 28
Mouth, exploration of . 138
Menstruation . . . 92
Meyer, Dr. on epidemic epilepsy . 531
Milk of nurses, examination of . 181
Mitscherlich, Dr. his chemistry . 505
Moles . . . . 92
Morphium, its use in hydrophobia 268
Mortality, relative rate of . 6
Morgan, Mr. on ophthalmia . 74
Mucus, an analysis of . 546
Negroes, mortality of . . 150
Nerve, facial, account of . 300
Nerves, termination of . 414
wounds of . 170, 271
on the structure of . 393
functions of the eighth pair of 574
Neuralgia, on mercury in . 576
relative prevalence of . 15
Niuwenhuijs, Dr. on Dutch medicine 275
Nitrate of silver in chronic cystitis 565
Nitrogen, its influence on plants . 552
Nurses, quality of their milk explained 181
Obituary of Dr. Sims . 594
O'Bryen, Dr. on chronic cystitis 565
O'Beirne, Dr. on strangulated hernia 568
Oliver, Dr. his physiology . 195
Ophthalmia in the Belgian army . 197
Orchitis, M. Marc d'Espine on . 69
Œsophagus, stricture of . 165
Os uteri, cartilaginous, incision of 235
Otaphone, an instrument for the deaf 199
Owen, Prof. on the teeth . 578
Paraplegia . . . 261
Pathological anatomy, influence of 293
Petrequin, M. on artificial auscultation 532
Perineum, rupture of . . 536
Perforation of the stomach . 221
Pettigrew, Mr. his portrait gallery . 194
Phillips, Mr. on amputation . 561
Phlebitis, fatal case of . . 228
Phthisis pulmonalis, early signs of 534
epidemic . 227
Physiology, Dr. Oliver's first lines of 195
Pineal gland, dropsy of . 268
Piorry, M. on diagnosis . 138
Plants, effect of nitrogen on . 552
Pneumonia, case of . . 547
Pneumogastric nerve, Mr. Cook on 60
Poisoning by oxide of zinc . 221
Polypus, bronchial . . 268
uteri . . 93
Portugal, state of medicine in . 284
Porrigo, Dr. Dick on . . 425
Pregnancy, extra-uterine . 232
Pressure in venereal phagedæna . 564
Pressure after delivery . 567
Prichard, Mr. on hysteria . 512
Prolapsus uteri . . 96

PAGE
Prolapsus vaginæ . . 93
Provincial Association, meetings of 287, 569
 transactions of 1, 380
Pupil in typhus, Dr. Graves on the 654
Purkinje on artificial digestion . 527
Pus, evacuations of, in hepatitis . 351
 chemistry of . . 646
Ranula . . . . 164
Rasori, obituary of . . 289
Rayer, M. on glanders in man . 113
Reflex motions, Dr. Volkmann on 211
Reid, Dr. on the respiration . 580
 Dr. J. on the 8th pair of nerves 574
Rejuvenescence, Dr. Mehliss on 75, 80
Remak on the structure of the nerves 394
Reports of infirmaries, plan of . 381
Respiration, quantity of air used in 580
Rheumatism, on the nature and cure of 352
 chronic . 371
 muscular . . 354
 visceral . 373
 articular . . 358
 treatment of, by bloodletting 369
 relative prevalence of 14
Rheumatic gout, treatment of . 556
Ribs, excision of . . 548
Rigg, Mr. on the growth of plants 552
Royle, Dr. on Hindoo medicine . 506
Rupture of the uterus . 539
Ryan, Dr. on abortion . . 81
Saucerotte, M. on pathological anatomy, 293
Sanson, M. on cataract . 231
Salter, Mr. on hypertrophy . 387
Scrofula, relative prevalence of . 19
 Sir A. Carlisle on . 188
Semen, human, Dr. Davy on . 551
Semeiology, M. Piorry on . 128
Seutin, M. his treatment of fractures 314
Serous membranes, adhesion of . 232
Severn, Vale of, topography of . 4
Sewell, Dr. on amputation in frostbite 550
Shapter, Dr. on Exeter . . 382
Siebold, Dr. on abortion . 81
Skin, diseases of, in children . 425
 anatomy of . . 333
 hemlock baths in diseases of 538
Small-pox same origin as cow-pox 482
 on puncturing . 563
Speculum, use of . . 91
Spermatorrhœa, treatment of . 532
Spine, diseases and injuries of . 161
Spleen, exploration of . 160
Statistics, medical . . 1
Stammering, on the cure of . 253
St. Martin, his curious case . 173
Stomach, exploration of . . 139
Stone in the bladder, case of . 70
Strangulation, new mode of treating 558
Strychnine, M. Bally on . 225
Suckow, Dr. on semeiotics . 147
PAGE
Suicide, statistics of . . 241
Sulphuric acid, absorption of . 244
Suppuration, Mr. Gulliver on . 553
Surgery, Sir C. Bell's institutes of 154
Surgery, is it a science? . 102
Synovial effusions, treatment of . 224
Syphilis, endemic in Lithuania . 223
 Sir C. Bell on . 171
Tapping the head in hydrocephalus 265
Tartar emetic in synovial swellings 224
Taylor, Mr. on the lungs of infants 62
Teeth, Prof. Owen on the structure of 578
Temperature of the body . 142
Temporal bone, anatomy of . 201
Thomson, Dr. S. on climate . 148
 Dr. Th. his oration . 196
 Dr. A. on fever . 556
Thymus gland, anatomy of . 218
Tic douloureux, Sir C. Bell on . 169
 Mr. Hutchinson on 511
Tin hydrochlorate, medicinal use of 530
Topography, medical . . 1
Transactions of the Provincial Association . . . 1, 380
Travers, Mr. his Hunterian oration 191
Treviranus on the structure of the retina, 394
Tumours, abdominal, Dr. Bright on 72
Ulcers, excision of . . 230
 description of . 159
 venereal, on pressure in . 564
Ure, Dr. A. on the diseases of children 516
Urethra, structure of . . 165
Uterus, rupture of . . 539
 Dr. Ashwell on diseases of 71
Uterine diseases, relative prevalence of 26
Utrecht, university of . . 277
Vaccination, history of . 478
Vaccine fluid, examination of 247, 546
Vaccine virus, M. Estlin on . 691
Valentin on the termination of nerves 394
Varicocele, new mode of treating . 274
Veins, varicose, treatment of . 257
 introduction of air into 455, 517
 substances introduced into 577
Velpeau on air in the veins . 455
 his treatment of fractures 515
 his anatomy of regions . 193
Virilescence, Dr. Mehliss on . 75
Vital phenomena, Mr. Carpenter on 553
Volkmann, Dr. on reflex motions . 211
Vulva, affections of . . 87
Ward, Dr. on empyema . . 266
Warren, Dr. on excision of the ribs 548
Werneck, Dr. on anatomy of the eye 205
Wetzlar, Dr. on bloodletting . 518
Wigan, Mr. on rheumatic gout . 556
Wormald, Mr. his anatomical sketches 202
 on varicocele . 274
Wounds, death after, but not from 540
Wound of the tongue, fatal . 69

END OF VOL. VI.

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